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JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

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AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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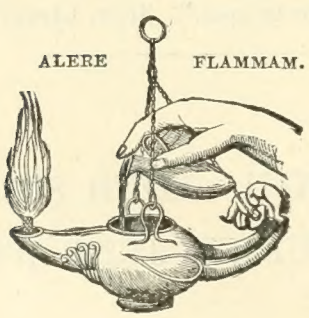
"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.



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ERRATUM IN VOL. XI.

Page 414, *for* E. H. Nichols *read* E. L. Nichols.

ERRATA IN VOL. XII.

Page 370, second line from bottom, *for* maximum velocity *read* maximum rate of change of velocity

— 371, second line from top, *for* kinetic energy *read* acceleration

ERRATA IN VOL. XIII.

Page 45, end of art. 45, for $+e_2$ read $-e_2$

— 46, art. 50, line 6, end of numerator for $\frac{3}{2n}$ read $\frac{3}{2n}$ J

— 46, art. 52, line 4, for (13) read $f(\mathrm{D})_z z + x^3 \mathrm{F}(\mathrm{D})_z z = 0$. (14)

— 46, art. 52, line 5, for (13) read (14).

— 135, last line of text, in equation (7), *for* $\frac{\Theta}{\Theta_c}$ *read* $\frac{\Theta_c}{\Theta}$

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[FIFTH SERIES.]

JANUARY 1882.

I. *On the Electrical Resistance of Vacuum.* *By* E. EDLUND, *Professor of Physics at the Swedish Royal Academy of Sciences**.

§ 1.

IT has been generally assumed that a vacuum presents a total absence of electric conductivity; and there are experiments dating from long ago which seem to favour the truth of this opinion. A multitude of researches have been made from time to time, with the view of discovering the real state of the question. This last is, indeed, of the greatest importance in several respects. The heavenly bodies are separated from one another by space in which, as far as we know, no other matter exists but the luminiferous æther. It is a fact generally known at the present time that the solar spots exert a sensible influence upon the aurora borealis and the magnetic condition of the earth, or that all these phenomena derive their origin from a common cosmic cause. Now, if the space between the celestial bodies were perfectly non-conducting, it would be difficult to conceive the possibility of a direct correlation between those phenomena; indeed it would hardly be possible that an appreciable action of electrical induction should assert itself at so great a distance. And it is quite as impossible to see, in the correlation observed, a secondary action of the insignificant modification of the thermal

* Translated from a copy, communicated by the Author, of a memoir presented to the Swedish Royal Academy of Sciences on the 23rd of April 1881, *Kongliga Svenska Vetenskaps-Akademiens Handlingar*, Bandet xix. no. 2.

radiation which could be produced by the variable number and size of the sun-spots. It is henceforth difficult to doubt that the auroræ boreales owe their production to electric currents passing through the terrestrial atmosphere; but there has sometimes been observed an auroral light originating at a height so considerable above the earth's surface that the tenuity of the air at that altitude must far exceed that which it is possible to produce by means of the best apparatus of our laboratories. This auroral light shows that the rarefied air of those high regions must necessarily be conductive, seeing that it transmits electric currents, although the experiments of our laboratories lead to the admission that air of corresponding density is a nonconductor of electricity*. The question whether electricity needs or does not need any ordinary ponderable matter in order to propagate itself from one place to another has been much discussed, and is of great importance when viewed from the standpoint of theory. If a satisfactory answer could be made to it, the question of the electrical resistance of vacuum would also, of course, be found solved. It is, unfortunately, impossible to produce an absolute vacuum, so as to solve the said question by a simple experiment;

* Warren De La Rue and H. W. Müller could not, with a battery of 11,000 elements, make the current pass through hydrogen of which the pressure was 0·000055 millimetre of mercury. According to the calculations, this pressure corresponds to the atmospheric pressure at a height of 81·47 English miles above the surface of the earth. As electricity passes with more facility through hydrogen than through air, it would therefore follow from those negative experiments that no electric discharge can take place at that altitude (Nature, [5] xxii. p. 33. 1880). On the ground of his experiments, Plücker carries to 9 geographical miles the maximum height above the earth at which auroræ boreales can be produced (Pogg. *Ann.* cxvi. p. 53, 1862). According to the calculations of Loomis, the intense aurora borealis which appeared on the 28th of August 1859 extended to 534, and that which appeared on the 2nd of September following to 495 English miles above the surface of the earth (American Journal of Science and Arts, n. s. vol. xxxii. p. 318, 1861). From observations made upon 28 auroræ boreales Professor Newton has calculated that their altitude varied between 33 and 281 English miles, and their mean height above the earth amounted to 130 miles (Nature, vol. xxii. p. 291, 1880). The calculation of observations made simultaneously at Berlin and Christiansand (Norway) upon the intensely luminous aurora borealis of Jan. 7, 1831, permitted Hansteen to estimate its height above the earth's surface at 26 geographical miles (J. Müller, *Lehrbuch der kosmischen Physik*, Brunswick, 1861). That the aurora in question, seen at Upsal with its crown in the magnetic zenith (Pogg. *Ann.* xxii.), must have presented a considerable height above the earth, appears to follow from the fact that it was simultaneously visible at Madrid (*Verzeichniss beobachteter Nordlichter*, von Fritz, Vienna, 1873). Lemström (*Öfvers. kongl. Vetensk.-Akad. Förhandl.* 1869), however, has shown that auroral rays may also appear, principally in the polar regions, at a small distance above the surface of the earth.

moreover, in experimenting, various circumstances present themselves which act in a perturbing manner upon the experiments, and of which the consequences are difficult to calculate. It will therefore be worth while to attempt to obtain, by a synoptic account and the discussion of the researches already made, some bases for the correct appreciation of this important question.

§ 2.

Walsh took a glass tube bent into an arc in the middle so that the two branches were parallel; this he filled with mercury and placed it vertically with the arc uniting the two branches above, each branch dipping in an insulated cistern filled with mercury. The mercury in the tubes then sank, forming a vacuum of several feet length between the vertical columns of mercury. When one of the cisterns was then charged with electricity, this passed with a violet light through the arc-shaped vacuum, and the mercury in the other cistern showed itself electric. On the other hand, when the mercury was carefully boiled in the tube in order to expel all traces of gas or humidity, the luminous appearance was not presented, and the mercury in the second vessel showed no trace of electricity. Thus the vacuum freed from air and moisture showed itself perfectly insulating. The experiment took place in the presence of Franklin, Smeaton, de Luis, and several other men of science*.

Morgan made an experiment for the same purpose, although he proceeded differently. He filled with mercury a glass tube, which he placed vertically in a cistern filled with the same metal. This cistern could be closed hermetically by means of a metal plate, and the air above the mercury removed by an air-pump. When this was done the mercury of the vertical tube descended, leaving a vacuum behind it. The upper extremity of the tube was covered over outside by a piece of tinfoil. When the foil received electricity from the conductor of an electrical machine, the interior of the tube became also electric by induction; and this electricity passed, with a green and violet light, through the vacuum and the column of mercury to the cistern: this took place when the tube had been filled with mercury without taking special precautions; but when the latter had been carefully boiled, the vacuum obtained appeared to be insulating†.

For the investigation of the resistance of a vacuum Paul Ermann made use of an ordinary cistern-barometer in which

* Gilbert's 'Annals,' vol. ii. p. 161. † Phil. Trans. 1785.

the mercury had been carefully boiled, and the vacuum was put into electric communication with the exterior by a platinum wire soldered to the glass in the upper extremity of the tube*. The mercury-cistern was connected with a sensitive electroscope. When the platinum wire was electrified the electroscope showed no trace of electricity. Thus the Torricellian vacuum appeared here likewise to be a nonconductor of electricity. Davy obtained the same result with a similar experiment, in which two fine, very light threads were fixed to a platinum wire soldered to the tube. When the electricity was communicated to this, the fine threads diverged and maintained the divergence without modification, while if the vacuum had been a conductor they must have put themselves into contact again†. E. Becquerel made a similar experiment, which led to the same results‡.

Masson employed for his experiments the Torricellian vacuum of a barometer in which the mercury had been carefully boiled§. Two platinum wires were soldered to the part of the tube surrounding the vacuum. Masson tried in vain to produce a discharge between these wires by means of a Ruhmkorff induction-apparatus set in action by a powerful battery; and he infers from this experiment that an absolute vacuum must be considered an electric nonconductor. Some very just remarks were made by Gaugain‖ in opposition to the results of a more recent experiment¶, in which Masson succeeded in passing the current of a powerful Ruhmkorff induction-apparatus through the Torricellian vacuum.

Some mercurial vapour always remains in the Torricellian space above the column of mercury; it is therefore not an absolute vacuum. To obtain a more complete one, Gassiot** proceeded as follows:—Through a glass tube furnished with platinum wires at its extremities, into which a certain quantity of potass had been introduced, he passed a current of pure carbonic acid until it could be assumed that all the air had been expelled; then the carbonic acid was rarefied by means of an air-pump, and the tube was closed. Connecting now the two platinum wires with the two poles of a powerful induction-apparatus, a continual transformation of the electrical luminous appearance presented itself as the carbonic acid was

* Gilbert's 'Annals,' xi. p. 164; Riess, *Die Lehre von der Reibungselectricität*. i. p. 39.

† Phil. Trans. 1822; Riess, *l. c.* p. 42.

‡ Mascart, *Traité de l'électricité statique*, ii. p. 100.

§ *Ann. de Chim. et de Physique* (3) xxxi. pp. 312–315.

‖ *Comptes Rendus*, xli. p. 152. ¶ Ibid. xxxvi. p. 255.

** Mascart, *Traité de l'électricité statique*, ii. p. 101.

more and more absorbed by the potass. At last, when all the carbonic acid had disappeared, the electricity could no longer pass through the vacuum between the platinum wires. If a galvanometer was then introduced into the circuit, it did not show the slightest deflection.

Gassiot here calls attention to a fact which appears to us to possess a certain importance:—When the gas was so strongly rarefied that the direct discharge ceased to take place, it was nevertheless possible to render the tube luminous by influence. It was sufficient for this purpose to put one of the poles of the induction-coil into communication with one of the electrodes of the tube and connect the other with a strip of tinfoil fixed to the outside of the tube, or, again, to put the two poles each into communication with a special strip of tinfoil fixed to the outside of the tube. Then the current coming from the induction-apparatus could not develop itself, and acted only by induction upon the vacuum. Although the latter was sufficiently complete to prevent the direct current from passing from the induction-apparatus, it was nevertheless possible to produce a current in it by induction.

Gaugain did not succeed in making the current of a Ruhmkorff induction-apparatus traverse a perfect Torricellian vacuum*; but at the same time he makes another observation which appears to us to be of great importance. If a strip of tinfoil be inserted between the electrodes of a less complete vacuum perpendicularly to the path of the current, the electric light shows, by its colour, that the side of the foil turned towards the positive electrode forms a negative pole, and that turned towards the negative electrode a positive pole. If one of the electrodes be then brought sufficiently near to the tinfoil, the current pierces the latter with a fine hole, through which it afterwards takes its path *exclusively*, which puts an end to the polarity of the foil. This fact proves, according to Gaugain, that the tinfoil does not act here solely as a simple conductor of electricity; for, if that were the case, the greater part of the current would pass through the tinfoil itself, which must rightly be regarded as a better conductor than the rarefied gas which fills the aperture. Gaugain draws from this the conclusion that there must exist at the surface between the metal and the gas a special resistance (we would add, or a contrary electromotive force) opposing the passage of the current through the metal.

Plücker† has also made an observation which throws a bright light upon the point which now occupies us. In some

* *Comptes Rendus*, xli. p. 152.

† Pogg. *Ann.* cv. p. 70; cf. vol. civ. p. 629.

Geissler tubes furnished with platinum electrodes which had been soldered to them, tubes from which the air had been expelled as completely as possible, the current of a Ruhmkorff induction-apparatus passed at first with a whitish light. In the continuation of the experiment, however, the discharge soon became intermittent, to cease totally at the end of a few minutes. Plücker attributes the cause of this phenomenon to the circumstance that the oxygen of the insignificant quantity of air which was still present in the tube was absorbed by the platinum electrodes, after which the nitrogen remaining was unable to transmit the discharge. If now strips of tinfoil were placed round the tube near its two extremities, but insulated from the platinum electrodes, and the poles of the induction-apparatus put in contact with the tinfoils, the tube recommenced shining with electric light, indicating that induced currents were thus produced in the vacuum by the charge and discharge of the tinfoils. If after some time the electrodes were connected with the poles of the induction-apparatus, the discharges again passed at first with a whitish light, soon became intermittent, and ended by totally ceasing. Afterwards, on the poles of the apparatus being connected with the tinfoils, the tube was again seen to shine with electric light; and these alternations could be continued *ad libitum*. When the electric light was explored with a magret, it appeared that the induced current producing it circulated with a to-and-fro movement, as there was every reason to expect. Plücker's experiments consequently show that, if the air of the tube is so rarefied that the current of the induction-apparatus cannot traverse it, it is nevertheless possible to produce, with the same apparatus, induced currents in the rarefied air —that is to say, to set in motion the electric material present there. These experiments therefore entirely confirm the above-mentioned experiments of Gassiot*.

Lastly, Hittorf† succeeded in producing, by the simultaneous employment of the air-pump and intense heating of the tube, a still more perfect vacuum than that obtained by Gassiot with the aid of pure carbonic acid. Even when the distance between the electrodes amounted to only 2 or 3 millim.,

* von Waltenhofen deduces from his researches the result that, as is natural, the form of the electrodes influences the limit of rarefaction at which the current ceases to traverse the rarefied gas. He remarks, in consequence, that if it were possible to introduce the current into the rarefied gas without the aid of electrodes, the discharge would take place even when the rarefaction was carried so far that a discharge could not be produced with the aid of the electrodes (Pogg. *Ann.* cxxvi. p. 537).

† Pogg. *Ann.* cxxxvi. p. 201.

it was impossible for the current of the most powerful Ruhmkorff induction-apparatus to leap from one of the electrodes to the other. Instead of this, the current passed from one of the platinum wires to the other along the outside of the tube. We shall, further on, return to Hittorf's very instructive investigation. The experiments we have just mentioned show that, if the air of a glass tube is rarefied beyond a certain limit, it is impossible for the strongest current to pass through it; but the experiments of Gassiot and Plücker prove moreover that, even when the limit of rarefaction is reached, it is possible by induction to excite a current in the rarefied gas. From that time there does not appear to have been any reason for maintaining that it is the lack of conductivity in the rarefied gas that prevents the transmission of the direct current of a discharge, that lack not bringing any obstacle to the development of the assuredly weaker influence-currents. The cause of the dissimilarity of the two currents can hardly be sought elsewhere than in the electrodes, which in some way hinder the passage of the current from them to the rarefied gas or *vice versâ*.

The influence-current has not such an obstacle to surmount, seeing that it is formed in the gaseous mass itself and has no need to pass, at any point whatever, from a solid conductor to the gas. This deduction is moreover in accordance with the experiment made by Gaugain with the tinfoil; and we shall have, in the sequel, repeated opportunities of confirming it. Thus the experiments which we have mentioned do not appear to constitute a satisfactory proof of the assumption that highly rarefied gas or an absolute vacuum is by itself a nonconductor of electricity.

§ 3.

We now pass to the researches respecting the electrical resistance of gases at different degrees of density.

According to the researches of M. E. Becquerel, gases under the ordinary pressure of the atmosphere commence to become conductors if they be heated to redness*. In his experiments he employed an ordinary battery to produce the current. Among the observations which he was able to make in this respect we shall cite two which are of special interest for our subject. He observed that, if the surfaces of the electrodes between which the current passed were of different sizes, the apparent resistance of the gas was greater when the current passed from the larger to the smaller than if it circulated in the opposite direc-

* *Comptes Rendus*, xxxvii. p. 22; *Traité du magnétisme et de l'électricité*, Paris, 1855.

tion. Therefore, to obtain as small a resistance as possible, as large a surface as possible must be given to the negative pole. It appeared moreover that the material composing the surfaces of the poles had an influence upon the amount of the apparent resistance. These observations have been confirmed by those which we shall cite below, and present the interesting feature that the current was produced by an ordinary battery, which was of course not sufficiently powerful to call forth the formation of a voltaic arc. Hittorf has specially confirmed the fact that gases become conductors when heated to redness*.

Gaugain† made the following observation :—If the current of a Ruhmkorff apparatus passes through the air in a glass tube in which two platinum wires are inserted as electrodes, a galvanometer intercalated in the circuit shows that the intensity of the current increases in proportion as the air is rarefied, until the rarefaction has reached a certain limit, but that the intensity begins to diminish if the rarefaction be carried beyond that limit. The degree of rarefaction at which the intensity of the current arrives at its maximum depends on a multitude of circumstances—such as the distance between the electrodes, the size of their surfaces, the width of the tube, &c. In general the degree of rarefaction necessary for the maximum of intensity of the current has increased in the same measure as the augmentation of surface of the negative electrode; when, on the contrary, the surface of the latter was small, the rarefaction of the gas did not need to be carried so far in order to bring about the maximum of intensity of the current.

Morren has found in his experiments the observation confirmed that the intensity of the current augments with the rarefaction of the gas up to a certain limit, past which it begins to diminish if the rarefaction is carried further‡. He employed for this purpose a Ruhmkorff apparatus, and a glass tube of 29 millim. internal diameter and 45 centim. length, in which the distance between the soldered aluminium electrodes amounted to 24 centim. Hydrogen commenced giving evident deflections when the rarefaction had reached a pressure of 74 millim. of mercury ; the deflections presented themselves at 39 millim. for carbonic acid, at 29 for atmospheric air, and at 23 for nitrogen and oxygen. Of course these figures possess only a relative importance and are of value only for the conditions under which the experiments took place. The maximum intensity of the current presented itself, for the first-mentioned gas, at a pressure of 1 millim., for oxygen and

* Pogg. *Ann.* Jubelband (1874), p. 434.
† *Comptes Rendus*, xli. p. 152 (1855).
‡ *Ann. de Chimie et de Physique*, (4) iv. p. 325 (1865).

atmospheric air at 0·7, for carbonic acid at 0·8, and for nitrogen at 1 millim. Employing tubes and electrodes of equal size in all his experiments, Morren had not, like Gaugain, the opportunity of observing how the maximum of intensity of the current depended on the width of the tube, as well as on the distance and the size of the electrodes.

Schultz*, in his researches, employed as source of electricity the Holtz machine instead of the Ruhmkorff apparatus. He also used Faraday's method for the determination of the electric tension necessary upon the electrodes for the production of a discharge at different densities of the enclosed gas. The following were the results of Schultz's researches:—For the passage, through rarefied air, of electricity between electrodes at a determined distance from one another, the tension necessary diminishes with the reduction of the density of the gas, until the rarefaction has reached a certain limit at which the before-mentioned tension acquires its minimum value; if the rarefaction is carried beyond that limit, the electric tension must again increase for a discharge to be possible. The density of the gas at which the tension necessary for the discharge presents its lowest value depends on the width of the tube and the condition of the electrodes. These two conclusions are in conformity with those established by Gaugain and Morren. But Schultz made another important observation, of special interest for our subject: he found that, if the pressure of the gas was greater than that at which the minimum of tension was presented, the tension necessary for the discharge increased, all other circumstances being equal, with the distance between the electrodes; when, on the contrary, the pressure of the gas was inferior, the tension was independent of the distance between the electrodes. This agrees with the observation made by De la Rive, according to which, *cæteris paribus*, the tension is proportional to the distance between the electrodes†. In the experiments made by De la Rive, however, the density of the gases examined (hydrogen and nitrogen) was higher than that at which they possessed their least resistance; he made no experiment at a lower density than that at which the resistance was at its minimum.

G. Wiedemann and Rühlmann executed in conjunction, and afterwards the former alone, some researches on the course of electricity in rarefied gases‡. The source of electricity em-

* Pogg. *Ann.* cxxxv. p. 249 (1868).

† *Comptes Rendus*, lvi. p. 669; *Arch. d. Sci. Phys. et Nat.* (2) xvii. p. 53; *Mém. de la Soc. de Phys. et d'Hist. Nat. de Genève*, xvii. p. 69 (1863).

‡ Pogg. *Ann.* cxlv. pp. 235, 364, and cxlviii. pp. 35, 252.

ployed in these two series of investigations was a Holtz machine giving a constant quantity of electricity proportional to the velocity of rotation of the disk. In the first series the rarefied gas was contained in a large receiver made of stout sheet brass, and the discharge took place between spherical electrodes of metal, of course insulated from the brass receiver. For the observation of the electric light the receiver was furnished with two apertures opposite each other and covered with glass. In the second series the electrodes were also spherical, but were placed in balls of glass joined hermetically to a glass tube the length and width of which were varied with the experiments. In all the experiments the discharge appeared discontinuous even when the gas had its maximum of rarefaction: this latter, however, was never carried far enough to surpass that of the minimum of tension necessary for the production of the discharge. The object of the investigation was to determine the electric tension necessary under different circumstances to produce an electric discharge.

With the aid of a rotating mirror and a heliometric apparatus ingeniously arranged by Wiedemann, a more particular description of which would not be in place here, it was possible to determine the time between two successive discharges. The source of electricity being constant, that time was proportional to the electric tension of the electrodes at the commencement of the discharge. The most important results of these experiments for the question which we have set ourselves to elucidate here are the following:—(1) If the flow of electricity to the electrodes is constant, the electric tension necessary at the electrodes for a discharge to take place increases with the pressure of the gas. (2) This tension depends, at equal pressure, on the nature of the gas employed; it is greater for dry air than for hydrogen; but it is independent of the chemical nature of the electrodes*. (3) The experiments made at a pressure of 25 millim. or above showed that the necessary tension increased as the distance between the nearest points of the electrodes was augmented; still the increase was less than proportionate to the augmentation of the dis-

* That the tension necessary for the discharge was independent of the chemical nature of the electrodes is in contradiction with Hittorf's experiment of which we shall speak further on. The difference of the result obtained by Wiedemann and Rühlmann is doubtless due to the electrodes in their experiments presenting relatively large surfaces and the pressure of the gas being considerable (13·8 millim. and above). It is known from other experiments that the influence of the electrodes is in the inverse ratio of the magnitude of their surfaces, and that it increases with the rarefaction of the gas.

tance*. (4) If the positive electrode was put into communication with the earth, the requisite tension was but little greater than when both insulated electrodes were connected with the electrical machine; if, on the contrary, it was the negative electrode that communicated with the earth, a still stronger tension was necessary in order to call forth the discharge.

The authors note, at the end of their first memoir, that the circumstances observed during the electric discharge in rarefied gases appeared to argue the existence of a sort of resistance to the passage at the surface between the electrode and the environing medium, obstructing the issue of the electricity from the electrode.

In the second memoir Wiedemann communicates various observations made with the view of determining the dependence of the tension necessary for the discharge on the length of the glass tube between the glass spheres furnished with electrodes. The minimum of pressure of gas employed in this series of observations was from 0·8 to 9·0 millim., and was consequently considerably lower than in the corresponding series of experiments of the first memoir. As conducted at pressures for the most part lower, the series in question give also as their result that the electric tension appears to be sufficiently independent of the length of the glass tube. For tubes of equal lengths but different widths, uniting the glass spheres, no great difference was shown in the tension of the electrodes necessary to produce the discharge. When the apparatus was filled with rarefied hydrogen, the necessary tension of discharge was less than when it was filled with rarefied air.

Wiedemann also made some experiments to ascertain the quantity of heat evolved, under the prevalence of different conditions in the tube, by the passage of the electricity. In regard to this, he found that, with an equal flow of electricity, the evolution of heat increased with the increase of the density of the gas, and that the amount of heat evolved was, *cæteris paribus*, nearly independent of the width of the tube. On a former occasion Wiedemann had found that, when the current of an induction-apparatus was passed through a Geissler tube, the evolution of heat was proportional rather to the intensity of the current than to the square of this, as would be required by Joule's law for continuous currents and solid or liquid conductors†. These two positions have been completely confirmed by Naccari and Bellati‡, who found, on employing a Ruhm-

* Pogg. *Ann.* cxlv. p. 374. † Ibid. cxlv. p. 237.

‡ *Beiblätter zu den Ann. d. Phys. u. Chem.* ii. p. 720 (1878).

korff induction-apparatus, that the quantity of heat evolved in a Geissler tube is proportional to the quantity of electricity that has passed through the tube, and is almost independent of the diameter of the tube. The quantity of heat evolved at the negative electrode was in the same way proportional to the quantity of electricity which had passed through the tube, and several times as great as at the positive electrode; at this latter the quantity of heat evolved was so very small that it was impossible to determine distinctly its proportionality to the amount of electricity.

§ 4.

From the observations above cited we can already deduce some results which will find their confirmation in the observations expounded below. When the electric current passes through rarefied gas, the gas is heated. This could not take place unless the gas opposed an obstacle to the electric motion. The current is forced to consume a certain amount of work to overcome this obstacle; and it is the work consumed that is transformed into heat. We must admit, on the ground of this observation merely, that the gas exerts a resistance to the propagation of the electricity; it is, besides, of no importance whatever for our consideration to know the nature of that resistance, whether it is or is not homogeneous with that which occurs among solid and liquid bodies. The observations abovementioned show that the quantity of heat evolved may be regarded as proportional to the intensity of the current, and not to the square of that intensity as Joule's law would require it to be. It follows directly from this, that, all other circumstances remaining equal, the amount of heat evolved must be independent of the section of the tube containing the gas.

Suppose, for example, two tubes of equal length, filled with the same gas, the section of one tube being n times as large as that of the other, and the same current s passing through both. Then $\frac{1}{n}$ of the current passes through each $\frac{1}{n}$ of section of the wide tube, whence it follows that the evolution of heat in each equal part is proportional to $\frac{s}{n}$, and consequently that the heat evolved in all the parts united is proportional to s (that is to say, of an amount equal to that in the narrow tube). Of course, moreover, *ceteris paribus* the evolution must increase with the length of the tube, and be in general proportional to that length. If r_1 denote the specific resistance in a column of gas of unit length, the heat evolved in l units of length will

be proportional to $r_1 ls$ when the current s traverses that column. Now Wiedemann's experiment has demonstrated that the quantity of heat evolved diminishes with the rarefaction of the gas, while the quantity of electricity which has passed is the same, whence it follows that the resistance r_1 of the gas diminishes as the gas is rarefied.

As was said above, according to Wiedemann and Rühlmann the electric discharge in rarefied gases presents phenomena similar to those which would be called forth by the existence of a sort of resistance to the passage of electricity impeding its escape from the electrodes. Gaugain's above-mentioned observation with the tinfoil is in favour of such an admission. If such a resistance between the tinfoil and the surrounding rarefied gas be not admitted, it is impossible to understand why the electricity passes *exclusively* through the aperture, as the metal must be regarded as a better conductor than the gas. We can cite an allied phenomenon well suited to furnish an elucidation in regard to this. When the resistance opposed by the voltaic arc to the propagation of the galvanic current is measured, that resistance is found to be composed of two parts, one of which is independent of the length of the arc, and the other proportional to that length—provided that, by an appropriate modification of the rest of the resistance in the circuit, the intensity of the current is maintained without sensible variation while the length of the arc is diminished or increased. Here, therefore, the resistance can be expressed by $a+bl$, where a and b are constants, and l denotes the length of the arc*. As a remains always invariable however short the arc be made, this resistance must have its seat upon the electrodes themselves, whether it be shared between the two or be situated exclusively upon one only. Experiment shows, it is true, that the obstacle a to the propagation of the current is not produced by a resistance in the ordinary sense of the term, but by an electromotive force which diminishes the intensity of the current quite as much as an ordinary resistance of the amount a. The propagation of the current in the voltaic arc and in a rarefied gas assumes forms so different that it is impossible to deduce *à priori*, from that which takes place in the one case, what must happen in the other. In the arc, particles are torn from one electrode and transported to the other; but a similar transference is not effected in rarefied gas. The intense evolution of heat which was found by Naccari and Bellati in the negative electrode

* *Œfvers. Kongl. Svenska Vetensk.-Akad. Förhandl.* 1867, p. 95; Pogg. *Ann.* cxxxi. p. 586.

seems nevertheless to show positively that there, in the same way as in the voltaic arc, an obstacle must exist to the propagation of the current. If we designate this obstacle by r, the total obstruction to the propagation of the current through the rarefied gas from one electrode to the other must consequently be $= r + r_1 l$; and it is necessary that the electric tension of the electrodes be able to overcome this sum in order that a discharge may be possible.

According to the observations which have been cited in the preceding pages, the tension necessary for the discharge diminishes as the gas is rarefied, until a certain limit is reached, after which the tension must be augmented for it to be possible to effect the discharge. The degree of rarefaction at which this turning-point is met with is dependent on the distance between the electrodes, the width of the tube, the amount of surface of the negative electrode, and several other circumstances. Now, as, from the preceding, r_1 diminishes constantly with the increase of rarefaction, the fact in question, demonstrated experimentally by several physicists, can only be explained by the first term, r, increasing with the rarefaction. I consequently assume that the term r increases when the density of the gas is diminished. In this manner the sum $r + r_1 l$ can reach its minimum at a certain density; and when this takes place, the minimum of electric tension is sufficient to produce the discharge. If a Ruhmkorff induction-apparatus or an ordinary battery endowed with great electromotive force be employed as the source of electricity, the intensity of the current is found to increase as the rarefaction is continued, until the above-mentioned turning-point is reached, after which the intensity begins again to diminish. If the rarefaction be carried far enough, r will have so increased that no tension will be sufficient to cause electricity to pass. Now this by no means comes, as it has been assumed to come, from excessively rarefied gas being an insulator, but from the resistance r having become so great. The fact that the degree of rarefaction at which the point in question is met with depends, as Gaugain has observed, on the distance between the electrodes is explained by the above-mentioned expression of the resistance, into which l enters as a factor.

If the gas has a pressure corresponding with the ordinary barometric pressure, r is very small in proportion to r_1, and can be neglected in comparison with the latter term. It is then seen that the tension necessary for the discharge must be proportional to the distance; that is to say, we arrive at the well-known old law of the distance of the discharge. If the pressure be diminished, r increases, while r_1 decreases; and

then it is found that the tension necessary for the discharge must increase with the distance between the electrodes, but more slowly than proportionally to that distance. If the gas is strongly rarefied, r_1 can be neglected in comparison with r; and the tension necessary for the discharge is then independent of the distance between the electrodes. All these deductions are rigorously conformable to the observations cited.

The fact that electricity cannot penetrate an almost complete vacuum depends, then, according to this exposition, on the resistance to its passage between the electrodes and the surrounding medium having risen above a certain limit*, and has by no means its reason for existence in the circumstance that the resistance, properly so called, of the gas has received, by the rarefaction, an insurmountable value; on the contrary, the resistance of the gas is diminished when its rarefaction is augmented, so that an absolute vacuum is to be regarded as a good conductor. If that is the case, one ought to be able to call forth, without employing electrodes, an electric motion in a vacuum sufficiently complete for it to be impossible to pass a current in it by means of electrodes. This is in reality what Gassiot and Plücker have already done, in their above-mentioned induction experiments with strips of tinfoil pasted to the outer surface of the tubes. Morgan's experiment, however, also mentioned above, might appear to be in contradiction with the fact cited, since it was impossible to that physicist to produce by influence a luminous phenomenon in the most perfect vacuum. But to this it may be objected that the absence of a luminous phenomenon is not a positive proof that no electric motion took place in the vacuum. Morren, in his experiments upon the passing of electricity through rarefied gases, saw that a galvanometer inserted in the circuit gave an evident deflection before it was possible to perceive any electric light in the tube, although the experiment took place in a dark room†. The reason that in Morgan's experiment no electricity passed to the mercury column was simply that there was a resistance to its passage between the mercury and the vacuum.

I have not been able to find a single certain experimental proof that absolute vacuum is a nonconductor; on the contrary, every thing indicates that vacuum is a good conductor

* I have designated as a resistance the obstacle which the current has to surmount in order to pass from the electrodes to the gas, or *vice versâ*; now by that I do not at all mean that the obstacle is due to a resistance in the ordinary meaning of the term, but rather that it is an electromotive force producing in this respect the same effect as an ordinary resistance.

† *Ann. de Chim. et de Phys.* (4) iv. p. 337.

of electricity. It seems to me that, in drawing from the known impossibility of an electric current traversing the most perfect vacuum between two electrodes the conclusion that a vacuum is absolutely nonconducting, the same mistake has been made as when, from the circumstance that the sun rises in the east and sets in the west, it was believed that one might infer that the sun in reality goes round the earth. The first of these conclusions does not appear more certain than the second.

I will now cite some observations furnishing a new support for the correctness of what has just been said.

§ 5.

Hittorf made a great number of very instructive observations on the conductivity of gases*. In the first series of his researches he employed, as electric source, a powerful Ruhmkorff induction-apparatus; the gas, contained in Geissler tubes of different shapes and sizes, was rarefied by means of a mercury air-pump. The electrodes were mostly composed of aluminium, but sometimes of other metals. The current was measured by a mirror-galvanometer. Of the results obtained we shall mention only those which have a connexion with our subject.

Two cylindrical tubes, equal in width and length, were furnished at their extremities with electrodes of platinum wire of 0·67 millim. thickness. The electrodes were in part encompassed with tubes of glass. The two positive electrodes had each an equal portion of the wire without a glass covering; but one of the negative electrodes presented a free surface 3·87 times as large as that of the other. The distance between the extremities of the electrodes was the same in both tubes. The tubes therefore presented perfect similarity, except that the negative electrode of one had a larger free surface. When they had both been filled with hydrogen and the pressure of the gas was lowered to 1·35 millim. of mercury, they were inserted side by side in the circuit of the induction-apparatus, the current of which traversed them simultaneously; but it appeared that the current of the tube whose negative electrode had the largest free surface was, on an average, 4·25 times as intense as the current of the other. The intensity of the current was therefore not far from proportional to the free surface of the negative electrode. The same result was obtained with carbonic acid and with nitrogen, as also with aluminium electrodes. Therefore, all other circumstances remaining equal,

* Pogg. *Ann.* cxxxvi. pp. 1, 197 (1868), Jubelb. p. 430 (1874); Wied. *Ann.* vii. p. 553 (1879).

when the pressure of the gas is very slight the resistance is inversely proportional to the surface of the negative electrode.

Two tubes of equal width, and with equal electrodes, between the extremities of which the distances were 12 and 146 millims. respectively, were filled with hydrogen and connected, side by side, with the poles of the induction-apparatus. The current divided itself between the two tubes; and it appeared that the two portions continually approached nearer to equality as the hydrogen was rarefied. When the pressure had fallen to 1·85 millim., the ratio between the intensities was 1·65; as soon as it had fallen to 0·55, the two intensities became perfectly equal, although the distance between the extremities of the electrodes in one of the tubes was twelve times as great as that in the other. Therefore the resistance designated by r_1 in the foregoing decreased with the pressure of the gas, and at a pressure of 0·55 millim. $r_1 l$ could be neglected comparatively with r.

On the other hand, other experiments proved that the resistance r really increases as the gas becomes more rarefied. Two equal ellipsoidal glass vessels were furnished with equal electrodes of aluminium, then filled with hydrogen at a pressure of 1·05 millim., and connected, side by side, with the poles of an induction-apparatus. The two vessels being equal, the current ought to have been shared equally between them; but it was found that, in consequence of a slight difference between the electrodes, the ratio between the respective resistances of the two vessels was 1·17. The pressure of the gas was afterwards diminished by one half in the vessel which had the less resistance; and now it was ascertained that the resistance was thereby so much increased that it became 2·34 times as great as in the vessel in which the pressure was not changed. Consequently, if the two vessels had had exactly the same resistance before the reduction of the pressure in one of them, the resistance would have been raised to $2{\cdot}34 \times 1{\cdot}17 = 2{\cdot}74$. If in both vessels the pressure was 0·5 millim. and was afterwards reduced in one to 0·25 millim., the resistance was thereby augmented in that vessel from 1 to 2·59. These experiments were modified in different ways and with different current-intensities; but the result remained nearly the same.

The gas-pressure in these last experiments being only 1·05 millim. or less, according to the foregoing $r_1 l$ can be neglected in comparison with r. These last observations consequently prove, as we have had cause to admit for other reasons beforementioned, that the resistance r increases as the gas is rarefied. Much importance attaches to Hittorf's observation that the resistance r depends on the material of which the negative

electrode is composed. When it was aluminium, r was at least 4 times less than when the same electrode was composed of silver or of platinum, 2·5 times less than when the constituent material was zinc. Aluminium being, of all metals, that which suffers least from the current, while the latter tears silver and platinum without difficulty, Hittorf is inclined to admit that here, as in the voltaic arc and the electric spark, at the surface of the electrodes exists an electromotive force acting in the opposite direction to the discharge.

In order to decide if the diminution of what we designate by $r_1 l$ proceeds continuously with the decrease of the gas-pressure, or if it ceases at a certain degree of rarefaction, Hittorf carried on the latter till the pressure was, by calculation, only 0·003 millim.; but to this extreme limit $r_1 l$ continued to diminish with the pressure. Detailed study of the questions relative to the foregoing furnished to Hittorf the result that there exists at the negative electrode an altogether peculiar cause opposing the passage of the electric current from the gas to the surface of the electrode. When the light surrounding this electrode was explored with a magnet, it was ascertained that the current here passed from the gas to the electrode.

In the subsequent researches which he has published, Hittorf substituted for the Ruhmkorff induction-apparatus a galvanic battery of from 400 to 600 cells. These were composed of carbon, amalgamated zinc, and a solution of potassium bichromate and sulphuric acid. With the Holtz machine and Ruhmkorff apparatus the current which passes through the gas is constantly discontinuous; with the battery, on the contrary, the current was always continuous as soon as the resistance of the rheostat inserted in the circuit did not exceed a certain limit. But if the resistance was above that limit, the current of the battery became discontinuous. It was therefore easy to give the current those two forms at pleasure. Hittorf calls attention to the very slight increase of temperature produced by the current in highly rarefied air. When the tube was a little wider, a thin strip of paper enclosed in it did not show a trace of carbonization when the current traversed it producing an intense light. This is in accordance with the result found by E. Wiedemann*, that a rarefied gas can be rendered luminous by the passage of the current, although the temperature of the gas may be much below 100° C. This has received further confirmation from the observations of Hasselberg†. But while the gas was so little heated by the current,

* Wied. *Ann.* vi. p. 298 (1879).

† *Mém. de l'Acad. des Sci. de St. Pétersbourg*, [7] xxvii. no. 1 (1879).

the negative electrode became intensely incandescent, though the temperature of the positive electrode was much lower. There must therefore be, at the negative electrode, some cause opposing a considerable obstacle to the propagation of the current. Moreover, on employing the battery as the source of electricity the same phenomena were verified as when the Holtz machine or the induction-apparatus was employed—namely, that the tension necessary for the passage of the current diminished when the gas was rarefied, until the rarefaction had reached a certain limit, past which that tension rapidly augmented. In a cylindrical tube of 1 centim. diameter, and with filiform electrodes, the pressure of the air or hydrogen amounted to 0·7–0·5 millim. when the tension necessary for the discharge was at its minimum. With less than 200 cells it was possible, at the rarefaction mentioned, to pass the current through those gases. The increase or the decrease of the distance between the electrodes had little or no influence upon the number of cells necessary for the passage of the current.

§ 6.

It has, then, been proved by direct observations that the resistance, properly so called, in rarefied gas, or r_1, diminishes constantly with the pressure of the gas till that pressure has fallen to an insignificant fraction of a millimetre, and that during that time r, which represents the resistance undergone by the current on passing from the gas to the solid electrode, constantly increases in value. The question may now be stated thus:—Does the known fact that the current is incapable of traversing the Torricellian vacuum depend on r_1, after constantly decreasing simultaneously with the pressure of the gas, suddenly acquiring a very high value when the last molecules of gas are removed? or is it more probable that the resistance r upon the electrodes continues to increase? Or, in other terms, which of these two magnitudes is it that increases when the highly-rarefied gas passes to absolute vacuum? Without any doubt the correct assumption is that r continues to increase with the continuation of the rarefaction, and, in doing so, receives so great an augmentation that the current cannot circulate. Only in this manner, too, is it possible to explain the fact that, without electrodes, a current can be called forth, by induction, in a vacuum deprived of gas, while it is impossible for a current to pass there between electrodes. The observations above related lead therefore, in my opinion, to the remarkable result that *vacuum is a conductor of electricity.*

This at the same time annihilates the difficulty of explaining how one heavenly body can exert an electrical action upon

another, as is the case, for instance, between the sun and the earth. The heavenly bodies are in this way in a sphere of mutual action, not only by universal gravitation and by the radiation of light and heat, but also by the electric force. If vacuum is a good conductor of electricity, an electric motion arising or disappearing on one heavenly body, will of necessity excite, by induction in the vacuum, an electric motion which can be propagated to any distance whatever in that good conductor, and in its turn call forth electrical phenomena upon another heavenly body. The inconsistency with the electrical nature of the aurora borealis which has been supposed to be found in its altitude, sometimes very great, above the surface of the earth, loses thereby all its importance. In the last place, the assumption that the presence of ordinary matter is necessary for the propagation of an electric current from one place to another to be possible will have to be abandoned. If it be agreed that it is possible to an electric motion to propagate itself with the greatest facility *in vacuo*, the notion of *conductivity* will be deprived of all physical meaning. The various material bodies produce only a greater or less *resistance* to the propagation of electricity; their effect in this respect is not *active*, but *passive*.

II. *Some Spiral Figures observable in Crystals, illustrating the Relation of their Optic Axes.* *By* Lewis Wright*.

[Plate I.]

THE true relation of the optic axes in uniaxial and biaxial crystals has always been an interesting subject. We know that if the crystals be both polarized and analyzed circularly, and we disregard any dispersion of the axes for various colours, the axis of a uniaxial and *one* of the two axes of a biaxial present ultimately similar phenomena. Here, for example, is the well known system of rings and brushes presented by a plate of calcite (fig. 1). As is well known, if we interpose between the polarizer and the crystal a quarter-wave plate, the black cross disappears, to be replaced by grey nebulous lines (fig. 2), on alternate sides of which the quadrants are dislocated; and if now we interpose a second quarter-wave plate between the crystal and the analyzer, when the latter is either crossed or parallel even these lines disappear, and we get simply a series of circular rings with no break whatever (fig. 3). Let us now take a plate of sugar cut across one of

* Communicated by the Physical Society, having been read at the Meeting on November 12, 1881.

Fig 1

Fig. 2.

Fig 3

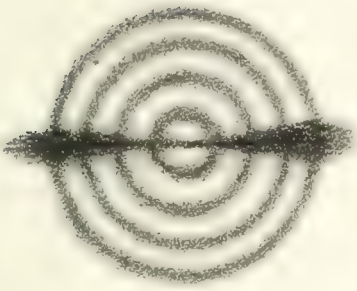


Fig 4

Fig 5

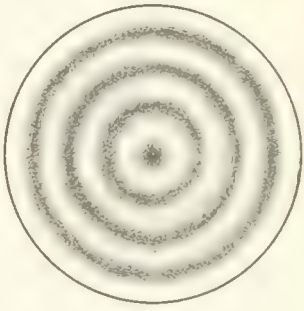


Fig 6

Fig 7.

Fig. 8.

Fig. 9.

Fig. 10.

Fig 11

Fig 12

Mintern Bros. imp.

its two optic axes. This crystal is suitable for our purpose as having scarcely any axial dispersion, so that one of its axes gives sensible circles, which many other biaxials do not. Placing it in the stage, we have a system of rings traversed by a straight brush (fig. 4), which, on interposing the first quarter-wave plate, becomes a grey line, on each side of which the semicircles are dislocated (fig. 5); but now interposing the second quarter-wave plate, we have perfect unbroken circles as before (fig. 6).

Now this might seem to imply that the optic axis of the uniaxial calcite resembled in its character that of a *single* axis of the sugar, or biaxial. It need hardly be said here, that such was not the view taken of the matter by those intellectual giants who chiefly shaped into definite form the theory of double refraction in crystals. Gradually this theory was simplified, until Fresnel finally framed the conception of three elasticities within the crystal in the direction of three rectangular axes. If all three elasticities were equal, there was no double refraction; if only two were equal, there was a single axis of no double refraction in the direction of the third; if all were unequal, there were two such optic axes. In any conceivable case the wave-surface could be calculated or geometrically projected upon this hypothesis; and it is needless to repeat how, after its author had passed away, Sir William Hamilton worked out from his conceptions the remarkable and unforeseen results of conical and cylindrical refraction which were experimentally verified by Dr. Lloyd (also removed from us during the past year). That extraordinary verification of Fresnel's theory, which makes the optic axes mere resultants of three rectangular elasticities, has always been considered to have placed it upon an impregnable basis, and seems only to have left for future experiment the possibility of perhaps some further illustration, which is the sole object of this paper.

For observe that, according to this theory, the optic axis of our calcite would *not* correspond in character with a single axis of the sugar or any other biaxial, but must be regarded as simply a limiting case in which *both* such axes coincide. This is well illustrated by the celebrated experiments of Professor Mitscherlich in gradually applying heat to crystals, especially to a crystal of selenite, and thereby altering by the unequal expansion their respective elasticities.

Of the two axes gradually approaching till they unite into one as the elasticities are gradually equalized, there could be no clearer proof than this old experiment. But it seemed worth while to seek further illustration of one particular point, viz. that the axis of the uniaxial crystal did actually retain or

embrace within itself, in some visible form, optical characteristics of the two axes thus brought, temporarily or permanently, into coincidence. This object seemed most likely to be obtained by the aid of quartz, or some other substance possessing similar properties of rotary polarization. Such substances having, apart from their ordinary doubly-refractive effects, two different *axial* velocities or waves capable of being brought into interference, and the two axes of a biaxial being according to hypothesis dissimilar, one being principal and the other secondary to it, it seemed probable that by proper means the two axes might be made to exert some kind of differential or selective action upon the two sets of waves passing through the rotary substance. I was confirmed in this expectation by the curious double spiral, first noticed by Sir George Airy, as displayed by quartz itself when subjected to circularly polarized light, the cause of which appeared to me to be connected with this very matter, as we shall presently see to be the case. After observing with more care than usual, therefore, the effects of quartz in combination with other crystals in various ways, most of which have been described by various observers, I finally adopted the following arrangement :—We introduce first, next to the polarizer, a quarter-wave plate, then (in the convergent rays) a plate of calcite, and next to this a plate of quartz 5 to $7\frac{1}{2}$ millim. thick. The result of this arrangement is the system of double spirals, mutually enwrapping each other, now on the screen (fig. 7). This figure only changes in colour, or moves to or from the centre, as the analyzer is rotated—though there are of course only certain complementary positions of the polarizer, related to that of the quarter-wave plate, which produce them. The point to be here observed is the *double* character of the spiral in this uniaxial crystal.

This figure, however, so closely resembled in all but the number of its convolutions the one exhibited by quartz alone, as described by Sir George Airy in the Cambridge Transactions for 1831, that it might possibly be due to the quartz itself in the convergent light; it was necessary to see if there were any differential results with other crystals, and, finally, to see if they remained when any such possible cause for them was removed. A single axis of sugar was therefore next placed in the crystal-stage in place of the calcite; and the result is again before you (fig. 8). Observe that, with this single axis of a biaxial, we no longer have the double spiral, but a single one, corresponding exactly to the supposed relation of which we are in search, and also showing that the figures are not proper to the quartz as such, but to some selec-

tive action of the axes of the other crystals upon the two axial waves of the quartz and their interferences. A single axis of iron sulphate, and this crystal, cut across a single axis of a topaz, gives similar single spirals.

The single axis being thus tested, we place in the stage a biaxial cut across both axes—in this case nitre (fig. 9). The supposed relation still holds good: each axis now has its own distinct spiral, and the two mutually enwrap one another as in the calcite.

The same is true of crystals whose axes include much wider angles; but to show their spirals we must alter our arrangements. Extra convergent lenses are added in a moment; but if we placed a quartz plate in the strongly convergent light we have to employ to bring both axes of such crystals upon the screen togther, the rings and spirals proper to the quartz itself, which have not appeared in the very moderate convergence so far used, would now appear so strongly as to overpower and distort those due to the crystals under examination. We also want to ascertain beyond doubt that the effects are not due to any *convergence* of the rays traversing the quartz, but solely to selective action upon the right-handed and left-handed waves traversing it axially. We therefore reverse the combination, placing a large plate of quartz about $7\frac{1}{2}$ millim. thick next to the polarizing Nicol, in the parallel rays, and removing the quarter-wave plate to a position between the crystal to be examined and the analyzer. Of course, as it is now the analyzer which is related in position to the quarter-wave plate, the spirals only appear in complementary positions; while, on the other hand, when the analyzer is in those positions the polarizer may be completely rotated. Of course, also, we might have adopted this arrangement all along; but I have given the experiments as they were made, in order to show how each successive question was determined.

A multiplicity of crystals would be useless: three of various angles will show the uniformity and gradation of the phenomena. Our former crystal of topaz cut across one axis, being thin enough also for the more convergent arrangement, will show that the single spiral appears precisely as before, the convolutions being simply closer in this strongly convergent light. Next we will take again the small angle of another nitre crystal, cut thin enough to show conspicuous figures in the convergent apparatus (fig. 10). Observe that we can now barely distinguish its two spirals, by their oval contour, from those just now presented by the calcite: they are a little *drawn out*, as it were, precisely as we should expect; and that is all; otherwise the visible elements are manifestly the same

in both. Arragonite (fig. 11), with an angle of $18\frac{1}{2}$ degrees, shows a spiral of several turns round each axis; but still they finally enwrap each other: and now mica, with an angle in this specimen of 60° or 70° (fig. 12) gives the same phenomena. With the wider separation the spiral round each axis has room to show separately more of the character of the single axis of the sugar; but the two always preserve the same relation, and only crystals which, owing to very powerful dispersion of their axes, fail to show perfect lemniscates in the ordinary way, fail for the same reason to show these figures complete. Here, for instance, is a plate of borax, whose axial dispersion is considerable and peculiar; but as this still leaves the ordinary lemniscate curves tolerably unbroken, we can trace the spirals without difficulty.

That these figures are solely due to differential action upon the interferences of the quartz rotational colours, we shall demonstrate absolutely in a few minutes; meantime we can almost prove it in two ways. First, though all the arrangements remain complete, the spirals disappear or lose their character in monochromatic light; and, secondly, substituting a quartz of opposite rotation, the direction of the spirals is, as you see, reversed.

And now we will project the beautiful experiment of Prof. Mitscherlich, afterwards applying to it this additional method of analysis. There are the two axes arranged vertically; as we apply heat they gradually unite. The crystal is now uniaxial. And now the axes open out again, but horizontally. It is a beautiful demonstration, which never loses its fascination for the student. We now add our arrangements for the spirals. There they are, arranged perpendicularly on the screen. They approach as the crystal is heated, till now we have them as in the calcite. Now they open out again in a horizontal direction, like those of a plate of nitre turned round 90° in its own plane. Observe that all through we have the *double* spiral. We can only get a single one by taking a single axis; while the axis of a uniaxial always preserves what we may call its "twin" character. Thus we have the ocular illustration sought at the commencement, of the precise relation predicated by Fresnel's theory between the axes of uniaxial and biaxial crystals, and that the former class do contain, within their single axis, elements (capable of being made optically visible) of both the axes in the latter class.

We have here also objectively demonstrated the reason of the double spiral, first observed by Mr. Airy, in quartz itself. We see that the quartz, considered as an ordinary uniaxial crystal, owing to its peculiar effects upon plane-polarized light

passing through it axially, is able to *show its own spirals*, which of course are double. They are not seen at all in parallel light; and, on the other hand, if we increase the rings by convergence, the spirals become more definite. Here, for instance, is a rather thin quartz (one of a pair generally used to show Airy's spirals): in this strongly-convergent circularly polarized light, it shows spirals as well defined as did our calcite. A crucial test of this view readily suggests itself. If it be well founded, obviously we can combine the two properties of our quartz artificially, as it were, since many fluids possess the same power of dividing into two opposite circular waves, differently retarded, a plane-polarized ray. If therefore we take a column of such fluid of sufficient length, and any ordinary uniaxial crystal, the one will represent the peculiar axial properties, and the other the ordinary doubly-refractive properties of the quartz; and the two ought to give similar double spirals. In fact an adequate column of fluid ought to replace the quartz successfully in all the foregoing experiments. Our last step, therefore, is to prove that this is the case. I have here a tube of oil of lemons 200 millimetres (8 inches) in length, which we introduce into the parallel-plane-polarized beam in place of the quartz, whose axial properties it now represents. In the crystal-stage we adjust the calcite, which in all except being a negative crystal (and a positive one would be just the same) represents the ordinary doubly-refractive properties. And now introducing the quarter-wave plate between crystal and analyzer, we have the spirals as given by the quartz in all respects. The same effects are produced by other crystals, any slight inferiority being due to the slightly yellowish tint of the fluid, which so far approximates to homogeneous light. Spirit of turpentine is free from this defect; but we could hardly project through a tube of sufficient length.

These phenomena hold good through all the ordinary analogies with, or substitutes for, natural crystals. This round disk of chilled glass, placed in parallel light, which behaves in all other respects like a crystal in convergent light, also gives double spirals like those of the calcite. I have here also an artificial uniaxial crystal formed of crossed mica-films, after Norremberg, and an artificial quartz of mica-films, after Reusch, for both of which I am indebted to my kind friend Mr. Fox, who made them with his own hands. The first gives the calcite spirals with a quarter-wave plate and quartz; the Reusch preparation gives the quartz spirals with the quarter-wave plate alone.

These experiments of course add nothing to the theory of

the matter, and make no such pretension; their whole interest lies in the visible, ocular demonstration they afford of the truth of conclusions long ago worked out by the brilliant mathematical genius of Fresnel. But to the best of my belief they are new; and if it should prove that some other student has been before me, I hope the beauty of the phenomena may excuse my bringing them before you.

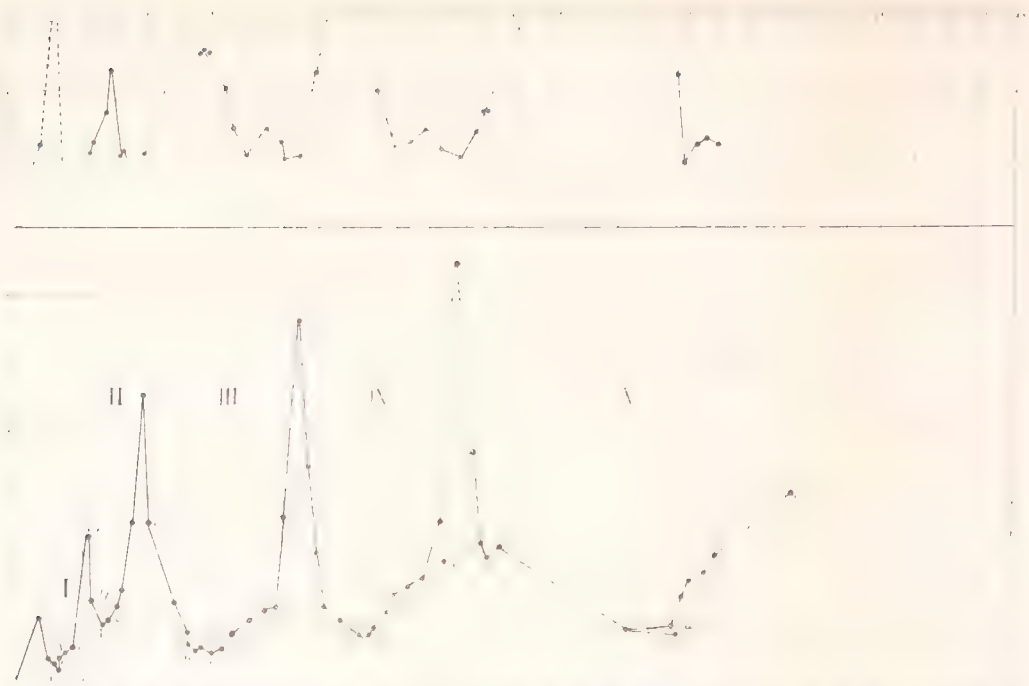
III. *On the Connexion between the Atomic Weight and the Chemical and Physical Properties of Elements.* *By* Thomas Bayley*.

[Plate II.]

When we arrange the elements in the order of the atomic weights, hydrogen of course comes first. After this there is a considerable interval, corresponding to an increase of six in the atomic weight, before the next element, which is lithium. After lithium the elements follow in this order,—beryllium, boron, carbon, nitrogen, oxygen, fluorine—the rise in atomic weight between each being on an average about two. The next element is sodium, with the atomic weight 23; and after sodium come magnesium, aluminium, silicon, phosphorus, sulphur, chlorine—the average rise in atomic weight being again about two. If we stay to consider the properties of the elements we have so placed in sequence, our attention is soon arrested by some curious circumstances. Excepting hydrogen, separated as it is by so great an interval from all other elements, lithium is the first. Lithium is a metal of the alkalies, which is only another way of saying that it has properties exceedingly like those of the well-known metals potassium and sodium. It is metallic, strongly electropositive, and chemically active, and of low specific gravity; it forms an oxide and salts which are soluble in water, and formed by the union of the metal with the smallest proportions of oxygen, chlorine, or other negative radical, simple or compound, that are found to be capable of reacting chemically. The next element (beryllium), although its atomic weight is greater than that of lithium only by two, has very different properties. It is metallic, electropositive, and chemically active; but its oxide is insoluble in water, and the metal unites with two atoms of chlorine or bromine to form its halogen compounds instead of with one. The succeeding elements have no longer the metallic character, which, however, has not quite disappeared in the case of boron, the next in succession. Carbon

* Communicated by the Author.

Cd


II
III
IV
V

is non-metallic, very stable under ordinary conditions, and its atom-fixing power is four. Oxygen is gaseous, chemically active, but electronegative, and its atomicity is two. Fluorine is in all probability gaseous; it is chemically active, so much so that it has never been isolated, powerfully electronegative; and its atomicity is one. At this point the character of the elements suffers an abrupt change; and the next element (sodium) is a metal of the alkalies, and almost exactly repeats the properties of lithium. After sodium, as after lithium, come six elements, at small intervals of atomic weight; and they repeat the changes and transitions that take place in the elements succeeding lithium. There is, first, magnesium, a metal of the alkaline earths; and then an earth-metal (aluminium), triad like boron. Next comes silicon, analogous in so many respects to carbon, being neither strongly electropositive nor strongly electronegative, and having also an atomicity of four. Then comes sulphur, electronegative, but less so than oxygen, but with analogous properties in many respects. Chlorine (gaseous, electronegative, and a monad) is like fluorine repeated in a modified form; and then again comes an abrupt change, and an alkali metal (potassium) succeeds. Thus we recognize a periodicity in atomic functions corresponding to intervals of seven elements, and to increase of sixteen in atomic weight.

Potassium and the six succeeding elements form another period, but repeat less distinctly the typical characters of the first periods. There is the same transition from alkali metal to alkaline-earth metal (calcium), from low to high atomicity; and from highly positive elements to the elements chromium and manganese, which, although possessed of basylous properties, have also well-marked chlorous characteristics. But it is evident that potassium and the six elements in succession fail to repeat the full periodic change from alkali metal to alkali metal; there is not the complete transition from positive to negative, and again to positive; nor from low atomicity to high, and again to low. The three elements succeeding to manganese form with it and with copper, which they serve to link together, the iron group, which exhibits within itself so many interesting relations of resemblance and difference. Copper has long been held to have strong affinities with sodium; and it is similar, among other things, in being the first of a series of seven exhibiting the periodic change. For although copper is not a monad, like sodium, the atomicity has fallen rapidly from manganese; and although it is not intensely active, like sodium, it is metallic and electropositive, and has no symptoms of chlorosity. Zinc, the next element,

has analogies with magnesium, and the newly-discovered gallium with aluminium; after gallium the metallic tendency grows weaker and the atomicity higher, in arsenic; and the negative monad type culminates once more in bromine. Again there is a break, followed by a metal of the alkalies—rubidium. We thus recognize the completion of the full period; but it is double, and consists (if we omit the intermediate elements iron, cobalt, and nickel) of two series of seven. The increment from lithium to sodium and from sodium to potassium was sixteen; the increment from potassium to rubidium is approximately three times sixteen—viz. 46·4. From rubidium to the next alkali metal (cæsium) the increment is 47·5—almost the same as 46·4; and the intermediate elements form a cycle parallel in a remarkable manner with the cycle from potassium to rubidium. Each of these twin cycles is double, consisting of two series of seven united by an intermediate group, consisting in the second case of palladium, rhodium, and ruthenium. The element silver, at the head of the second series of the fourth cycle, has long been classified with copper, and cadmium with zinc; and the resemblances between antimony and arsenic, between tellurium and selenium, and between bromine and iodine, are among the best-known facts in chemistry.

There is no alkali metal known of higher atomic weight than cæsium; but there is strong evidence that, just as the third and fourth cycles succeed the first and second, and correspond to increment of atomic weight three times as great, so the third and fourth cycles are succeeded at any rate by a fifth, with increment of atomic weight again three times as great. The fifth cycle is incomplete, as indeed are the third and fourth in a few members; but it seems to consist of four series of seven, united two to two by the metals osmium, iridium, and platinum, which closely resemble palladium, rhodium, and ruthenium.

It will be seen that, while the cyclic change retains its characteristics unimpaired from the lowest to the highest known atomic weight, the series, which in its early form is cyclic, loses its prominent features as the atomic weight progresses. But besides the natural classification into cycles and series, the elements are capable of another mode of grouping. Certain elements have long been seen to have strong points of resemblance and analogy, and have therefore been grouped into natural families. The alkali metals, the metals of the alkaline earths, and the halogens, are the best examples of family groups. A moment's consideration of these typical examples will show that families consist of elements which have corre-

sponding positions in the ascending series: thus the alkali metals are the first members of certain series, the alkaline-earth metals are the second, and the halogens the sixth members.

The systematic grouping into families, in accordance with this conception, is shown in the table (Plate II. fig. 1), and will be seen to be similar to, but not identical with, Mendelejeff's classification.

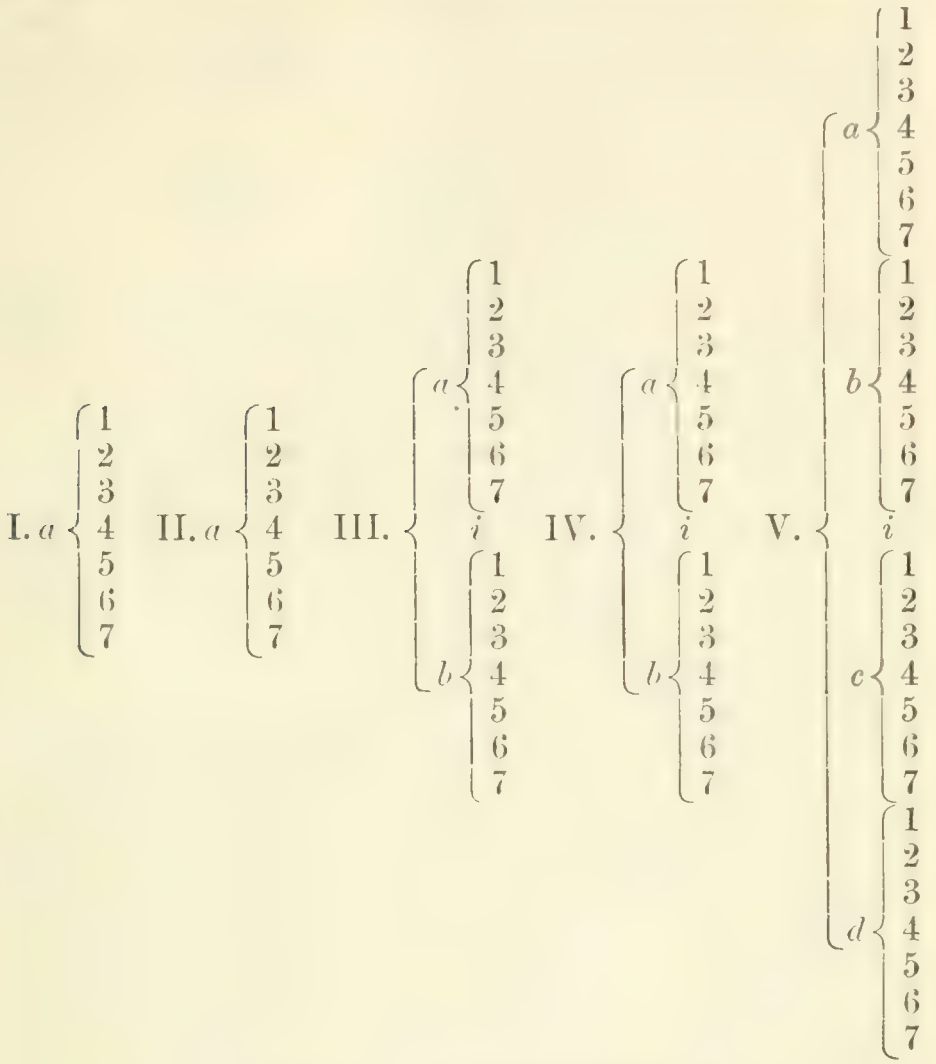
Family relationships between elements may be of two orders of intimacy. The halogen group, the group of the alkalies and of the alkaline earths, are among the best examples of complete family resemblance; but there are other groups of elements usually arranged in families which have a certain number of points of resemblance, and yet differ in many important respects. For instance, there is the group consisting of hydrogen, lithium, sodium, copper, silver, and gold; the group comprising beryllium, magnesium, zinc, cadmium, and mercury; the group carbon, silicon, tin, and lead; and the group nitrogen, phosphorus, arsenic, antimony, and bismuth. It is evident that in these cases there is less complete resemblance between the various members of the family, than exists in the case of the halogens and the other aforesaid perfect families. A little consideration will show that the resemblances between some of the members of these imperfect subfamilies, as they may be termed, are often merely structural resemblances of the various compounds formed. For example, the lower chlorides of the subfamily sodium, copper, silver, and gold have little more in common than the fact that the metal in each case is monad, and united to one equivalent of chlorine; while the chlorides of the family of elements lithium, sodium, potassium, rubidium, cæsium, have this feature in common, as well as many others which are not common to all the members of the subfamily—as, for example, solubility in water, and stability under great variety of conditions. In short, we may say that the members of a natural family of elements resemble one another in most physical and chemical characteristics; while the members of a subfamily chiefly resemble the members of the family in atomicity, which accounts for the similar structure of their compounds. But the atomicity of an element seems to depend almost entirely upon position in the primary septenary series; and, upon inquiry, we find that in all cases families and their collateral subfamilies are alike in this respect. It may be thought that too much stress should not be laid upon atomicity, a conception coming of late to be thought less rigidly applicable by chemists. But we here employ the notion of atomicity under

circumstances that admit of comparison under equal conditions. Thus we do not compare the highest atomicity of an element with the highest atomicity of another element, or their atomicities in different compounds and under widely different conditions; but we compare the atomicity of the two elements in those cases where they are united to the same elements—that is, if they are of similar character. And we generally compare them in those compounds in which the two constituents are of oppositive properties (positive and negative) and have most completely balanced each other's affinities. And if we take this last consideration always into account, it sometimes follows that the subfamilies are found to have, besides their resemblances, important structural differences, in which case the family relationship may be considered almost to disappear. The sodium, copper, and silver group, in this way considered, betray, besides the structural resemblances of their lower (proto)chlorides, oxides, &c., more important differences in their *normal* (that is, most stable and evenly balanced) compounds. It may be said that, as the atomic weight increases and the cycles become of greater magnitude, natural families differentiate, splitting first into two and then into four branches, the divergence in properties each time becoming greater and the resemblances less distinct. But in each family there is one branch which preserves unimpaired the original main characteristics of the family.

We may now consider what are the causes of this last phenomenon, this maintenance of the original conditions by one branch of a family; but before doing so, we may indicate some circumstances that are very probably connected with the smaller differences between the members of a family or subfamily of elements. We refer to the proportional position of the elements in the series, or, in other words, to the intervals of atomic weight at which the elements of each series occur. Thus, if we represent increment of atomic weight in each series by a line, we shall find that the lines representing the various series are not proportionally divided, analogous elements occurring at unequal distances from the commencement of the line, and the spaces between pairs of analogous elements being disproportionate. These circumstances, it is conceivable, may be connected with irregularities in the properties of the most closely allied elements.

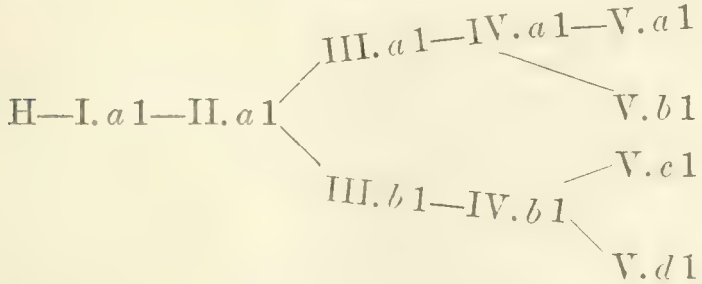
Passing on to consider the conditions that are associated with and, it may be, cause the resemblances between the members of the main branch of a family, we notice that they are all similarly situated in the cycle. We may, for convenience, distinguish the cycles, the series, and their individual

members by letters and numbers in some such way as the following:—



(*i* indicates the intermediate groups.)

According to this system, the main family of the alkaline group is I. *a* 1, II. *a* 1, III. *a* 1, IV. *a* 1, V. *a* 1; and with this are connected the various members of the group as follows:—



By this method each element, whether known or unknown,

is represented by a symbol expressing its position as regards other elements and its relations to them, whether axial as among the members of a family, or lateral as between adjacent elements in a series or cycle. Up to the present we have recognized the following circumstances to be connected with the properties of an element:—

(*a*) Position of sequence in the series,
(*b*) Proportionate position in the series,
(*c*) Position in the cycle;

and we have seen that elements having the closest family resemblances are most alike in respect of the first and last of these, and most probably in all.

Accompanying the periodic variations in the chemical properties of the elements by means of which they have been so far systematically classified, are variations in their physical properties synonymous with the chemical variations, and so closely coordinated therewith as almost irresistibly to urge the conviction that the two sets of phenomena (the physical and the chemical) are intrinsically due to the same ulterior causes, or stand in the sequence of cause and effect.

The diagram, Pl. II. fig. 2, which is in some respects the same as those published by L. Meyer in *Die moderne Theorien der Chemie*, has been constructed for me by my friend Mr. Worrall: along the horizontal line as ordinate the elements are set out, according to scale, in the order of atomic weights; and the various curves are formed by joining points in the abscissæ corresponding to each element. The main curve on the diagram thus corresponds to the atomic volumes of the elements, obtained by dividing the atomic weight by the density when known. In cases where the density is not known the element has been simply passed by, a straight line being drawn from the preceding to the succeeding element, or from the nearest preceding to the nearest succeeding element of known atomic volume. The effect of this is somewhat to obscure the resemblance in form between the portions of the curve corresponding to the cycles; but it has the advantage of making the diagram entirely a record of known facts. The line of atomic volume is well worthy of attentive study, which will reveal better than any array of words the singular resemblances and points almost of identity in atomic volume between elements resembling each other quite as closely in chemical properties. It will be seen how the alkali metals in every case have the highest atomic volume, and how the metals with the lowest atomic volumes (those occupying the troughs of the curves) are metals with comparatively small affinities for

oxygen, including the iron-copper group, the platinoid groups, and all the noble and semi-noble metals. Thus the easily oxidizable metals are those which have their atoms exposed at large intervals; the semi-noble metals those which have atoms of medium weight at small intervals; and the noble metals those which have heavy atoms at small intervals. The last may be compared to a Macedonian phalanx of large men with shields interlocked; the semi-noble metals to a phalanx of smaller men; and the alkali and alkaline-earth metals to companies of small men in skirmishing order. The melting-points of the elements are no less closely associated with the periodic variations of chemical properties than are the atomic volumes. A glance at the line of melting-points (at the top of the figure) shows the closeness of the connexion. The alkali metals have in every case low melting-points; the alkaline-earth metals melt at considerably higher temperatures; and the melting-point rises still higher in the metals of the earths, until it culminates in those metals which occupy the intermediate position in the cycle and have low atomic volumes. High melting-point, however, is not always associated with low atomic volume; and it is curious how, in the second, third, fourth, and fifth cycles, the curve of melting-points, after attaining its maximum in silicon in the iron group and in the two groups of the platinoid metals, suddenly falls, until the melting-point again becomes very low, the minimum being within the region of moderately low volume (about 15). This depression is succeeded by a moderate rise; and then the melting-point falls to the alkali-metal standard.

The low melting-point of gallium has been considered to be one of the facts which could not have been anticipated by the light of Mendelejeff's conception; and the Russian chemist, who apparently occupied himself more systematically with chemical than with physical considerations, does not seem to have assigned a low melting-point to ekaluminium, although he has subsequently somewhat empirically attempted to assimilate the fact into his system. But gallium, in having a low melting-point accords perfectly with its atomic weight and density and properties in general; it occupies, in fact, the minimum that immediately succeeds the maximum of melting-point, and is analogous to (nitrogen) phosphorus, indium, and mercury in this respect.

The magnetic properties of the elements have been shown by Carnelley to follow the periodic variations (Chem. News, vol. xl. no. 1038); and he has stated the law in the following manner:—"Those elements belonging to the even series of Mendelejeff's classification are always paramagnetic, whereas

the elements belonging to the odd series are always diamagnetic." This supposition is evidently true in the first four cycles, as far as the present state of knowledge enables us to verify it. Thus the first cycle is paramagnetic, and the second diamagnetic. The first half of the third cycle is paramagnetic and the second half diamagnetic; and this is the situation in the fourth cycle also. But if the fith cycle contains four periods of seven elements, excluding platinum, iridium, and osmium, and we know the first four cycles are strictly septenary, then the series containing gold, mercury, thallium, lead, and bismuth is an even series; and yet these are diamagnetic elements. This and other considerations lead to the modification of the law expressing the magnetic relations. The first cycle (a simple cycle of seven elements) is paramagnetic; the second cycle, also simple, is diamagnetic; after this, in the complex cycles, the first half is paramagnetic and the second diamagnetic. The fifth cycle is incompletely known at present; but if subsequent research shows the first half to be paramagnetic and the second half diamagnetic, it will justify the system of semicycles at the expense of the system of odd and even elements of Mendelejeff.

The property possessed by certain metals of giving coloured solutions when associated as bases with colourless acids is strictly periodic; and the law may be stated as follows:—The metals in the first and second cycles form no coloured solutions; in succeeding cycles the metals occupying the position of lowest atomic volume and the elements immediately succeeding them form coloured solutions. Thus in the third cycle the metals from titanium to copper, in the fourth cycle the metals from niobium to palladium, and in the fifth cycle the platinum group gold and tungsten have this property. It is interesting to observe how the power of forming coloured solutions ceases abruptly in the same region of each complex cycle—in the third cycle after copper, in the fourth after palladium, and in the fifth after gold. To be consistent, silver should form coloured solutions; but its solutions are not coloured, and it is not itself a coloured metal. Possibly its solutions are colourless because they correspond to an oxide of the form R'_2O, just as the salts of copper corresponding to Cu_2O are generally, but not always, colourless. If this is so, salts of silver corresponding to AgO, whenever they are discovered, may have coloured solutions.

There is one other circumstance with regard to the position of lowest atomic volume worthy of notice; it is the following:—The increments of atomic weight which, starting from hydrogen, successively give the point where the atomic volume is a minimum, are members of the geometric series

$$a,\quad a\times b,\quad a\times b^2,\quad a\times b^3,\quad a\times b^4,$$

where $a=10$ and $b=1{\cdot}67$,

$$=\frac{10}{6}.$$

The increments are

10, 16·5, 27·9, 46·6, 77·8;

and the corresponding atomic weights,

11, 27·5, 55·4, 102·0, 179·8.

Enough has now been said to indicate the close connexion between the physical properties of elements and the periodic variation; and it has been shown that in the majority of instances the density, the melting-point, (the expansion for heat,) the magnetism, and the behaviour of the solution to transmitted light have definite relations to the atomic weight. This being so, and it being well known that certain other physical properties of substances closely associated with atomic weight (for instance, vapour-density, isomorphism, and specific heat) are extensively used to decide which of the powers of the equivalent of an element is the weight of its atom, it may be worth while to ask whether the general physical properties of an element, and not only certain isolated ones, should not be called in to decide the all-important question of atomic weight. A crucial test of the value of this method is afforded by uranium. This element has the equivalent 60; and its atomic weight was, until lately, taken as 120. But Mendelejeff, for reasons which he states fully, has assigned to it the atomic weight 240. Our knowledge of uranium and its compounds is largely due to Peligot; and he and others have established the following facts regarding it:—

It has high density, 18·4.

It melts only at the heat of a wind-furnace.

It forms strongly coloured solutions, and, according to Verdet, it is magnetic.

If the atomic weight of uranium is 120, it comes where there is no place for it, between tin and antimony; for its volume is 6·5, while the volume of tin is 16·1 and that of antimony 18·2; it has high melting-point, while they melt at comparatively low temperatures; and it forms coloured solutions, while their solutions are colourless. In the same way, if the atomic weight of uranium is 240, the volume is 13 instead of about 30; and it occupies the region of the fifth cycle beyond bismuth, where, unless all analogy fails, we should not expect to find a metal with very high melting-point and coloured solutions. But if the atomic weight of uranium be 180, it conforms with the known properties of the element. The atomic volume is then 10, and the element takes its place

near tungsten and the platinum group, in the region of high melting-point, low atomic volume, and coloured solutions.

The facts known about the specific heat of uranium are, as far as they go, compatible with the atomic weight 180. The specific heat of metallic uranium has not been determined; but the specific heat of the lowest oxide Ur_2O_3 (Ur=180) was found by Regnault to be ·0619. The product of the specific heat of this oxide and the molecular weight is therefore 25·25; and if we subtract from this the assumed specific heat 13·00 of the two atoms of uranium and divide the remainder by 3, we arrive at a highly probable value for this heat of the atoms of oxygen, viz. 4·08. The heat of the oxygen atoms is by a similar process found to be 3·93 in sesquioxide of chromium, 4·12 in tungstic anhydride, and 4·04 in molybdic anhydride.

The following table exhibits the formulæ of the uranium compounds, taking the atomic weight to be respectively 120, 180, 240:—

Ur=120.	Ur=180.	Ur=240.
$UrCl_2$	$UrCl_3$	$UrCl_4$
UrO	Ur_2O_3	UrO_2
Ur_2O_3	$Ur_2O_{4\frac{1}{2}}$ (Ur_2O_5?)	UrO_3
$UrOCl$	$Ur_2O_3Cl_3$	UrO_2Cl_2
Ur_3O_4	UrO_2	Ur_3O_8
UrO_2	UrO_3	UrO_4

Uranium with atomic weight 180 may be regarded as the atom-analogue of chromium, Ur_2O_3 being the analogue of Cr_2O_3, and UrO_3 of CrO_3. The fact of the normal oxide of uranium, Ur_2O_5, being a higher oxide than the normal oxide of chromium, Cr_2O_3, is in accordance with the general tendency of atomicity to increase with atomic weight. To the foregoing it may be objected that tungsten has also claims to be taken as the atom-analogue of chromium; and many arguments might be raised on behalf of each element. Whether it is worth while to raise them is an open question. For it it must be remembered that atom-analogy is likely to be most perfect in twin cycles, and to be weaker when the cycles are dissimilar. This is evidently so in the first four cycles: an element in the first cycle resembles its atom-analogue in the second more closely than its atom-analogue in the third or fourth cycle; and an element in the third cycle resembles its atom-analogue in the fourth cycle more closely than its atom-analogue in the first or second.

With increase in the complexity of the cycle comes the development of new features and individualities; thus the property of forming coloured solutions appears for the first time in the third cycle. The factor having the greatest influence in determining the properties of an element appears to be

cyclic position, especially among the higher atomic weights. Now, as the cycle increases, the same progression of atomic weight produces less and less change in cyclic position, and consequently less in properties. Comparing the best-known members of the fifth cycle with the best-known members of the third and fourth, we remark that the progression of atomic weight from gold to bismuth is only about 11, while the progression from silver (107·66) to antimony (120) is about 12, and from copper to arsenic 11·9; and this, although the semicyclic progression from cæsium to gold, viz. 66, is three times the semicyclic progression from rubidium to silver, viz. 22·4. Therefore, while the dimensions of the fifth cycle are three times those of the fourth, the mean progression of atomic weight between adjacent atoms is about the same; the obvious inference is the presence of three times as many atoms in the fifth cycle: and upon these facts an argument may be founded to prove the fifth cycle to contain six primary series of seven elements. This supposition receives support from the closeness in atomic weight between cerium and lanthanum and between tantalum and tungsten, and also from the number of newly discovered elements, many of them earth metals, waiting to be classified*.

On the other hand, this equality in atomic progression does not hold good at the extremities of the cycles; and in these positions the atomic progressions are greatest. There is between potassium and calcium a difference of 1, between rubidium and strontium a difference of 2, while the progression from cæsium to barium is 4. Again, small progressions of atomic weight between adjacent elements in a large cycle might, it is conceivable, be insufficient to bring about transition from one full atomic type to another, and might result in the formation of twin or triplet elements not sufficiently different to have separate atom-analogues in the lower cycles. Cerium, lanthanum, and perhaps didymium may thus have common atom-analogues in yttrium, scandium, and aluminium, and uranium and tungsten in molybdenum and chromium. In this way the fifth cycle may have four or even only two internal periodic variations, and yet have more than thirty-one elements. Again, tungsten may be the atom-analogue of manganese and of the unknown element IV. *a* 7. However, these are questions too obscure to be answered in the present state of chemistry, and await the light of further investigation; but this seems clear, that we must expect to find in the fifth cycle developments not to be found in the lower ones.

* Some of these may form part of a sixth cycle.

IV. *Note on the Electrical Resistance and the Coefficient of Expansion of Incandescent Platinum.* *By* E. L. Nichols, *Ph.D. (Göttingen)**.

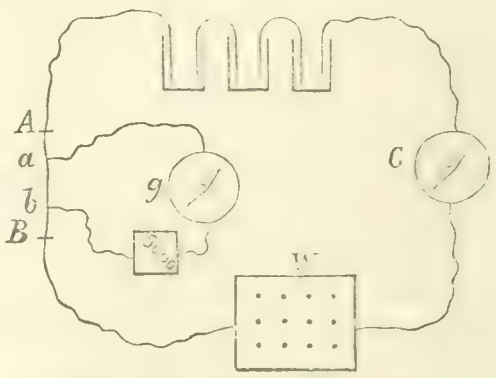
I. IN the measurement of temperatures above the red heat, the platinum pyrometer, in one form or another, is as important as the mercury thermometer at ordinary temperatures. The researches already completed, on the electric resistance and the coefficient of expansion of platinum, and on the specific heat of that metal, only serve, however, to remind us of the much that remains to be done before we may hope to attain to even a fair degree of accuracy in the measurement of temperatures above 500°.

The present writer, in order to compare the existing formulæ for the temperature of platinum from its electric resistance with those by means of which the temperature is calculated from the coefficient of expansion, and thus to gain a clearer idea of the relative usefulness of the two methods, has determined the resistance and the corresponding length of a platinum wire at various temperatures between 0° and the melting-point of that metal.

II. Upon a platinum wire 0·4 millim. in diameter and 100 millim. long, at points 55 millim. apart and equally distant from the middle of the wire, two very fine platinum wires were welded. They served to mark the ends of the portion of the wire to be measured, and to make electrical connexion with a shunt containing a sensitive galvanometer. The wire was heated by the current from a battery of forty Bunsen's cells. Its resistance was determined by the following method.

The wire (A B, fig. 1), together with a tangent-galvanometer (G) and a resistance-box (W), was in in direct circuit with the Bunsen's battery. A very small portion of the current was shunted around *a b*, the portion of the wire to be tested, and carried through a sensitive sine-galvanometer (*g*) and through a resistance-coil (*w*) of 5000 ohms.

Fig. 1.



* From Silliman's American Journal for November 1881, having been read at the Cincinnati Meeting of the American Association for the Advancement of Science, August 1881.

Now, with the above arrangement of apparatus, if w is very much larger than r (the resistance of the wire $a\,b$), so that the current through $a\,b$ is not sensibly less than that through the main circuit, we shall have

$$E = \frac{C}{r} = \frac{C'}{r'},$$

where C and C′ are the currents through $a\,b$ and through the shunt, and r is the resistance of the shunt.

But

$$C' = \sin U k',$$
$$C = \tan V k,$$

where U is the deflection of the sine-galvanometer and k' the constant of the instrument, and where V is the deflection of the tangent-galvanometer and k the constant of the latter instrument.

Then

$$r = \frac{\tan V}{\sin U} \cdot \frac{k}{k'} r' = \frac{\tan V}{\sin U} \cdot K,$$

where

$$K = \frac{k}{k'} r'.$$

The length of the wire $a\,b$ was measured by bringing the two microscopes of a comparator into such position that the terminal a was in focus in the field of one of the microscopes and b in the field of the other. Since these points were quite as near the middle as the end of the wire, every change of temperature caused a movement of both a and b; and it was by taking the differences of these that the true change in the length of ab was determined. As the microscopes were provided with excellent micrometer-scales and screws, a fair degree of accuracy was obtained by this method. Readings of the length of the wire at 20° agreed with a series taken upon a dividing-engine of known accuracy to within ·002 millim. The distance $a\,b$ at 20° was found to be 53·5576 millim.

The resistance of the cold wire was found (in terms of U, V, and K) by placing the wire in a naphthaline bath, and obtaining values of U and V with various amounts of current. From these readings a curve was drawn with $\frac{\tan V}{\sin U}$ as abscissæ and $\tan^2 V$ as ordinates, $\tan^2 V$ being taken as an expression for the heating effect of the current. The point of this curve corresponding to $\tan^2 V = 0$ was taken as the proper value of $\frac{\tan V}{\sin U}$ for the cold wire.

In measuring the resistance of the hot wire, the galvanometers were read simultaneously before and after each determination of the length.

The following table gives the results of the measurements, for temperatures ranging between 0° and a point not far below the melting-point of platinum. Both resistance and length of wire at 0° are taken equal to unity.

TABLE I.

Resistance.	Length.	Resistance.	Length.
1·0000	1·00000	3·7090	1·01229
1·0410	1·00002	3·7427	1·01223
1·5071	1·00125	3·7813	1·01285
1·9000	1·00289	3·8750	1·01349
2·1212	1·00380	3·8904	1·01371
2·2934	1·00456	3·9305	1·01378
2·3035	1·00489	4·0303	1·01450
2·7821	1·00732	4·0631	1·01469
2·8633	1·00763	4·0655	1·01495
2·9696	1·00809	4·0747	1·01499
3·3533	1·01022	4·0841	1·01514
3·3741	1·01003	4·1248	1·01540
3·4151	1·01042	4·2005	1·01567
3·6449	1·01160	4·2447	1·01632

III. Dr. Siemens has published three formulæ for the variation of the resistance of a platinum wire with the temperature.

The temperatures were calculated in one case (formula *a*) from the heating effect of a copper ball, the specific heat of copper being regarded as a constant; while the other two formulæ were derived from measurements with the air-thermometer.

These formulæ are:—

$$r = \cdot 039369\ T^{\frac{1}{2}} + \cdot 00216407\,T - \cdot 24127, \quad . \quad . \quad (a)$$

$$r = \cdot 0021448\,T^{\frac{1}{2}} + \cdot 0024187\ T + \cdot 30425, \quad . \quad . \quad (b)$$

$$r = \cdot 092183\ T^{\frac{1}{2}} + \cdot 00007781\,T + \cdot 50196, \quad . \quad . \quad (c)$$

where T is the absolute temperature and r the resistance of the wire. The following formula by Benoit is also sometimes used for the determination of high temperatures:

$$r = 1 + \cdot 002445\,t + \cdot 000000572\,t^2. \quad . \quad . \quad . \quad . \quad (d)$$

In this expression t denotes the temperature in degrees Centigrade.

When, as is frequently the case, it is more convenient to measure the length of a wire than its resistance, we may employ Matthiessen's formula,

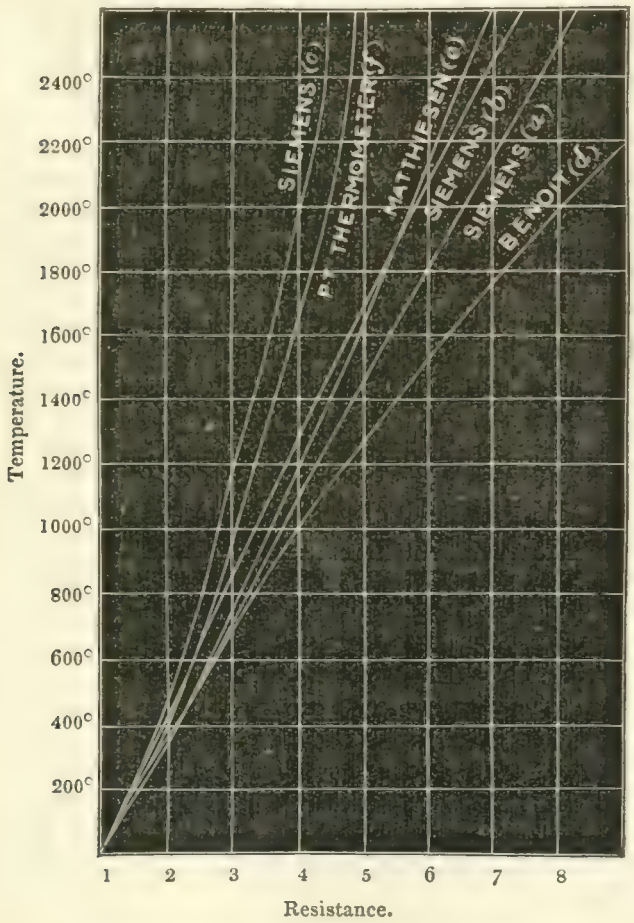
$$l = l_0(1 + \cdot 00000851t + \cdot 0000000035t^2); \quad . \quad . \quad (e)$$

or we may use the uncorrected scale of the platinum thermometer. The latter scale is expressed by the formula

$$l = l_0(1 + \cdot 00000886t). \quad . \quad . \quad . \quad . \quad . \quad (f)$$

These being almost the only data we possess for the calculation of the temperature of a hot wire, the question of their accuracy is of some importance. The formulæ may be best compared by plotting side by side the curves which represent them (fig. 2).

Fig. 2.



In fig. 2, resistance is substituted for length in curves *e* and *f*, using for that purpose the measurements given in

Table I. The following table affords a further comparison of the six formulæ.

In the columns *a* to *f* are given the temperatures, calculated by the several formulæ, at which the resistance of the wire, compared with its resistance at 0°, is given in the column marked "*r*."

Table II.

Length,	*r*.	Siemens.			Benoit.	Mat-thiessen.	Platinum thermo-meter.
		a.	*b*.	*c*.	*d*.	*e*.	*f*.
		°	°	°	°	°	°
1·0000	1·000	0	0	0	0	0	0
1·0032	2·000	325	402	420	378	342	375
1·0082	3·000	692	812	1108	708	720	917
1·0146	4·000	1086	1244	1950	1000	1170	1623
1·0280	5·000	1464	1682	3170	1272	1638	3100
.....	6·000	1828	2072	.....	1512	2158	
.....	7·000	2170	2387	.....	1766	2800	
.....	8·000	2470	2692	.....	1978		

A glance at the curves and at this table suffices to show how ill-deserved is the confidence generally felt in these formulæ. The discrepancies involve differences of hundreds of degrees.

IV. The methods employed by Dr. Siemens in the measurements represented by curves *b* and *c* were identical; but the platinum used contained slight impurities. To these impurities the disparity was due. Dr. Siemens found that such foreign substances as usually occur in commercial platinum affected both the resistance of the cold metal and the law of the change of resistance with the temperature.

Benoit's formula (*d*) depends for its accuracy upon the determination of the boiling-points of mercury, sulphur, cadmium, and zinc, for which temperatures he adopted the values given by Deville and Troost*. M. Ed. Becquerel opposed those values at the time of their publication; and later researches have confirmed him, at least so far as cadmium and zinc are concerned, in thinking them to be entirely too high.

In the following table the results obtained by Deville and Troost are compared with the more probable values given by other physicists:—

* Deville and Troost, *Annales de Chimie*, (3) vol. lviii.

TABLE III.

Metals.	Boiling-points. Deville and Troost.	Boiling-points. Other values.	
Hg	360°	350°	Regnault. [Journal, 1878).
S	440	448	Bennett (Silliman's American
		446	Carnelly and Williams (Quart.
Cd	860	772	Journ. Chem. Soc. 1876–78).
Zn	1040	884	Becquerel (*Comp. Rend.* lvii.).

The substitution of these values in Benoit's formula places it more at variance than before with the measurements of Matthiessen and Siemens—a variation probably due to the difference of behaviour noticed by the latter physicist in the case of different specimens of platinum.

The brief discussion of the above-mentioned results suffices, we think, to show that—

1st. The formulæ in question are based for the most part upon unwarrantable suppositions, such as the constancy of the specific heat of copper and platinum, the constancy of the coefficient of expansion of the latter metal, and upon the accuracy of certain very doubtful values for the boiling-points of zinc, cadmium, &c.

2nd. That, aside from the inaccuracy of those data, the varying resistance of different specimens of platinum renders any formula for the calculation of temperature of that metal from its electric resistance applicable only to the identical wire for which the law of change of resistance with the temperature has been determined.

3rd. That, from the data at command, we are not in position to calculate the temperature of an incandescent platinum wire from its change of resistance, nor from its length, nor indeed in any other manner, further than to express the temperature in terms of the length or the resistance of the wire.

4th. That, owing to the great variations shown by different specimens of platinum as regards its resistance, the determination of the expansion of the wire is to be preferred, whenever practicable, to the measurement of its conductivity.

V. *New Transformations of Ordinals.* *By* Sir James Cockle, *M.A., F.R.S., F.R.A.S., &c.**

37. It follows from my paper in the Number for last September (p. 189) that, when certain conditions are fulfilled, we get new transformations.

38. Before showing this, I give a brief verification† of some of the calculations.

39. We have identically

$$N_2 - L_2 = N - L + \omega(\Omega - 1); \quad . \quad . \quad . \quad . \quad (9)$$

and the values of B_2 and $C_2 + b_2$ are given in art. 20.

40. Art. 17 yields the system

$$\frac{C_2 + b_2}{N_2 - L_2} = \eta_2 = 3 - \frac{B_2}{b_2},$$

which the relation

$$b + 3\omega(\omega + 1) = 0 \quad . \quad . \quad . \quad . \quad . \quad (10)$$

reduces to

$$-(2\omega + 3) = \eta_2 = -(2\omega + 3).$$

41. But, by $(3)_2$ and (10),

$$\eta_2 = -\tfrac{1}{2}\{3 \pm (4\omega + 3)\} = -(2\omega + 3) \text{ or } 2\omega.$$

42. If, therefore, we take the radical positively, all the conditions of art. 17 are fulfilled, and we determine ω from (10), thus (see art. 32) obtaining :—

$$\omega = \frac{U}{4} - 1 \text{ or } -\frac{U}{4};$$

$$\eta_2 = -\frac{U}{2} - 1 \text{ or } \frac{U}{2} - 3.$$

43. The other two forms for η_2, viz. $\frac{U}{2} - 2$ and $-\frac{U}{2}$, are improper, and in general irrelevant. If, however, $\omega = -\frac{3}{4}$, one of the improper forms may be relevant, and only one. Both cannot be so, because, when ω takes a single value, that value is $-\frac{1}{2}$.

* Communicated by the Author.

† To verify art. 10, let $b = 0$, and take the radical negatively. Then (1), (2), become B, C = 0, and (3) is an identity. But B, C = 0 together make $x^3 X t$ linear in x^n, while $b = 0$ makes $x^2 X s$ linear in x^n ; or, in other words, the terordinal in z is binomial as it stands. I have verified arts. 19 and 20 by the substitutions

$$A, E = 0, 0;\ 1, 1;\ -1, -1;\ 2, 1.$$

44. Using suffixes in conformity with the analogies of art. 17, and determining h_2 and k_2 from $(\mathrm{iv})_2$ and $(\mathrm{v})_2$, we shall have (art. 13),

$$f(\mathrm{D})_2=(\mathrm{D}+a_2-1)\{\mathrm{D}^2+2(a_2-1)\mathrm{D}+3f_2-a_2(2a_2-1)\},$$

$$\mathrm{F}(\mathrm{D})_2=(\mathrm{D}+e_2-1)\{\mathrm{D}^2+2(e_2-1)\mathrm{D}+3g_2-e_2(2e_2-1)\},$$

and it only remains to determine f_2 and g_2.

45. But

$$f_2=\mathrm{L}_2+a_2^2-a_2=\mathrm{L}-\mathrm{A}^2+\mathrm{A}+a_2^2-a_2,$$

$$g_2=\mathrm{N}_2+e_2^2-e_2=\mathrm{N}-\mathrm{E}^2+\mathrm{E}+e_2^2+e_2.$$

46. Now (art. 21) we have four sets of relations; and for the first (art. 22) we get

$$f_2=4\mathrm{L}+a_2^2-a_2,\quad g_2=4\mathrm{N}+e_2^2-e_2\,;$$

for the second and third sets respectively (arts. 34 and 35)

$$f_2=4\mathrm{L}+a_2^2-a_2,\quad g_2=\mathrm{N}+(e_2-\tfrac{1}{2})^2,$$

and

$$f_2=\mathrm{L}+(a_2-\tfrac{1}{2})^2,\quad g_2=4\mathrm{N}+e_2^2-e_2\,;$$

and for the fourth (art. 36),

$$f_2=\mathrm{L}+(a_2-\tfrac{1}{2})^2,\quad g_2=\mathrm{N}+(e_2-\tfrac{1}{2})^2.$$

47. The quadratic factor of $f(\mathrm{D})_2$ can be written

$$(\mathrm{D}+a_2-1)^2-(3a_2^2-3a_2+1-3f_2),$$

which for the first and second sets is

$$(\mathrm{D}+a_2-1)^2-(1-12\mathrm{L}),\ =(\mathrm{D}+a_2-1)^2-(\tfrac{3}{2}\mathrm{I})^2,$$

while for the third and fourth it is

$$(\mathrm{D}+a_2-1)^2-(\tfrac{1}{4}-3\mathrm{L}),\ =(\mathrm{D}+a_2-1)^2-(\tfrac{3}{4}\mathrm{I})^2;$$

and there are corresponding results for $\mathrm{F}(\mathrm{D})_2$, e_2, N, and J.

48. The relation characteristic of the first set has (art. 33) been given. That for the second is obtained thus:

$$\tfrac{1}{4}-3\mathrm{L}=(\mathrm{A}-\tfrac{1}{2})^2=\omega^2,\text{ or }{}_{1}{}^{9}\!/_{6}-\tfrac{9}{4}l=\omega^2;$$

whence (arts. 29 to 32)

$$\tfrac{9}{16}\mathrm{I}^2=\omega^2=\left(\frac{\mathrm{U}}{4}-1\right)^2\text{ or }\left(\frac{\mathrm{U}}{4}\right)^2,$$

and

$$\mathrm{U}\pm3\mathrm{I}=0,\text{ or }4.$$

49. The corresponding relations for the third and fourth sets are :—

$$\mathrm{U}\pm3\mathrm{J}=0\text{ or }4\,;\quad \mathrm{U}=0\text{ or }4.$$

50. We may state all our results concisely as follows:— When I, U, J are connected by one of the relations

$$U-3(1-0^{2-m})I+3(1-0^{2-n})J=4, \quad . \quad . \quad (11)$$

$$U+3(1-0^{2-m})I-3(1-0^{2-n})J=0, \quad . \quad . \quad (12)$$

the terordinal in z can be transformed into

$$u+\frac{\left(D+e_2-1+\frac{3}{2^n}J\right)(D+e_2-1)\left(D+e_2-1-\frac{3}{2^n}\right)}{\left(D+a_2-1+\frac{3}{2^m}I\right)(D+a_2-1)\left(D+a_2-1-\frac{3}{2^m}I\right)}x^3u=0$$

. . . . (13)

wherein, for the first set $m=1=n$; for the second, $m=1$, $n=2$; for the third, $m=2$, $n=1$; and for the fourth, $m=2=n$.

51. Both I and J are independently bisignal; and, without affecting I or J, we can change U into $U\pm4k$, where k is an integer. The restriction in art. 35 disappears.

52. When any one of the conditions implied in (11) or (12) is satisfied, Boole's algorithm would enable us at once to form a terordinal having its factors, as well in the numerator as in the denominator, in arithmetical progression. But (13) yields as many results again; for, since a_2-e_2 has two values, (13) has two forms.

53. Take $U=0$. Here $\eta_2=-1$ or -3. The result yielded by -3 may be always transformed* into that obtained by Boole's algorithm; not so that yielded by -1, which is peculiar to the criticoidal transformation.

2 Sandringham Gardens, Ealing near
London W., December 15, 1881.

Errata in Vol. XII. (*additional*).

Page 189, art. 2, line 2, first term, for d^2 read d^3
„ 190, last line of text, for b read $-b$
„ 195, line 2, for ϖ *read* ω
„ 196, art. 34, line 3, *for* L *read* 3L
„ „ „ „ 5, *for* N *read* 3N
„ „ art. 36, line 3, *for* -2 *read* 0 or 4

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* This transformation is direct, viz. effected by Boole's process. The criticoidal transformation is indirect: viz. we connect two terordinals by means of a biordinal. I have applied Boole's algorithm in the 'Messenger of Mathematics' for November 1881 (pp. 109, 110).

VI. *On the Distribution of the Molecular Velocities in Gases.* By C. Cellérier*.

THE molecules of a gas are, according to the modern theory, withdrawn from each other's action during almost the whole of their passage, which takes place in this case in straight lines and with a uniform motion. In all that follows, we shall suppose that a homogeneous gaseous mass, everywhere at the same temperature, is contained in an invariable enclosure impermeable to heat. It is then in a permanent state. The molecular velocities, to which a mean value is often attributed, are really unequal, constantly modified by the collisions; and after a certain time the proportion of the molecules possessing a more or less great velocity will no longer change: the aim of this investigation is to determine their law—that is to say, the *final distribution* of the velocities.

The men of science who have occupied themselves with the molecular collisions, among others M. Clausius, have in general likened them to that of two solid, perfectly elastic spheres, equal in mass and radius, at least in the conditions of permanence which we have just mentioned. We shall attribute to the molecules any form whatever, variable from one to another, so as not to exclude the cases in which they are regarded as a grouping of atoms, those in which they are supposed to possess polarity, &c. With this exception, we shall admit the other hypotheses contained in the preceding point of view, and which may be summed up thus:—

1. The sum of the *vires vivæ* of translation of two molecules is not altered by the collision;

2. All the masses are equal;

3. The collision may be compared to that of two solid bodies.

It must be remarked that the total *vis viva* of translation in the whole of the gas, measuring the pressure, is permanent; it is equally invariable for each of the small portions into which the mass may be divided; therefore the first hypothesis, like the second, consists in neglecting some irregularities which may present themselves in one direction or the other, and which will but little modify the result sought. Hence, if the hypothesis of solidity be admitted, the laws of the collision for two spheres or two surfaces of any kind will certainly be the known laws—that is to say, that the projection of the velocity of one of the centres of gravity upon the common tangent

* Translated from the *Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles*, Oct. 15, 1881, t. vi. pp. 337–373.

plane is not altered by the collision; otherwise the sum of the *vires vivæ* of translation would also be so.

In regard to the solidity so far as the collision is concerned, let us remark that the trajectory of one of the centres of gravity is composed of two straight lines joined by a curve corresponding to the period of mutual action. In the case of two springs bending one another this curve has a certain extent; for two solid spheres it is reduced to a point. More precisely, if α denotes the displacement of the centre of gravity of a molecule during the collision (that is to say, during the sensible action of another molecule), and d the distance of the two centres at that instant, the condition of solidity consists in α being imperceptible compared with d.

This is only a consequence of the gaseous constitution of which we wish to calculate the effect; for this calculation consists in regarding as negligible the ratio $\frac{\alpha}{d'}$, d' being the mean distance traversed by a molecule between two successive collisions. It is so also for gases strongly compressed; for such was the case in one of the principal experiments from which the nothingness of the internal work has been concluded. Now, if a body in the solid state pass to that of a compressed gas occupying either eight times or twenty-seven times its original volume, the mean interval between two neighbouring molecules is merely doubled or trebled. At the distance of the original interval they certainly act the one upon the other; and it is evident that in the gaseous state they can hardly be displaced the distance d without collision: therefore d and d' are quite of the same order of magnitude; and if $\frac{\alpha}{d'}$ is negligible, so is $\frac{\alpha}{d}$.

It is moreover easy to see, for solid molecules it is true, that the degree of incompressibility of a body serves as a measure for that of its molecules. If two equal masses, divided each into n parts, collide with great force, the stress borne by one part will be the same as if it had been isolated and received the impact of one part only. If in that case it is sensibly compressed, as much will happen when it forms part of the mass, and this will be compressed in the same proportion.

We shall examine some particular cases of molecular motions before attacking the general question; but first some remarks are necessary in order to avoid repetitions.

(1) The unit of length is supposed to be chosen so that the total volume comprised in the enclosure is the unit, although it be very great. Even when the molecules are divided into

numerous groups, each of them will contain an immense number. The molecules are classed in groups according to the nature of their velocities ; the molecules of one and the same group are supposed to be distributed in a sensibly uniform manner throughout the space.

(2) If n is the number of molecules of a group, the number of those of which the centre of gravity is comprised in a determined volume H will be nH, with an imperceptible relative error. Indeed this is equivalent to saying that it is proportional to the volume. Now it is evident that, when the total volume is divided into a great number of equal parts, the number of centres contained in each will be very great, and the ratios between them sensibly equal to unity. There would only be a difference for the proportions contiguous to the side, it not being possible for the centre of a molecule to exist at a distance from this which falls below a certain limit ; but this limit being infinitely small in comparison with the total dimensions, the result is not thereby altered.

(3) We shall have to value the total number of collisions, or that of certain collisions, during a period constantly designated by t ; this is supposed such that there is an immense number of collisions, and yet they affect only an imperceptible fraction of the total number of molecules of one and the same group; also the cases in which during that time one and the same molecule suffers two collisions can be completely neglected, their number being infinitesimal in proportion to the former.

(4) The directions of the velocities will be indicated by drawing parallel to each of them a radius of a fixed sphere, which will be named the *typical sphere*: the point of the surface at which it ends will be the *typical point* of the velocity ; any portion of the surface will be called the *typical region* of the velocities whose typical point is on that region—which will serve for distributing them in groups.

First Disposition.

All the centres of gravity of the molecules have one and the same velocity $\frac{1}{2}$V, *in parallel directions, which we figure vertical; but* n *of these molecules are descending, and the others, in number* n′, *are ascending.*

Let G, G′ be the centres of gravity of two molecules which collide, and O their common centre of gravity; this is in the middle of GG′, and consequently immovable before the collision; it will therefore continue to be so after it in the absence of any extraneous force : consequently, if after the collision G, G′ are in P, P′ and an instant afterwards in Q, Q′, the triangles POQ, P′OQ′ being equal, the small spaces PQ, P′Q′

passed through will be equal and parallel. It will therefore be the same with the velocities after the collision, which will be besides in the opposite direction. The *vis viva* not having changed, their value will still be $\frac{1}{2}V$; their projection upon the common tangent plane, too, not having changed, the old and the new velocity will have for their bisectrix the normal to that plane.

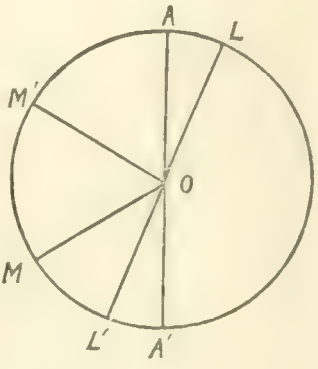
Case I. *All the molecules are spheres of the same diameter* ρ.

Let O be the centre of one of the ascending molecules, and let us trace a sphere with centre O and radius ρ. In order that a molecule of the first kind may come into contact with the sphere during the time t, its centre G must encounter the surface; and for this it must be, at the commencement of the time t, interior to the cylinder which would be formed by drawing, through all the points of the upper hemispherical surface, verticals directed upwards and of the length Vt, since V is the relative velocity of the two molecules. This cylinder is terminated below by the hemisphere, above by an equal hemisphere; its volume is that of a cylinder of height Vt and base $\pi\rho^2$; the total volume of the similar cylinders corresponding to all the molecules of the second kind is $H=n'\pi\rho^2Vt$; the number μ of centres of molecules of the first kind which are found there at the commencement of the time t is therefore nH, according to the second remark above. Hence, putting $\pi\rho^2=a$, results

$$\mu=ann'Vt. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This number, then, is also that of the collisions during the time t; for, according to the third remark, the cases in which two descending molecules would have their centres in one and the same cylinder can be omitted, the collision of the one preventing that of the other.

Let us employ the above sphere as typical of the velocities, and draw various meridians through the vertical O A. If the point G (the centre of a descending molecule) meets the surface on one of them in M′, the tangent plane common to the two molecules will be parallel to the tangent plane in M′, so that, O M being drawn parallel to the new velocity, its typical point M will be upon the same meridian, and the angle A O M double



A O M′. The latter can moreover take all values from 0 to $\frac{\pi}{2}$, while the other varies from 0 to π.

Let ω' be a surface-element situated in the upper hemisphere, and comprised on the one side between two meridians very near one another and making the angle $d\phi$, on the other between two horizontal circles having i and $i+di$ for their angular distances from the point A, and consequently $\rho \sin i$ for radius; we can, as is known, assimilate ω' to a small rectangle having for its sides $\rho\, di$, $\rho \sin i\, d\phi$, and for its surface $\omega' = \rho^2 \sin i\, di\, d\phi$. The points M corresponding, as above, to all the points M′ situated in the area ω' evidently constitute an element ω, comprised between the same meridians and between two horizontal circles having, for their angular distances from the point A, $2i$ and $2(i+di)$, and therefore for their surfaces

$$\omega = 2\rho^2 \sin 2i\,.\,di\, d\phi, \text{ or } \omega = 4\omega' \cos i.$$

By drawing a vertical of the length Vt through each point of the area ω', we shall form a very thin prism comprised in the cylinder of which we have spoken, in the interior of which the centre G of a molecule must be in order that the typical point of the velocity due to the collision may be within the element ω. Its horizontal section, the plane of which makes the angle i with the tangent plane in M′, will consequently be $\omega' \cos i$ or $\frac{\omega}{4}$.

If now ω designates not an element, but a finite portion of the sphere, it can be decomposed into elements such as the preceding; and for the velocity due to the collision to correspond to this new region, the point G must be interior to one or the other of the small prisms corresponding to each element, or, what comes to the same thing, within the second cylinder formed by their union, the section of which is again $\frac{\omega}{4}$. Supposing the construction of this second cylinder identically repeated for all the molecules of the second kind, the number μ' of the collisions for which the new velocity corresponds to the region ω will be the number of the centres G of descending molecules situated in the interior of the second cylinder and of its homologues.

Now μ was the number of points G interior to the first cylinder and its homologues; $\frac{\mu}{\mu}$ is therefore the ratio of their

volumes or of their sections $\frac{\omega}{4}$, $\pi\rho^2$, whence results

$$\mu' = \frac{\omega}{4\pi\rho^2}\mu. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In particular, if it be wished that the acute angle of the new velocity with a given fixed direction LOL′ shall be greater than a given acute angle α, it will be necessary that M be within a zone bounded by two circles whose poles are L, L′, and having an angular distance α from those two points. The height of the zone being $2\rho\cos\alpha$, its surface will be $\omega = 4\pi\rho^2\cos\alpha$, and the number of collisions satisfying the preceding condition will be

$$\mu' = \mu\cos\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Prof. Clausius has remarked that for the new velocities all directions are equally probable. This is what equation (2) expresses, since the number of collisions μ' is independent of the position of ω. It is this fundamental property that we have now to demonstrate for molecules which are not spherical.

Case II. *All the molecules of the first kind have any surface* S, *those of the second kind any surface* S′; *those of the same sort are not only equal, but similarly oriented; that is to say, the homologous straight lines joining two points of them are parallel.*

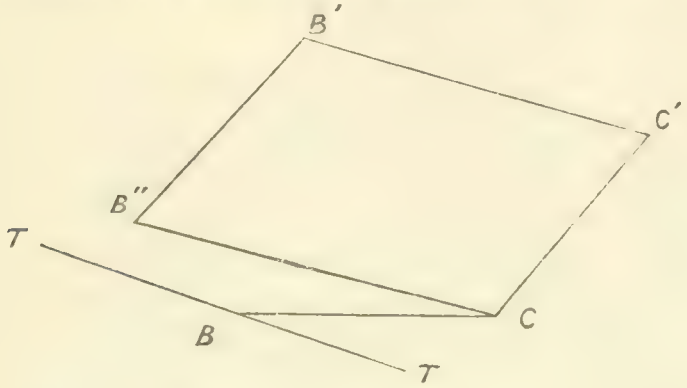
Let us trace apart a typical sphere with centre O, radius 1, for which we will again use the same figure, A being its highest point. If we wish the new velocity of a descending molecule to have M for its typical point, the normal to the plane of the collision must be OM′, the bisectrix of OMA. On an ascending molecule S′ we shall find the point M″, at which the interior normal is paralled to M′O; there is only one such point, because we shall suppose S, S′ convex, without points or sharp edges. We shall displace S by a motion of translation only, so that this surface shall touch S′ in M″.

On repeating the operation for all the points M within an infinitely small circle ω, the locus of the points M′ will be an element ω' of the sphere, that of the points M″ an element ω'' of S′, that of the corresponding positions of G a surface-element γ in the space. Carrying through each of these points a vertical of the length Vt, we shall have formed a thin prism of the volume $h = Vt\gamma\cos i$, i being the angle made by the plane of γ with the horizontal. We will name it the *efficient volume* of the region ω, because it is that in which G must be at the commencement of the time t in order that the new

velocity may correspond to ω. We shall thus have

$$\frac{h}{\omega} = Vt\cos i, \quad \frac{\gamma}{\omega''}\frac{\omega''}{\omega'}\frac{\omega'}{\omega}.$$

To simplify this value, let B′, C′ denote two points of γ the distance between which is not very small relatively to the dimensions of γ; let B, C be the corresponding points of ω'', and let us draw C B″ equal and parallel to B′C′. The point



G being at first in C′ the contact was in C; a motion of translation bringing G to B′, the point of S which was in C comes to B″; at that instant the contact being in B, let T T be the tangent plane common to S and S′. The point B″ being upon S, and C upon S′, the distance of each from the tangent plane is, as we know, an infinitesimal of the second order. Therefore the straight line C B″ makes with the plane an infinitely small angle; and it is the same with B′ C′. The same could be said of B″D′, D′ being another point of γ. Therefore the plane of γ, in its entirety, is parallel to the plane of ω'', and consequently to that of ω'.

Next let us call M specially the centre of the circle ω, the points M′, M″ being those which correspond to it; it is evident that the ratios $\frac{\omega'}{\omega}$, $\frac{\omega''}{\omega'}$, $\frac{\gamma}{\omega''}$ do not change if we alter the form of the elements, provided that they exactly correspond to one another and, further, are still at the same place—that is to say, contain M, or M′, or M″; we have, then, $\omega = 4\omega'\cos i$, a relation previously found for a rectangular form. We shall moreover substitute

$$\frac{\omega''}{\omega'} = f(M''), \quad \frac{\gamma}{\omega''} = F(M'', S):$$

the second of these indicates that $\frac{\gamma}{\omega''}$ depends at the same time on the position of M″ and that of S relatively to S′; and the

first, that $\frac{\omega''}{\omega'}$ depends only on the position of M″ relatively to S′. It must be remarked that, if while preserving these relative positions we change the general orientation of the figure to make it correspond to another region ω, the functions f, F will remain the same.

Lastly, if we name H the total efficient space $n'h$ found by combining those of all the molecules of the second kind, we shall get, by the preceding substitutions,

$$\frac{H}{\omega} = \frac{n'Vt}{4} . f(M'') . F(M'', S).$$

(It could be demonstrated that $f(M'') = RR'$, R and R′ being the principal radii of curvature of the surface S′ at the point M″.)

Case III. The data are the same as in the second, except that, for each kind of molecules, S and S′ can be oriented in all ways, indifferently. It is clear that the equally frequent orientations of S can be replaced by a numerous series of suitably chosen orientations D_1, D_2, D_3, &c., the character of which could even be indicated by a differential notation ; but it is needless to make a choice as regards the mode of determining them; only they possess the essential property that, if they be all rotated one and the same angle in the same direction about parallel axes, they form an equivalent system of equally frequent orientations. We shall designate by D'_1, D'_2, D'_3, ... the analogous orientations for S′.

The totality of the collisions will be divided into groups by combining those in which the molecules of the first kind have one and the same orientation D, and those of the second kind one and the same D′.

The efficient space for a single group is the number H which we have just found, if n' be replaced by n'', the number of molecules of the second kind in the group. Designating now by H the total efficient volume for all the groups combined, we shall have

$$\frac{H}{\omega} = \frac{Vt}{4} \Sigma' \Sigma n'' f(M'') F(M'', S),$$

the sum Σ' applying to the orientations D'_1, D'_2, ..., and Σ to D_1, D_2, &c. To form the latter, one has to leave the position D′ of S′ invariable, as well as M″, so that $f(M'')$ is the common factor ; then S is to be associated with them in the successive positions D_1, D_2, ..., n'' being always the corresponding number of molecules of the second kind: this number is consequently sensibly constant. The positions once

found, if the figure be rotated one and the same angle about one and the same axis carried through G′, the orientations of S will again be of equal frequence. Therefore $\Sigma n''F(M'', S)$ is a function of the same nature as $f(M'')$, depending only on the position of M″ upon S′, and not on the orientation of the latter. We shall therefore have

$$\frac{H}{\omega} = Vt\Sigma'\phi(M''),$$

the function $\phi(M'')$ being again of the same nature.

The sum will be formed by giving to S′ the positions D'_1, D'_2, ... and taking for M″, each time, the point at which the normal is parallel to M′O. If we rotate all the positions thus formed one and the same angle about one and the same axis passing through G′, the normals to the points M″ will again have a common direction, and $\phi(M'')$ will be the same for each figure; consequently $\Sigma'\phi(M'')$ will not have changed. The new positions of S′ will again be of equal frequence, and will then all be found to be those which it would be necessary to employ on taking for M′ another point of the sphere, or (which comes to the same) on supposing ω at another place. Moreover, as this may be any place whatever, the value of H is independent of the position of ω; and consequently it is the same with the corresponding number of collisions—which was to be demonstrated.

Case IV. This will be the general case, in which *the molecules will have any unequal forms whatever.*

We may evidently assimilate them to a numerous series of forms F_1, F_2, F_3, &c., serving as types, and each of which may be oriented in all possible ways. We shall divide the collisions into groups by putting in the same those in which the molecules of the first kind have one and the same figure F, and the others one and the same, F′. From what we have just seen, all directions of the new velocities will be equally probable for each group, and therefore also for their union or for all the collisions; and the same property once demonstrated, equation (3), which is the immediate consequence of it, may also be said to be so, μ being the total number of the collisions. As to this number, whatever may be the shapes of the molecules, it is proportional to each of the quantities n, n', V, t separately when it varies alone, the others remaining constant; thus its ratio to $nn'Vt$ is an absolute constant depending on the nature of the molecules, and which may be designated by a. Equation (1) is thus verified.

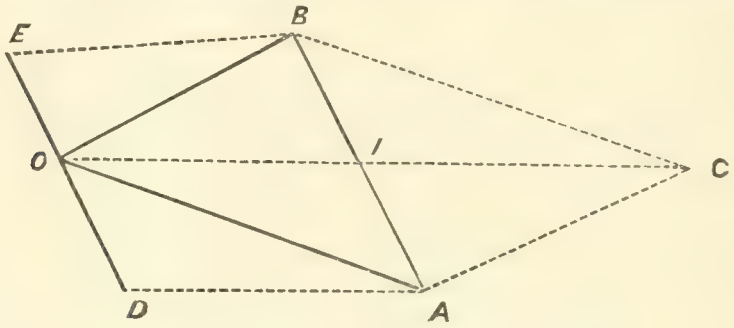
Second Disposition.

The molecules are of two kinds, of which the numbers are n, n′. *Those of one and the same kind have equal velocities, parallel and in the same direction, of which the value is* u *for the first, and* v *for the second; the angle of the velocities is* θ.

Let us represent these velocities, in magnitude and direction, by $OA=u$, $OB=v$; let V, V′ be the diagonals AB, OC of their parallelogram; we shall have

$$V=\sqrt{u^2+v^2-2uv\cos\theta},\quad V'=\sqrt{u^2+v^2+2uv\cos\theta}. \quad (4)$$

To these let us draw parallels D O E, A D, B E. O B may be regarded as the resultant of two other velocities O E, O I; in like manner O A is that of O D, O I. Thus the velocities of



all the molecules are exactly the same as if the entire mass with its boundary had a velocity of translation represented in magnitude and direction by O I or $\frac{1}{2}V'$, while the relative velocities of all the molecules at the commencement of the time t amounted to O D for the first species, to O E for the second. As in the first disposition, these velocities are parallel, in opposite directions, and all equal to $\frac{1}{2}V$. Consequently in the relative motion every thing will take place as we have already seen. The total number of the collisions will again be the value of μ given in equation (1); besides, designating by θ' the acute angle made by one of the new velocities with O I, there will be, according to equation (3), a number $\mu\cos\alpha$ of collisions, for which $\theta'>\alpha$, α being an acute angle chosen arbitrarily.

All these new absolute velocities will be found by compounding the preceding ones with O I or $\frac{1}{2}V'$, which makes the angles θ' and $\pi-\theta'$ with those resulting from one and the same collision. These absolute velocities will therefore be

$$\sqrt{\tfrac{1}{4}V^2+\tfrac{1}{4}V'^2\pm\tfrac{1}{2}VV'\cos\theta}$$

It can be verified from formula (4) that the sum of their squares is u^2+v^2, as it ought to be, the *vis viva* not having changed.

For brevity let us agree to designate by δ, for any collision whatever, the numerical difference of the squares of the new velocities; in the present case we have

$$\delta = VV' \cos\theta', \text{ whence } \delta = \text{ or } < VV'.$$

Next, let z be a given positive number. If the number of collisions for which $\delta < z$ is required, the answer will be $\delta = \mu$ if $z > VV'$, since the condition is satisfied by all the collisions; but if $z < VV'$, on the acute angle α being chosen so that we have $VV' \cos\alpha = z$, the relation $\delta < z$ gives $\cos\theta' < \cos\alpha$ or $\theta' > \alpha$; and, as we have seen, the number of collisions sought will be $\mu \cos\alpha$, or $\frac{\mu z}{VV'}$. On replacing z by $z + dz$, those numbers in the two cases will be μ or $\frac{\mu(z+dz)}{VV'}$; taking the difference, we shall have the number μ'' of collisions for which δ is comprised between z and $z + dz$: consequently

$$\mu'' = 0 \text{ if } z > VV'; \quad \mu'' = \frac{\mu dz}{VV'} \text{ if } z < VV'. \quad . \quad (5)$$

Third Disposition.

The molecules are again of two kinds, in number n, n'; *for each kind the velocities are all equal, viz.* u *for the first,* v *for the second. For the first kind all are parallel and in the same direction; for the second they are in all directions indifferently.* We neglect the collisions occurring between the molecules of the second kind.

Let us suppose a typical sphere described having for its centre a fixed point O, and for its radius unity; let us divide it into elements ω, and take each of them for the typical region of a group of velocities—that is to say, of the group of molecules of the second kind to which those velocities belong. The velocities being in all directions indifferently, the number of molecules of the group will be $\frac{n'\omega}{4\pi}$, 4π being the surface of the sphere. The number of collisions effected between this group and the molecules of the first kind returns into the second disposition, and will be deduced from μ by replacing n' by $n'\frac{\omega}{4\pi}$. Putting

$$k = ann't, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

formula (1) will give $\mu = \frac{k\omega V}{4\pi}$; the number of collisions for which δ is comprised between z and $z + dz$ will be, according to equations (5), $\mu'' = 0$ or $\frac{\mu dz}{VV'}$, for the single group. Let us

designate this number by m' for all the groups united, and the total number of collisions by m; we shall have

$$m=k\Sigma\frac{\omega}{4\pi}\mathrm{V},\quad m'=k\mathrm{F}(u,v)dz,\quad \mathrm{F}(u,v)=\Sigma\frac{\omega}{4\pi\mathrm{V}'};\quad .\quad (7)$$

the sums are extended to all the elements ω of the sphere by suppressing in the second the terms for which the angle θ of the velocities would not satisfy the condition $\mathrm{VV}'>z$. Supposing the radius O I of the sphere parallel to the velocity u, we can take for ω the narrow zone comprised between two circles having I for their pole, and θ and $\theta+d\theta$ for their angular distances from that point. Its altitude being $\sin\theta\,d\theta$, we shall have $\omega=2\pi\sin\theta\,d\theta$; the angle θ, constant throughout its extent, will be that of the velocities; V, V′ will also be the same for the whole of the zone. The sums Σ will become integrals from 0 to π. We shall thus have for the indefinite integrals, from the values (7) and (4),

$$m=\tfrac{1}{2}k\int\mathrm{V}\sin\theta\,d\theta=\frac{k}{6uv}\mathrm{V}^3,$$

$$\mathrm{F}(u,v)=\tfrac{1}{2}\int\frac{\sin\theta\,d\theta}{\mathrm{V}'}=-\frac{\mathrm{V}'}{2uv}.$$

For $\theta=\pi$, $\mathrm{V}^3=(u+v)^3$, for $\theta=0$, $\mathrm{V}^3=(u-v)^3$ or $(v-u)^3$, according as $u>$ or $<v$. Consequently

$$m=kf(u,v)\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad (8)$$

on putting

$$f(u,v)=u+\frac{v^2}{3u}\text{ if }u>v,\quad f(u,v)=v+\frac{u^2}{3v}\text{ if }v>u.\quad .\quad (9)$$

This function is continuous in value, but not in form.

Let us remark next that, from values (4) we get

$$\mathrm{VV}'=\sqrt{(u^2+v^2)^2-4u^2v^2\cos^2\theta},$$

an expression varying with θ between the limits u^2+v^2 and $\sqrt{(u^2-v^2)^2}$, or the numerical value of u^2-v^2. z must be supposed $<u^2+v^2$, since the collision does not alter the sum of the squares of the velocities.

(1) If, moreover, z is $<\sqrt{(u^2-v^2)^2}$, the condition $z<\mathrm{VV}'$ being constantly satified, in the value of $\mathrm{F}(u,v)$ it will be necessary to integrate from $\theta=0$ to $\theta=\pi$, which gives

$$\mathrm{F}(u,v)=\frac{4}{2uv}[u+v-\sqrt{(u-v)^2}];$$

consequently

$$\mathrm{F}(u,v)=\frac{1}{u}\text{ if }u>v;\quad \mathrm{F}(u,v)=\frac{1}{v}\text{ if }v>u.\quad .\quad .\quad (10)$$

(2) But if $z > \sqrt{(u^2-v^2)}$, we shall designate by θ' an acute angle such that we have

$$\sqrt{(u^2+v^2)^2-4u^2v^2\cos^2\theta'} = z;$$

and it will be necessary to integrate only from $\theta=\theta'$ to $\theta=\pi-\theta'$, between which limits $VV' > z$. We thus find

$$F(u,v) = \frac{1}{2uv}\left[\sqrt{u^2+v^2+2uv\cos\theta'} - \sqrt{u^2+v^2-2uv\cos\theta'}\right]$$

The factor in brackets having for its square

$$2u^2+2v^2-2\sqrt{(u^2+v^2)^2-4u^2v^2\cos^2\theta'} \text{ or } 2(u^2+v^2-z),$$

the result will be

$$F(u,v) = \frac{\sqrt{2(u^2+v^2-z)}}{2uv}. \quad . \quad . \quad . \quad . \quad (11)$$

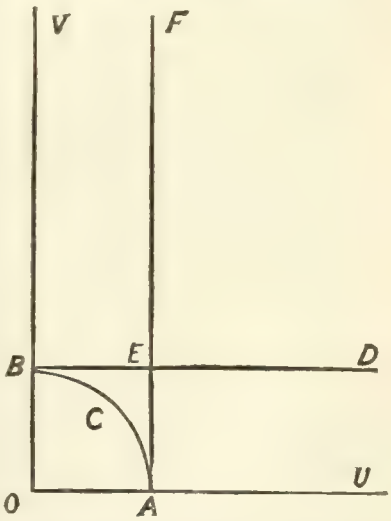
Let us now suppose that the number of collisions is required for which one of the new velocities is comprised between two given numbers x and $x+dx$ differing very little. The difference δ will then lie between two numbers z and $z+dz$ corresponding to those; and consequently the number sought will be again the value (7) of m'. If z corresponds to x, the squares of the new velocities being x^2, $u^2+v^2-x^2$, we shall have to suppose $x^2 < u^2+v^2$; and, moreover,

(1) If x is the greater of the two velocities, $z=2x^2-u^2-v^2$;

(2) If x is the smaller, $z=u^2+v^2-2x^2$.

In both cases it will be necessary to take $dz=4x\,dx$, the sign $-$ found in the second signifying merely that x corresponded to $z+dz$ and $x+dx$ to z.

Calling the second velocity x', x and x' will both be comprised between u and v, or both not comprised, according to whether we have $z <$ or $> \sqrt{(u^2-v^2)^2}$. Indeed in the first case the difference of the squares of the velocities, which was $\pm(u^2-v^2)$, has become z, or has diminished; the velocities have therefore come nearer to equality, whilst in the second they have receded from each other. It will therefore be necessary to employ formula (10) in the first case, formula (11) in the second.



The result can be better expressed by regarding u and v as rectangular coordinates of a variable point P, referred to the axes OU, OV, the point being in the angle UOV. The quadrant ACB

has O for its centre, x for its radius; the straight lines AF, BD are parallel to the axes. We shall then have

$$m' = 4k\mathrm{F}(u, v)x\,dx, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

the function F of the position of the point denoting:—

(1) If P is interior to OACB . . $\mathrm{F}(u, v) = 0$;

(2) If P is in the space ACBEA, . $\mathrm{F}(u, v) = \dfrac{\sqrt{u^2 + v^2 - x^2}}{uv}$,

(3) If P is in the space FED, . . $\mathrm{F}(u, v) = \dfrac{x}{uv}$;

(4) If P is in the space DEAU, . $\mathrm{F}(u, v) = \dfrac{1}{u}$;

(5) If P is in the space FEBV, . $\mathrm{F}(u, v) = \dfrac{1}{v}$.

In fact, for OACB we have $u^2 + v^2 < x^2$, which is impossible. For ACBEA, $u < x$ and $v < x$; for FED, $u > x$ and $v > x$; for both, x being not comprised between u and v, formula (11) is to be employed, substituting in it $z = 2x^2 - u^2 - v^2$ in the first case (where $x > x'$), and $z = u^2 + v^2 - 2x^2$ in the second (where $x < x'$).

For DEAU, $u > x$ and $v < x$; for FEBV, $v > x$ and $u < x$; in both cases x is comprised between u and v, and formula (10) is to be employed.

The function $\mathrm{F}(u, v)$ is continuous in value but not in form.

Fourth Disposition.

This differs from the third only in this—that the velocity u, common to the molecules of the first kind, is in all directions, indifferently, like those of the second; in the same manner, required the number of collisions happening only between molecules of different kinds.

Let us divide the molecules of the first kind into groups of which the velocities correspond to the various elements ω of the typical sphere. On reducing the first kind to a single group, we shall be brought back to the third disposition, except that the number n is to be replaced by $\dfrac{n\omega}{4\pi}$, and the numbers of collisions which were expressed by m, m', will now be expressed by $\dfrac{m\omega}{4\pi}$, $\dfrac{m'\omega}{4\pi}$; adding up these values for all the groups, the factors m, m' remain the same, and we have only to add the values of $\dfrac{\omega}{4\pi}$, which gives the unit. Consequently formulæ

(6), (8), (9), (12) will again give the total number m of collisions between the molecules of different species, and the number m' will be that of the collisions at which one of the new velocities is comprised between x and $x+dx$.

Fifth Disposition.

Let us now pass to the real case, in which the velocities are infinitely variable. Let N be the total number of the molecules of a gas. On making the velocity vary from 0 to infinity by equal small increments, the molecules will be divided into a like number of groups; and we will designate by $N\phi(x)dx$ the numbers of those whose velocity at the commencement of the time t is comprised between x and $x+dx$. The collisions against the sides do not alter the velocity; but if the whole of the velocities of all the molecules together be considered, every collision between these has the effect of suppressing two velocities and creating out of them two others. The number of of velocities comprised between x and $x+dx$ which are suppressed during the time t is that of the collisions which have happened between the single group numbering $N\phi(x)dx$, on the one hand, and all the groups, on the other. It will therefore be the sum of the values of m, on replacing therein n, n' by $N\phi(x)dx$, $N\phi(v)dv$, u by x, and making v to vary from 0 to infinity. According to formulæ (6) and (8) this sum will be $N^2at\psi(x)dx$, putting

$$\psi(x)=\phi(x)\int_0^\infty f(x,v)\phi(v)dv. \quad . \quad . \quad . \quad (13)$$

As to the velocities comprised between x and $x+dx$ which are created during the time t, they may proceed from collisions happening between any two groups whatever, corresponding to the velocities u and $u+du$, v and $v+dv$. The number sought will therefore be obtained by replacing in expression (12) n and n' by $N\phi(u)du$, $N\phi(v)dv$, then adding those values of the result which correspond to all those of u and v. In this way, however, every possible association of two groups will be found to have been reckoned twice; so that half of the sum must be taken. According to formulæ (6) and (12) the number sought will be

$$N^2at\psi'(x)dx,$$

putting

$$\psi'(x)=2x\int_0^\infty\int_0^\infty \mathrm{F}(u,v)\phi(u)\phi(v)\,du\,dv. \quad . \quad (14)$$

The number of the velocities comprised between x and $x+dx$ will constantly increase or decrease until the instant when an

equal quantity is suppressed and created during the time t; this condition, which characterizes the final state, amounts to $\psi(x)=\psi'(x)$. The function $\phi(x)$ is to be determined so as to satisfy the preceding equation for every value of x.

The unit of length, at first chosen arbitrarily, must now be reduced to one metre. The alteration thence resulting in the formulæ is the same as if we had supposed the enclosure a cubic metre; also, whatever this enclosure may be, it amounts to taking for N the number of molecules contained in a cubic metre.

Accessory Conditions.

The number of molecules of a group being $N\phi(x)dx$, on adding it for all the groups, N will be found, whence it follows that

$$\int_0^\infty \phi(x)dx=1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Another datum is furnished by the value of the pressure; this for a portion of side S of 1 square metre has for its value $p=\Sigma f$, the summation extending to all the molecules of the medium, f being for each its normal action upon S. But, these actions being intermittent, it is preferable to replace it by its mean value during a time t, this being short enough for the passage of a molecule during it to be inconsiderable in comparison with the dimensions of the enclosure. We thus get

$$p=\frac{1}{t}\Sigma\int_0^t f dt.$$

The quantity $\int f\,dt$ does not differ from 0 for a molecule unless it strikes the side during the time t; and in that case it measures the impulse given by the side to the molecule—that is, $2mv$, v being the normal component of its velocity. Consequently

$$p=\frac{2m}{t}\Sigma v,$$

the sum Σ extending to all the impacts against the side S during the time t.

Case I. Let us suppose that all the molecules have velocities h, equal and parallel, carrying them towards the side, and making with the normal to this an acute angle θ. In this case, for a molecule to produce a collision it must at the commencement of the time t be within a prism having S for its base,

and its edges of the length ht parallel to the velocities: its volume being $ht\cos\theta$, the number of the molecules included in it is $Nht\cos\theta$; and this is also the number of the collisions. Moreover $v=h\cos\theta$, whence results

$$p=2mNh^2\cos^2\theta.$$

Case II. The molecules have one and the same velocity h, in all directions, indifferently.

They will be divided into groups, of which the velocities correspond to the various elements ω of a typical sphere; and we shall have to replace N by $N\frac{\omega}{4\pi}$, taking for ω a thin zone parallel to the side. We shall have, as we have seen, $\omega=2\pi\sin\theta d\theta$; and on integration from 0 to $\frac{1}{2}\pi$, reckoning only the molecules moving towards the side, we get

$$p=mNh^2\int_0^{\frac{\pi}{2}}\cos^2\theta\,.\sin\theta\,.\,d\theta=\tfrac{1}{3}mNh^2;$$

a known formula, often deduced from other considerations, and from which is derived the numerical value of h.

Case III. By taking account of the inequalities of velocity, the molecules will be divided into groups. The number of those of one and the same group in the unit of volume is $N\phi(x)dx$, which will have to be substituted for N; and on adding the result for all the groups, we get

$$p=\tfrac{1}{3}mN\int_0^{\infty}x^2\phi(x)dx.$$

Hence results

$$\int_0^{\infty}x^2\phi(x)dx=h^2. \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Solution of the Equation $\psi(x)=\psi'(x)$.

According to formula (9), the function $f(u, v)$ having two distinct forms, the integral (13) is to be divided into two others, which gives

$$\phi(x)=\phi(x)\int_0^{x}\left(x+\frac{v^2}{3x}\right)\phi(v)dv+\phi(x)\int_x^{\infty}\left(v+\frac{x^2}{3v}\right)\phi(v)dv.$$

In the same way, in the value (14) of $\psi'(x)$, $du\,dv$ can be regarded as the surface-element of the plane, and the integral can be divided into several others corresponding to the different regions of the figure. According to the values (12) of

$\mathrm{F}(u, v)$, we shall thus get

$$\psi'(x)=2x\int_0^x dv\int_x^\infty \frac{\phi(u)\phi(v)}{u}\,du+2x\int_0^x du\int_x^\infty \frac{\phi(u)\phi(v)}{v}\,dv$$
$$+2x^2\int_x^\infty\int_x^\infty \frac{\phi(u)\phi(v)}{uv}\,du\,dv+2x\lambda;$$

where λ corresponds to the segment ACBE, so that we have

$$\lambda=\int_0^x \frac{\phi(u)du}{u}\,\mathrm{F}'(u),\quad \mathrm{F}'(u)=\int_{\sqrt{x^2-u^2}}^x \frac{\sqrt{u^2+v^2-x^2}}{v}\,\phi(v)dv). \quad (17)$$

The other parts of the value of $\psi'(x)$, two of which are equal, decompose into products. Putting

$$f=\int_x^\infty \frac{\phi(v)dv}{v}=\int_x^\infty \frac{\phi(u)du}{u},\quad f'=\int_x^\infty v\phi(v)dv,$$
$$g=\int_0^x \phi(v)dv=\int_0^x \phi(u)du,\quad g'=\int_0^x v^2\phi(v)dv,$$

we get

$$\left.\begin{aligned}\psi(x)&=\left[xg+\frac{1}{3x}g'+f'+\frac{x^2}{3}f\right]\phi(x),\\ \psi'(x)&=2x\lambda+4xfg+2x^2f^2.\end{aligned}\right\}\quad . \quad . \quad (18)$$

The first process which presents itself for finding $\phi(x)$ consists in assigning to it the form of a series $a_1x+a_2x^2+a_3x^3+\ldots$; we must then put

$$\int_0^\infty \frac{\phi(v)dv}{v}=k,\quad f=k-\int_0^x \frac{\phi(v)dv}{v};$$
$$\int_0^\infty v\phi(v)dv=k',\quad f'=k'-\int_0^x v\phi(v)dv.$$

$\psi(x)$. $\psi'(x)$ will thus contain, besides the constants k, k', only finite integrals. The value of λ, on replacing in it for the integration u and v by $u'x$, $v'x$, will be expanded into a series, the coefficients containing certain definite integrals easily reducible to one another. It is needless to give the development of this calculation, which is very complicated; it is only necessary to remark that the coefficients a_1, a_2, a_3, &c. are all expressed as functions of k and k'. Consequently, if a function is expansible according to the powers of x, contains two indeterminate parameters, and, when taken for $\phi(x)$, satisfies the equation $\psi(x)=\psi'(x)$, that will be the solution, and completely so, especially if it is expansible into

a convergent series for every value of x. The value of $\phi(x)$ which satisfies all these conditions is the following:—

$$\phi(x)=\alpha x^2 e^{-\beta x^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

e being the base of the Napierian logarithms, α and β two positive arbitrary constants. It is expansible into series for all the values of x; and, according to what we have just seen, to verify that it satisfies the equation $\psi(x)=\psi'(x)$ suffices: to suppose $\alpha=1$ will be sufficient; the common factor α^2 vanishes everywhere. The values of f &c. then become

$$g=\int_0^x v^2 e^{-\beta v^2}\,dv, \quad g'=\int_0^x v^4 e^{-\beta v^2}\,dv$$

(which cannot be found in a finite form), then

$$f=\int_x^\infty e^{-\beta v^2} v\,dv=\frac{1}{2\beta}e^{-\beta x^2}, \quad f''=\int_x^\infty e^{-\beta v^2} v\,dv\times v^2;$$

for f'' the indefinite partial integral is

$$-\frac{v^2}{2\beta}\epsilon^{-\beta v^2}+\frac{1}{\beta}\int e^{-\beta v^2} v\,dv,$$

or between the limits

$$f''=\frac{x^2}{2\beta}e^{-\beta x^2}+\frac{f}{\beta}=\left(\frac{x^2}{2\beta}+\frac{1}{2\beta^2}\right)e^{-\beta x^2}.$$

On substituting these values of f, f'', formulæ (18) become

$$\psi(x)=x^2 e^{-\beta x^2}\left[xg+\frac{g'}{3x}+\left(\frac{2x^2}{3\beta}+\frac{1}{2\beta^2}\right)e^{-\beta x^2}\right],$$

$$\psi'(x)=2x\lambda+\frac{2x}{\beta}e^{-\beta x^2}g+\frac{x^2}{2\beta^2}e^{-2\beta^2};$$

and on putting

$$e^{\beta x^2}[\psi(x)-\psi'(x)]=\psi''(x),$$

we get

$$\psi''(x)=\left(x^3-\frac{2x}{\beta}\right)g+\frac{x}{3}g'+\frac{2x^4}{3\beta}e^{-\beta x^2}-2x\lambda e^{\beta x^2}.$$

Then formula (17) becomes

$$\lambda=\int_0^x ue^{-\beta u^2}\mathrm{F}'(u)du, \quad \mathrm{F}'(u)=\int_{\sqrt{x^2-u^2}}^x \psi'(\sqrt{u^2+v^2-x^2})e^{-\beta v^2}v\,dv.$$

Taking for the variable

$$\sqrt{u^2+v^2-x^2}=y, \text{ whence } v^2=x^2-u^2+y^2, \; v\,dv=y\,dy,$$

we get

$$F'(u)=e^{\beta(u^2-x^2)}\int_0^u y^2dy\,e^{-\beta y^2},\quad \lambda=e^{-\beta x^2}\int_0^x u\,du\,F''(u),$$

putting

$$F''(u)\int_0^u y^2dy\,e^{-\beta y^2}.$$

The indefinite partial integral is

$$\lambda e^{\beta x^2}=\tfrac{1}{2}u^2F''(u)-\tfrac{1}{2}\int u^4e^{-\beta u^2}du;$$

and between the limits there results

$$\lambda e^{\beta x^2}=\tfrac{1}{2}x^2g-\tfrac{1}{2}g'.$$

On substituting in the above value of $\psi''(x)$ we have

$$\psi''(x)=\tfrac{4}{3}xg'-\frac{2x}{\beta}g+\frac{2x^4}{3\beta}e^{-\beta x^2}.$$

Finally, partial indefinite integration gives

$$g'=-\frac{v^3}{2\beta}e^{-\beta v^2}+\frac{3}{2\beta}\int v^2e^{-\beta v^2}dv,$$

or, between the limits,

$$g'=-\frac{x^3}{2\beta}e^{-\beta x^2}+\frac{3}{2\beta}g,$$

whence results identically

$$\psi''(x)=0,\text{ and consequently }\psi(x)=\psi'(x).$$

Formula (19) therefore well represents the law of the velocities; and α, β will be found on substituting it in equations (15) and (16), which gives

$$\alpha\int_0^\infty x^2e^{-\beta x^2}dx=1,\quad \alpha\int_0^\infty x^4e^{-\beta x^2}=h^2;$$

it will, however, be simpler to employ the number h, putting

$$h^2=\tfrac{3}{2}h'^2,\text{ or }h'=h\sqrt{\tfrac{2}{3}}=0{\cdot}82h,$$

The known integral

$$\int_0^\infty e^{-x^2}dx=\frac{\sqrt{\pi}}{2}$$

gives, on replacing x by $x\sqrt{\beta}$ and differentiating twice with respect to β,

$$\int_0^\infty e^{-\beta x^2}dx=\frac{\sqrt{\pi}}{2\sqrt{\beta}},\quad \int_0^\infty x^2e^{-\beta x^2}dx=\frac{\sqrt{\pi}}{4\beta^{\frac{3}{2}}},\quad \int_0^\infty x^4e^{-\beta x^2}dx=\frac{3\sqrt{\pi}}{8\beta^{\frac{5}{2}}};$$

the above relations thus become

$$\frac{\alpha\sqrt{\pi}}{4\beta\sqrt{\beta}}=1,\quad \frac{3\alpha\sqrt{\pi}}{8\beta^2\sqrt{\beta}}=\tfrac{3}{2}h'^2,\text{ whence }\beta=\frac{1}{h'^2},\quad \alpha=\frac{\alpha'}{h'^3},$$

if we put

$$\alpha' = \frac{4}{\sqrt{\pi}}.$$

Therefore the law of the velocities is

$$\phi(x) = \frac{\alpha'}{h'^3} x^2 e^{-\frac{x^2}{h'^2}}.$$

The maximum of this expression corresponds to $x = h'$. It seems as if the velocities for certain groups might become indefinitely great; but this is only apparent. Indeed, let μ be the proportion of the molecules whose velocity exceeds nh', n being a number containing several units, we shall have, on changing x into $h'y$,

$$\mu = \int_{nh'}^{\infty} \phi(x)dx = \alpha' \int_{n}^{\infty} y^2 e^{-y^2} dy,$$

or on partial integration

$$\frac{\mu}{\alpha'} = \tfrac{1}{2} n e^{-n^2} + \tfrac{1}{2} \int_{n}^{\infty} e^{-y^2} dy,$$

or

$$\int_{n}^{\infty} e^{-y^2} dy = \int_{n}^{\infty} \frac{1}{y} e^{-y^2} y\, dy < \int_{n}^{\infty} \frac{1}{n} e^{-y^2} y\, dy;$$

consequently

$$\mu < \alpha' \left(\tfrac{1}{2} n + \frac{1}{2n} \right) e^{-n^2}.$$

For $n = 10$ or $x > 8h$, about, this view of μ only commences at the 43rd decimal; and the theory, established for numerous groups, cannot be extended to some singular cases in which only one or two molecules, perhaps, would be concerned. The high velocities can only be brought about by a series of collisions favourably arranged by chance. It is quite otherwise with very low velocities, which a single collision can produce.

The proportion μ' of the velocities below a certain value of x is $\mu' = \int_{0}^{x} \phi(x)dx$; and if $\frac{x}{h'}$ is small, the exponential can be replaced by unity, which gives

$$\mu' = \frac{\alpha'}{h'^3} \int_{0}^{\infty} x^2 dx = \frac{\alpha'}{3} \left(\frac{x}{h'} \right)^3.$$

For example, the proportion of the velocities below $\frac{h'}{10}$ is about 0·00075.

VII. *The Beats of Mistuned Consonances.*

By Professor Silvanus P. Thompson, *B.A.*, *D.Sc.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a letter published in your December Number Mr. Bosanquet laments that his labours upon the beats of mistuned consonances have been thrown away upon me, because I have ventured to criticise two points in his valuable and interesting researches.

These points are, first, his misuse of the word "subjective" as applied to the phenomena in question; and, second, the priority of Dr. König in the analysis of the phenomenon so far as it can yet be said to be analyzed, to the credit of which Mr. Bosanquet lays a direct claim.

I will deal with Mr. Bosanquet's objections separately, merely observing that it would indeed be a pity if "the credit" of any discovery to which Mr. Bosanquet is entitled could be "taken away by a careless sentence" of mine.

Mr. Bosanquet's attempt to defend his use of the word "subjective," as applied to the difference-tones or beat-notes, is virtually an admission of the very point for which I contend. By way of defence, he retires behind the shelter of Helmholtz and of Mr. Ellis, who, it appears, have lapsed into the same laxity of language. On reading Mr. Bosanquet's reference to Helmholtz, I immediately turned to p. 234 of Ellis's translation (the only edition at hand at the moment), and found that I had underlined the word in pencil, and had added in the margin "*sic*" by way of protest. But whether Mr. Bosanquet sins in good company or not, I am still of opinion that it is unwarrantable to apply the term "subjective" to that which has a physical existence independently of the conscious "ego," or of the brain through which consciousness works. The ear is, as I have said elsewhere, only a complex resonator; and any other suitable resonator will also respond to the tones in question, provided it possesses the asymmetry of its parts which is necessary for the development of those tones. To speak of such tones as subjective is a palpable abuse of the term. But I really do not require to insist further on this point if, as Mr. Bosanquet says, he does not believe "*much*" in the "advancement of clearness by the employment of words very strictly defined."

Secondly, as to König's priority in the observation that the beats of imperfect consonances consist of variations in the intensity of the *lower* of the two tones. This observation Mr. Bosanquet says was "unquestionably" first obtained by him-

self. For Mr. Bosanquet's sake I regret that the tone of his letter leaves me no alternative but to produce the evidence upon which I deliberately assigned priority to Dr. König.

Mr. Bosanquet appears to have read König's papers; for he says he "knew" them "intimately," and that "there was no vestige of guidance in them on the subject;" that "König never analyzed his beats, and never enunciated any opinion whatever as to the notes of whose variation the beats consist, except in the one case of the octave . . . . where he says that fundamental and octave are heard alternately."

There lie before me at this moment the *Annalen* of Poggendorff for the year 1876 (Bd. clvii.) It is true that König states therein that the two primaries are alternately heard; but that he traced this alternation to its true cause is shown by the very paragraph in which this statement occurs. I quote from p. 188 of the volume of the *Annalen*, italicizing the words which Mr. Bosanquet appears to have overlooked when he says that König did not analyze or enunciate any opinion, and that König only touched the case of the octave.

"Eine Eigenthümlichkeit der Stösse harmonischer Intervalle besteht noch darin, das die beiden primären Töne abwechselnd hervortreten. Lässt man neben dem starken, grossen *C* das nur um einen geringen Theil einer Schwingung verstimmte kleine *c* ertönen, so dass sich sehr langsame Stösse bilden, so hört man abwechselnd einmal den Grundton und einmal die Octave so deutlich hervortreten, dass man, wenn das kleine *c* sehr stark ist, mitunter geneigt seyn könnte, jede Schwebung doppelt zu zählen. *Ist das kleine* c *dagegen schwach, so hört man nur den Grundton abwechselnd stärker und schwächer werden.* Ganz dieselbe Beobachtung habe ich auch bei den sehr langsamen Stössen der *Duodecime*, und der *Doppeloctave*, *C*: *g* und *C*: *c'* machen können, aber bei nur einigermassen schnellen Schwebungen lässt sich das periodische Hervortreten des höheren Tones nicht mehr wahrnehmen."

It is the more remarkable that König's insight into the cause of the alternation mentioned in the first sentence of the paragraph quoted above should have escaped Mr. Bosanquet's notice, since three months before the publication of Mr. Bosanquet's paper in the Philosophical Magazine König had published another very important paper in Wiedemann's *Annalen* (Bd. xii. 1881, p. 337), in which the results of a reexamination of the very point in question are most unequivocally laid down. I quote the passage from p. 341, again italicizing the important words.

"Regulirt man ferner die Intensität der beiden Töne eines alterirten harmonischen Intervalls in der Weise, dass die Stösse am deutlichsten hervortreten, so müssten, wenn sie durch den höheren primären Ton und einen im Ohre erzeugten harmonischen Oberton

des tieferen, also durch zwei Töne im Einklang gebildet wären, diese Stösse auch auf diesen Oberton gehört werden, man nimmt aber gerade das Gegentheil wahr, d. h. das *der Grundton seine Intensität periodisch ändert und dabei nur in den Momenten grösster Schwächung den höheren Ton hervortreten lässt, wie ich früher beschrieben.*"

With Mr. Bosanquet's permission, I will translate the words in which Dr. König states what he observes, namely that "*the fundamental tone alters its intensity periodically, and thereby allows the higher tone to come out only in the moments of greatest enfeeblement, as I formerly described.*

I will leave to your readers to decide whether the alleged injustice of my "careless sentence" is objective or subjective in character; or whether credit for discovery is one of those terms in the strict definition of which we are not to believe "much." I remain, Gentlemen,

Yours faithfully.

University College, Bristol,
December 19th, 1881.

SILVANUS P. THOMPSON.

VIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xii. p. 75.]

November 2, 1881.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Genus *Stoliczkaria*, Dunc., and its Distinctness from *Parkeria*, Carp. and Brady." By Prof. P. Martin Duncan, M.B. Lond., F.R.S., F.G.S., Pres. R.M.S.

2. "On the Elasticity- and Strength-constants of Japanese Rocks." By Thomas Gray, Esq., B.Sc., F.R.S.E., and John Milne, Esq., F.G.S.

In this paper the authors described the results of some experiments made to determine the elasticity-constants and strength against rupture and crushing of a few of the commoner Japanese rocks, their chief object being to obtain data for calculating the theoretical velocities of earthquake-wave transmission. The rocks submitted to experiment were a grey granite, a pure white crystalline marble, a greyish-green soft tuff, a mottled clay-rock, and clay-slate.

Young's moduli were determined by the bending of solid cylinders of the rocks in an apparatus described and figured; the deviations produced were read by means of the reflection from a mirror, which magnified them more than 200 times. The process for determining the rigidity was also described and illustrated by a figure; and the experiments in crushing were made upon columns of stone by means of a Bramah press. In experiments on the

rupture of the columns there was no marked deviation from the proportionality of strain to stress up to the breaking-point, except in the case of the marble. In crushing, the authors obtained considerably lower moduli than those quoted in tables for similar rocks; and as their experiments were performed upon columns about three times as long as their diameter, they repeated them upon columns of marble varying in length from one half to six times the thickness. The results seemed to show that the short specimens were the weakest; but there was little difference. The authors give the formulæ by which they worked out the results of each series of experiments, and bring together the mean results in a tabular form.

3. "The Glacial Deposits of West Cumberland." By J. D. Kendall, Esq., C.E., F.G.S.

The author gave a brief sketch of the physical geography of the district and of the distribution of the more remarkable and easily recognized varieties of rock. The glacial deposits, viz. Boulder-clays, sands, and gravels, occupy mainly the area of low ground skirting the hills, extending upward to a height of about 500 feet above the sea: above that contour-line they only occur in isolated patches or tongue-like prolongations up valleys to elevations occasionally of about 1000 feet. The deposits, where fully developed, consist of Upper Boulder-clay, Middle Sand and Gravel, and Lower Boulder-clay; together they sometimes attain a thickness of from 100 to about 130 feet. Certain peculiarities in the distribution of the deposits were described. The tripartite arrangement never occurs in the valleys in the mountainous district. Boulder-clays, indeed, sometimes occur here; but sands are more common. The distribution of the boulders from the more remarkable rocks was described; tables of these were given, as also of the maximum height above the sea at which each occurs. The origin of the deposits was next discussed. The author is of opinion that the presence in the Lower Boulder-clay of boulders derived from such widely different sources can only be explained by floating ice, but that the correspondence of the materials of the clay with rocks in the vicinity shows that glacier-mud produced the finer elements. The Middle Sand and Gravel he considers due to denudation of the above materials during a period of emergence. The Upper Boulder-clay he attributes to a second period of submergence corresponding generally in its conditions with the former one. The gravel mounds are probably caused by the stranding of bergs at the end of this period.

November 16.—R. Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. Additional Evidence on the Land Plants from the Pen-y-glog Slate-quarry, near Corwen." By Henry Hicks, Esq., M.D., F.G.S.

2. "Notes on *Prototaxites* and *Pachytheca* from the Denbighshire Grits of Corwen, North Wales." By Principal Dawson, LL.D., F.R.S., F.G.S.

IX. *Intelligence and Miscellaneous Articles.*

ON THE DETERMINATION OF THE ILLUMINATING-POWER OF THE SIMPLE RADIATIONS. BY MM. A. CROVA AND LAGARDE.

ONE of the most delicate problems of photometry is the measurement of the illuminating-power of lights of different tints. One of us has already indicated* how this question might be solved, by employing a spectrophotometer. That solution supposes that the coefficients of illumination of the different simple radiations composing the light which is to be compared are known.

If the determination of the radiant energy of a vibration of a determined wave-length can be expressed exactly in thermal or mechanical units, that of its illuminating-power admits of all the uncertainties inherent to the measurement of physiological sensations variable with each eye. There exists no known relation between these two quantities; we have therefore to commence by seeking out what are, for a determined eye, the illuminating-powers of the various simple radiations of the normal spectrum of two sources of light—the sun, and the Carcel standard.

The best-known treatise on this subject is that of Fraunhofer† on the Sun; the results obtained by him are not very concordant. The following is, briefly, the method which we have followed.

The illuminating-power of a simple light may be regarded as the property possessed by the latter of rendering distinguishable, upon a white screen illumined by it, minute details (lines, characters); it will be possible to measure them approximately, as several physicists have done, by weakening this light until the characters can no longer be distinguished, and taking the ratio of the initial intensity to this limit of intensity. The absolute value of these numbers will vary with the fineness of the characters; but their ratio will be sensibly constant, and will depend only on the wave-length of the light examined.

The light to be studied (sun or Carcel standard) is received perpendicularly upon the slit of a spectrophotometer covered with a strip of glass on which is photographed a series of very fine and very close dividing-lines; the direction of these lines cuts the slit normally. A pure spectrum is then seen furrowed by a considerable number of very fine longitudinal striæ. If the ocular slit of the telescope be brought onto a region of the spectrum, the simple radiations comprised between two very close known limits are isolated; and by a suitable rotation of the nicol, their intensity is weakened until the striæ cease to be perceptible. The phenomenon of the disappearance of the striæ is more delicate than one would have at first been inclined to believe; by practice one arrives at being certain about the degree or the fraction of a degree, according to the region of the spectrum.

1. We have traced the curve of the wave-lengths as functions of the divisions of the micrometer, and calculated its equation by

* *Comptes Rendus*, t. xciii. p. 512; Phil. Mag Dec. 1881, xii. p. 445.

† Gilbert's *Annalen*, xxvi. p. 297 (1817).

the method of least squares. The derivate of that equation gives, for each wave-length, the factor by which the intensities of the prismatic spectrum must be multiplied to reduce them to those of the normal spectrum.

2. We have measured for wave-lengths comprised between 480 and 740, at intervals of 20 units, the rotations which cause the vanishing of the striæ.

3. The variations of the Carcel standard are corrected so as to reduce the intensity to the constant value corresponding to the consumption of 42 grams per hour; for this we made use of M. Deleuil's automatic balance, which registers, during the time of the experiments, the successive times required for burning 10 grams of oil. The solar light was diffused by a wooden screen whitened with carbonate of magnesia. We operated when the sky was very clear, about noon.

4. The prismatic curves have been reduced, as we have said, to the normal spectrum; and, lastly, the absolute maximum being set off upon the curve, we have reduced the ordinates to those corresponding to a maximum equal to 100.

The curves obtained are tangents at their two extremities to the axis of the wave-lengths; they rise at first slowly, afterwards very quickly in the vicinity of the maximum. For the lamp, the curve is nearly symmetrical on both sides of the maximum. For the sun, the ascent and descent are more rapid than for the lamp; the descent towards the violet is more rapid than the ascent on the red side.

The following results are deduced from the two most regular series, selected from amongst numerous determinations:—

Wave-length		740	720	700	680	660	640	620	600
Illuminating-powers	Lamp	0·1	0·7	1·6	5·7	14	28	52·5	94
	Sun	...	...	...	0·5	1·5	4	10·2	23

Wave-length		580	560	540	520	500	480
Illuminating-powers	Lamp	72·5	37·5	23·5	13·0	6·0	1·0
	Sun	62·5	98·5	30·5	17·2	9·2	3·5

The maximum, =100, corresponds, for the lamp, to the radiation 592, and for the sun to 564.

The discussion of these results leads to conclusions which are in accordance with the theory of the emission of the radiations emitted by incandescent bodies *.

The numbers obtained require a slight correction, on account of the absorption exerted towards the violet by the material of the prisms. We purpose to continue these determinations with prisms exerting no appreciable absorption upon the visible radiations.—*Comptes Rendus de l'Académie des Sciences*, Dec. 5, 1881, t. xciii. pp. 959–961.

* *Journal de Physique*, viii. p. 357.

THE PHOTODYNAMIC PARABOLOID.

BY PLINY EARLE CHASE, LL.D.

In the Philosophical Magazine for September 1876 I referred to evidences of parabolic influence between α Centauri and Sun. The entire series of paraboloidal abscissas may be found in the Proceedings of the American Philosophical Society for June 17, 1881*.

Newcomb gives estimates of the parallax of α Centauri, which range between $0''{\cdot}48$ and $1''{\cdot}96$. The mean of Henderson's observations in 1832–33, as deduced by himself, was $1''{\cdot}16 \pm {\cdot}11$. Peters, from the same observations, found $1''{\cdot}14 \pm {\cdot}11$. Henderson obtained $0''{\cdot}913$ from Maclear's observations in 1839–40; Peters, $0''{\cdot}967 \pm {\cdot}064$ from the same; Maclear, $0''{\cdot}919 \pm {\cdot}034$ from declinations in 1842, 1844, and 1848; Moesta, $0''{\cdot}880 \pm {\cdot}068$ from declinations in 1860–64. There is therefore an uncertainty as to the actual distance, of the same order of magnitude as planetary eccentricities. If this fact should be thought to diminish the probability of a kinetic bond between the photodynamic paraboloid and the fixed stars, it will be well to bear in mind the following considerations :—

1. If there is an all-pervading interstellar medium, which is both material and elastic, all its persistent oscillations *must be* cyclically harmonic in some shape or other.

2. All such permanent rhythmical oscillations must be dependent upon or associated with permanent masses and velocities.

3. The mass and velocity from which the paraboloidal abscissas were deduced are Sun's mass and the velocity of light.

4. The coordinates indicate a solar motion in space which accords closely with Herschel's estimated velocity †.

5. The abscissas touch regions of incipient subsidence, which explain the formation of the several planetary belts, in accordance with Herschel's interpretation of the nebular hypothesis.

6. The abscissas are manifoldly grouped in ways that are phyllotactically and otherwise harmonically symmetrical, as might be looked for in a medium like the supposed luminiferous æther.

7. The last phyllotactic abscissa, A_{38}, is a fourth proportional to Sun's semidiameter, Laplace's limit of synchronous solar rotation and revolution, and the modulus of light at Sun's surface.

8. The paraboloid fixes Sun's position relatively to some other important stars in the Milky Way.

9. These are the most far-reaching indications of an unbroken chain of kinetic influences that have ever been published.

10. Being based upon the greatest mass and the greatest intercosmical velocity of which we have any measurable knowledge, the law of parsimony gives an *à priori* presumption that the chain may extend to other masses of a like order of magnitude with Sun.

11. The next abscissa to the solar phyllotactic series, A_{39}, is in

* Vol. xix. pp. 447, 448.

† Proc. Amer. Phil. Soc., Nov. 4, 1881.

the region of the fixed stars, its locus being (within the limits of probable error) the same as that of α Centauri.

12. The accordance of the terminal locus with that of α Centauri *may be* exact; it is almost impossible that the deviation from precise accordance can be so great as 8 per cent.; and such a deviation could be easily explained by stellar orbital motions.

13. The second stellar abscissa, A_{10}, indicates a distance corresponding to Bessel's estimate of the parallax of 61 Cygni.

14. Whatever may be thought of the last three indications, the first ten are clear, unmistakable, and incontrovertible.

Haverford College, Nov. 15, 1881.

ON THE ACTION OF COLD UPON THE GALVANIC ARC.

BY D. TOMMASI.

When the galvanic arc springs between two metallic rheophores, of copper for instance, formed each of a tube bent in the shape of a U, through which a rapid stream of cold water flows, and placed horizontally one opposite to the other, the following facts are observed :—

(1) The illuminating-power of the arc is considerably weakened; it is reduced, so to say, to a mere luminous point, even when a very intense electric current is employed (50–75 Bunsen elements, large size).

(2) The arc, if it can be called so, is very unstable; the slightest breath suffices to extinguish it*.

(3) If a sheet of paper be placed over the arc, at a distance of 4 or 5 millim., after a few moments a black point is seen to be produced, which extends and is at last perforated; but the paper does not take fire.

(4) The arc is constituted by a luminous globule moving up and down between the two rheophores. The form of this globule, as well as its extreme mobility, causes it to resemble very much a drop of liquid in the spheroidal state.

(5) If the south pole of a magnetized bar be brought near to the galvanic arc, the arc is attracted and approaches the magnet so that at length it leaves the rheophores and is extinguished. The same phenomenon is observed, but in a contrary direction, on bringing the north pole of a magnet towards the arc.

(6) The amount of ozone seems to be greater than when the arc is not cooled.

It is to be remarked that, in spite of the cooling of the rheophores, the flame of the arc is slightly green, proving that a portion of the copper burns. Hence one is entitled to ask if the arc would be produced by taking as rheophores two platinum tubes in which alcohol cooled to 30° C., for example, was caused to circulate.—*Comptes Rendus de l'Académie des Sciences*, Nov. 7, 1881, t. xciii. p. 716.

* The instability is such that it cannot ignite a match without being extinguished itself.

ON THE VARIATIONS OF THE RESISTANCE OF ELECTRICAL MACHINES WITH THEIR VELOCITY. BY M. E. LACOINE.

It is known that the resistance of magneto- and dynamoelectrical machines is not constant, but increases with their velocity. The experiments of which I am about to give an account to the Academy afford ground for thinking that these variations of the resistance are to be explained by those of the contact between the movable commutator and the rubber-springs.

I employed a copper cylinder 5 centim. in diameter, with longitudinal grooves resembling the insulating ones of a Gramme's commutator. Upon this cylinder, at two diametrically opposite points, rub two steel springs which form the terminations of the two extremities of a circuit comprising a battery, a telephone, and a galvanometer to measure the resistances. The resistance of the circuit was :—

	ohms.
At rest	68
With the velocity of 2000 rotations	183
„ „ 4000 „	900
„ „ about 5000 „	1567
With a very high velocity not measured	2900

The sound given by the telephone was the louder and shriller the greater the velocity.

In this first series of experiments every spring touched the cylinder over a space of three grooves; on making them touch over a greater extent (eight grooves), less sensible variations are obtained.

For each velocity the resistance diminishes when the pressure of the spring is increased. It is possible even to reach a pressure sufficient to suppress all microphonic effect and stop all sound in the telephone.

I have since modified the apparatus, and substituted for the fluted cylinder one of the same diameter but with its surface smooth. I obtained the same effects with a very little higher velocity. Instead of making the springs touch on the cylindrical portion, I caused them to press upon the plane lateral portion; the same results were observed.

The measurements which I have taken give me reason to believe that, for one and the same pressure of the springs, the increase of the resistance is proportional to the cube of the velocity; but it would be necessary to repeat these experiments with means which were wanting to me at Constantinople, in order to be assured of a constant velocity and to measure it exactly.

I will conclude by saying that with the telephone a more intense sound is heard when the intensity of the current is less; hence I conclude that the sound given by a telephone placed in the circuit of a Gramme machine is not due merely to the undulatory variations of the current, but also to the microphonic effect of the commutator.—*Comptes Rendus de l'Académie des Sciences*, December 5, 1881, t. xciii. pp. 958, 959.

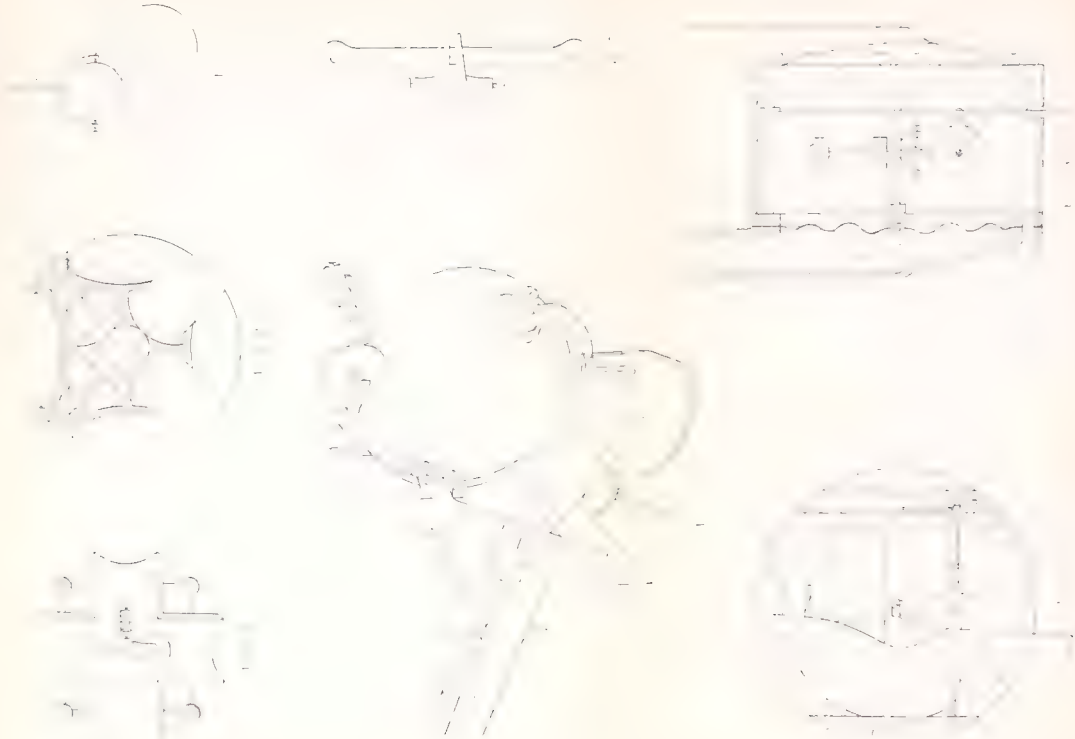


Fig. 4.

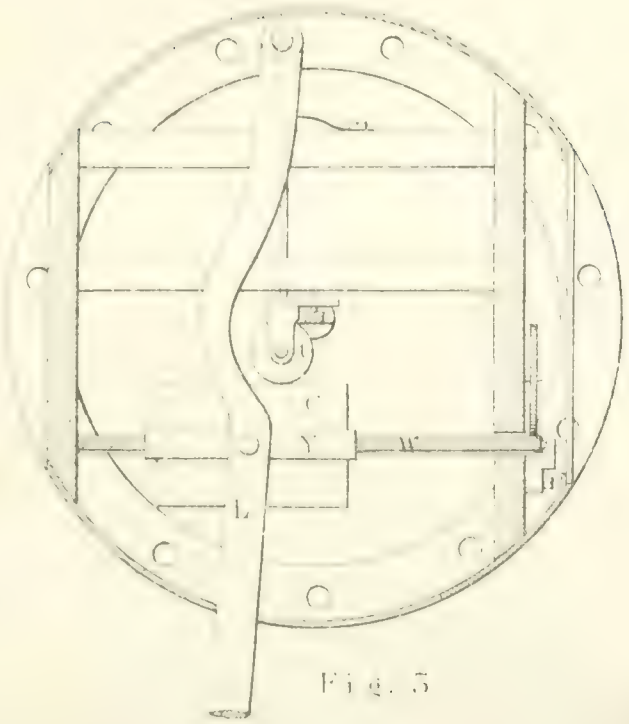


Fig. 5

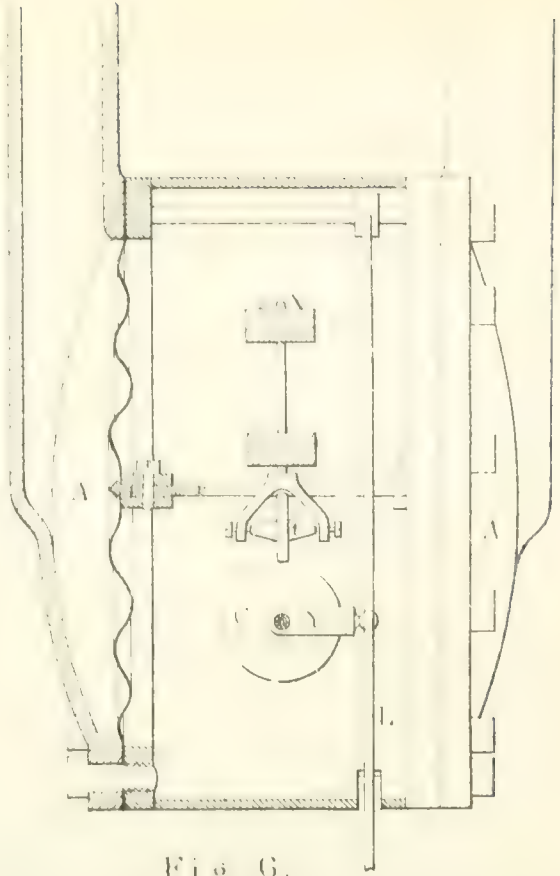
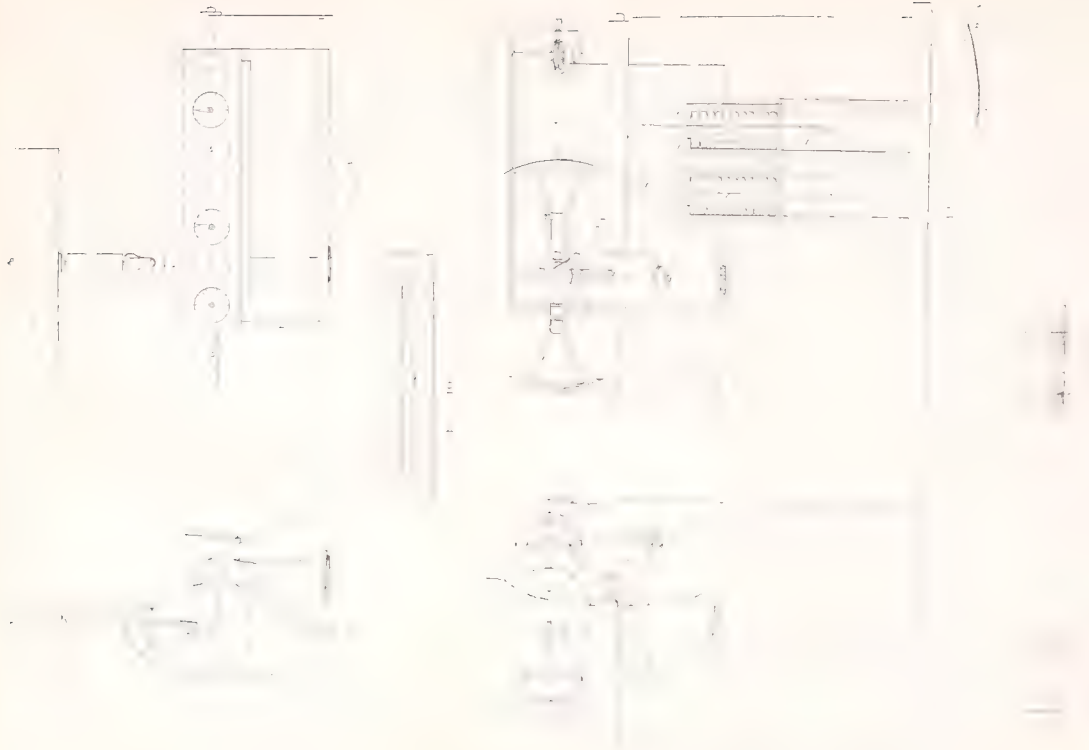


Fig. 6.

Mann... ...ress lith.



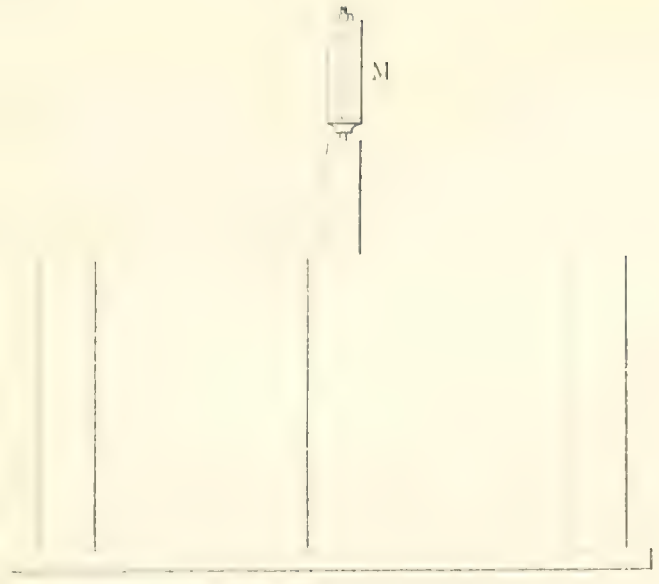


Fig. 11.

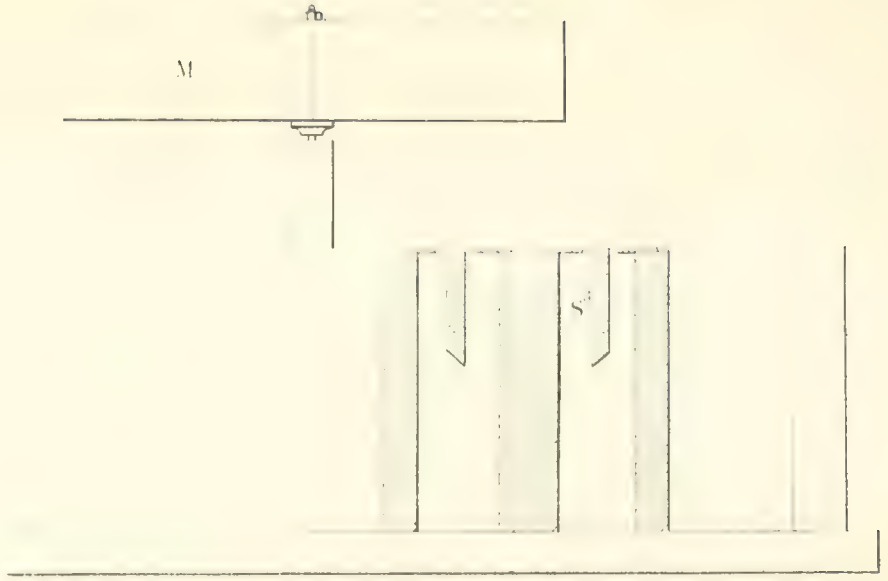


Fig. 12.

Fig. 13.

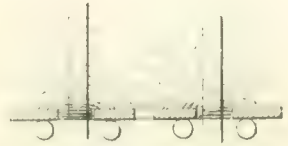


Fig. 14.

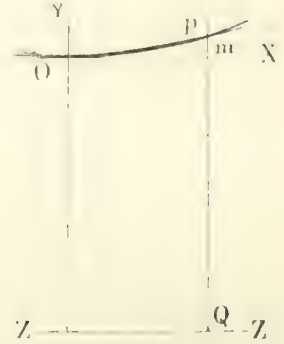


Fig. 15.

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

FEBRUARY 1882.

X. *On Integrating and other Apparatus for the Measurement of Mechanical and Electrical Forces.* By C. Vernon Boys, *A.R.S.M., Demonstrator of Physics in the Normal School of Science, South Kensington**.

[Plates III., IV.]

WHEN in February of this year I described my first integrating-machine† before the Physical Society, I felt that, unless the tangent principle could be so applied as to admit of an indefinite growth of the integral, such principle would be useless for practical purposes. In that machine the integral is determined by the *position* of a cart, and so is limited by the size of the apparatus. Since that time I have devised a variety of methods of applying the tangent principle in which the integral is determined by rotation, and so there is no limit to the extent to which the integral may grow. In the following paper, which is divided into two parts, I have given in the first a description of a variety of integrating-machines, while in the second are some useful applications of the most simple form of integrator described in Part I.

Part I.

At the present time there seem to be three types of integrating-machines: 1st, those that may be called radius

* Communicated by the Physical Society, having been read at the Meeting on November 26, 1881.

† Phil. Mag. May 1881.

ring, the other being an inside ring, the barrels must not be geared together. Fig. 2 is a plan on a larger scale, partly in section, showing how such barrels might be supported. Now, if this outside ring is placed horizontally within the inside ring and touching it at one point, then revolution without rotation of the inside ring will cause rotation without revolution of the outside ring; if, however, in consequence of the inclination of the tangent-wheel, the barrels of the inside ring are caused to rotate, then such rotation will cause revolution of the outside ring; and this revolution will be a true measure of the integral, as the outside ring and the tangent wheel touch barrels of the inside ring at points having the same radius. The astronomical convention with respect to the terms revolution and rotation has been used. By revolution of the ring is meant a turning of the whole round a centre; and by rotation a turning of barrels round their own axes.

Instead of a disk, a sphere similarly mounted may be used for a tangent-wheel, with the same result. Of course the cylinder will be in contact with the sphere at a point on its equator; but if the support of this globe is varied in position, so that the cylinder touches the sphere nearer the poles, then the rate of rotation will depend not only on any former inclination of the plane of the equator of the globe to the axis of the cylinder, but will also be inversely proportional to the cosine of the latitude of the point of contact. The latitude should be brought back to its original value before the rotation of the cylinder is measured. Fig. 3 shows the cylinder in contact with the sphere at a latitude λ. It so happens that the radius in the sphere at the point of contact is equal to $\cos\lambda \times$ the radius of the sphere; but this is not the cause of the introduction of that function, as the rotation of the cylinder is independent of the radius of the tangent-wheel. The true reason can be readily discovered by a simple geometrical construction, which, from the length of this paper, I omit. However, a good illustration may be seen by taking a bicycle and causing it to lean over on its side; then a given twist of the handles will be found to produce a greater deviation in the direction of its motion than would be the case if the bicycle were upright. The effect just described is most easily produced by mounting the cylinder on a rocking-frame, so that it can roll round the ball. Though the axis marked A in the figure remains vertical, yet the effect may be considered as due to a leaning to one side of this axis. If, however, the axis A is caused to lean forwards or backwards, then the rotation of the cylinder, which is still proportional to the tangent of any rotation about A, is also proportional to the sine of the

inclination of A above the horizon; so that if A is horizontal, no rotation about A as an axis will produce any effect; but as A rises higher, increased rotation of the cylinder will be produced, the maximum being when A is vertical. As in the former case, so here, the inclination of A should be brought back to its original value before the rotation of the cylinder is measured. This is the method of steering a wheelbarrow when pushing it. The elevation of the handles corresponds to the inclination of A with the horizon; the equality of the elevation of the two handles corresponds to absence of rotation about A; therefore the barrow goes straight. When turning a corner the outer handle is elevated more than the inner one; this corresponds to rotation about A; and the tangent of this rotation, multiplied by the sine of the elevation of the handles, measures the deviation of the barrow from its straight course. This deviation, then, is greater as the elevation of the handles is greater, and therefore in going round a sharp corner the handles should be raised as much as possible. By the means above described, either the quotient or the product of two functions may be directly integrated.

As in my former integrating-machine, so with these, the reciprocal of a function may be integrated by first inclining the tangent-wheel through a right angle; then, when a function passes through 0 from + to −, the tangent-wheel describes on the surface of the cylinder a cusp showing a momentary infinite motion. A machine so arranged may be used to integrate, not the reciprocal, but the function itself, if, instead of moving the cylinder longitudinally, it is caused to rotate, when the longitudinal motion, or the number of reciprocations if suitable gearing is employed, will measure the integral. In a similar way the machine as first described will integrate a reciprocal.

If for any purpose, in addition to the total result, the integral up to any time is required, a diagram must be drawn. This can be effected by covering the cylinder with a layer of black tracing-paper, and allowing a band of paper as wide as the cylinder is long to pass between the tangent-wheel and the black surface. The length of paper passed through the machine represents the integral; and the curve drawn shows its rate of growth continuously. Should it only be required to know the amount of growth during each of a series of short intervals of time, a narrow band (which is more manageable) may be used wrapped round a small wheel at the end of the cylinder, and so arranged that at the end of each double stroke of the cylinder it is caused to bear against the point of a stationary pencil; then the pencil-marks represent equal intervals

of time, while the distances between them measure the average rate of growth over each interval.

I have at present supposed that the integrating surface is cylindrical; but other surfaces of revolution may be employed for particular purposes. As the rotation of the cylinder depends on the linear motion of its surface, it is clear that its rotation must be inversely proportional to its diameter. If, therefore, instead of a cylinder any other surface of revolution is taken, its rate of rotation will depend not only on the inclination of the tangent-wheel, but also on the radius of contact. The simplest case is that of a disk with the tangent-wheel mounted so as to be capable of radial movement. Then, if the tangent-wheel moves in the direction of its own plane, it will simply describe on the disk a radial line, and there will be no rotation; but if it is inclined at any given angle the disk will rotate, and the rate of its rotation will be proportional to $\frac{1}{r}$, and its whole rotation will be $\int \frac{1}{r}\,dr$, which is $\log r$. Now the tangent-wheel, in its movement outwards, describes on the surface of the disk a spiral which everywhere cuts the radii at the same angle; therefore in such a spiral the angles are the logarithms of the radii; *i. e.* it is the logarithmic spiral If the inclination of the tangent-wheel is made to depend on some function, then such double-disk machine would integrate $\frac{\phi x}{c+x}\,dx$, in which c is the radius of contact when $x=0$.

If the axis of the tangent-wheel is made to pass through a fixed point over the disk removed from its line of travel by a right angle, then the tangent of its inclination to the direction of its motion is proportional to the radius of contact: but, other things being equal, the rotation of the disk is inversely as the radius of contact; therefore the amount of rotation of the disk for a given movement of the tangent-wheel is independent of the radius of contact, and the curve traced out on the disk is the spiral of Archimedes. But if, instead of passing over a disk, the tangent-wheel similarly mounted is made to pass along the surface of a cylinder, then the speed of rotation of the cylinder will be proportional to the distance of the disk from its neutral position, and its whole rotation will be $\int cx\,dx$, or $\frac{c}{2}x^2$, and the curve described on the cylinder will be a parabola. This arrangement of the disk and cylinder may be used, as described on page 84, in a polar planimeter to illustrate the formula $\iint r\,dr\,d\theta$.

After the cylinder and disk, the most simple form for an integrating surface is that of a sphere. Let a sphere be supported, with its axis horizontal, on a frame which can be made to reciprocate about a vertical axis which would, if continued, pass through the centre of the sphere; then, if a tangent-wheel is fixed so as to lie, when its inclination is nothing, in the horizontal plane which passes through the axis of the sphere, angular reciprocation, which must be less than 180°, will cause the tangent-wheel to describe on the sphere a meridian when it is in its neutral position, or a rhumb line if inclined at a constant angle. As the speed of rotation of the sphere is inversely proportional to the radius of contact—that is, to the cosine of the latitude of the point of contact—some means must be adopted whereby the rotation recorded is less than the rotation of the sphere in the same ratio. The most simple plan is to use Amsler's principle, and mount a small sliding and rolling wheel so as to be in contact with the sphere at the highest point on the equator (*i. e.* 90° from the tangent-wheel), but with its plane passing through the centre of the tangent-wheel; then the rotation of the Amsler wheel is always less than the rotation of the sphere, in the same ratio that the rotation of the sphere is too great. Instead of an Amsler wheel, a cylinder capable of moving longitudinally on its horizontal axis, and in contact with the sphere at a point exactly opposite to the tangent-wheel, would, by pure rolling and without any sliding, take off the correct proportion of motion, since it and the tangent-wheel always touch the sphere at points having the same radius.

Fig. 4 is a perspective view of a polar planimeter in which the integration is effected by a disk sphere and Amsler wheel, as described. All the parts marked *a* belong to a rigid frame, which balances on and can turn about a vertical spindle, the top of which is just visible below the tangent-wheel *t*. The vertical spindle is fastened to the stationary wheel *w*, which rests on three feet. The segmental wheel W in gear with *w* is secured to a vertical spindle, the upper end of which carries the crutch C. Screws in the crutch form the horizontal axis about which the sphere S may rotate. The tangent-wheel *t* is mounted in a frame which can be turned about a horizontal axis *e* by means of a lever *l*. The Amsler wheel rests by its weight on the highest point of the equator of the sphere, which is shown dotted. D D is an L-shaped piece, which carries at the angle the pointer P. At the end of the long limb is a slot embracing a pin, as shown. A part of the short limb is made cylindrical; against this part rests the edge of the lever *l*. This edge is not truly radial, but is

laterally displaced from the radial position to an extent equal to the radius of the cylindrical part of D D. This causes the true radius, which is parallel to the axis of the tangent-wheel, to intersect the axis of the cylinder. Now, if the pointer P is moved radially in the slot prepared for it, it is clear that the tangent of the inclination of the tangent-wheel t will be proportional to the square of the distance of P from the vertical axis about which the machine can turn, also that, during any turning of the machine about this axis, the sphere will turn about its vertical axis at a proportionate speed. Now it has been shown that, when the sphere is made to turn about its vertical axis, the rate of rotation of the Amsler wheel is proportional to such rotation multiplied by the tangent of the inclination of the tangent-wheel—that is, in this case to $r^2\,d\theta$. Therefore the whole rotation of the Amsler wheel is a measure of $\int r^2\,d\theta$; and so, if the pointer P is taken round any closed curve, the area of that curve may be read off from the Amsler wheel. The wheel W is three times the radius of w; so that the pointer may, if necessary, be taken completely round the pole, and yet the tangent-wheel will only move 120° on the sphere in latitude. The diameter of the Amsler wheel is one third of that of the sphere, so as to restore the diminished speed. Unlike Amsler's planimeter, this one shows the increment of area for each part of a closed curve, the reason being that it is an exact mechanical equivalent of the polar formula for integration. Though the machine works very well, it cannot be compared to Amsler's as a practically convenient instrument.

An exact mechanical equivalent of the formula $\iint r\,dr\,d\theta$ would be produced by retaining all the last machine, except the short limb of the L-shaped piece D D, and mounting on the long limb a tangent-wheel to traverse a cylinder, the rate of rotation of which for a given radial movement of the pointer would be proportional to the distance of the pointer from the pole—that is, to $r\,dr$—and the whole rotation would be $\int r\,dr$. Now, if the cylinder were by its rotation caused to change the inclination of the lever l so that the tangent of the inclination of l was proportional to the whole rotation of the cylinder, then, when the pointer was taken round a curve, the rotation of the Amsler wheel would be $\iint r\,dr\,d\theta$. In either case, instead of an Amsler wheel, a cylinder mounted as described on the last page would give the integral.

Part II.

The practical value of the tangent principle depends on the fact, that the only operation required of the function to be integrated is that of turning more or less a spindle and tangent-wheel, which may be as light and delicate as any part of a watch, and of which the moment of inertia may be inappreciable. This is in marked contrast to what is necessary in radius machines: the friction in the common double disk or disk and cone integrator, or the inertia of the ball in Sir James Thomson's machine, would be quite sufficient to make the former useless for the integration of such delicate forces as depend on the actions of electricity, or the latter inapplicable to machinery in rapid movement. Another point about tangent machines is, that the whole process of integration is the result of pure rolling, and any doubt that may be felt as to the effect of the sliding action on the accuracy of cosine machines is here removed. The rest of this paper describes some applications of the disk-cylinder integrator some of which are likely to be of practical value.

Engine-power Meter.

As work is motion multiplied by pressure, the work done in an engine may be found by integrating the difference of pressure on the two sides of the piston with respect to the motion of the piston. For any one stroke, this is usually done by measuring the areas of the indicator-diagrams, one taken at each end of the cylinder, and repeating, so as to get an average value. But as the work done, or the area of the diagram, is subject to variation depending on the load, pressure, and speed, only guesses can be made as to the whole amount of work that has been done by an engine during any length of time. Any machine, therefore, that will automatically find the total work done should be of value, not only to users of engine-power, but especially to experimentalists who are engaged on testing the efficiency of engines, and on other subjects where total work done should be known. It is only fair to mention that Messrs. Ashton and Story have an engine-power meter in which the integration is effected by a double-disk integrator acting on the radius principle; but it necessarily suffers from the defects common to all radius machines. The disk-and-cylinder is especially applicable to this particular case; for it is only necessary to make the cylinder reciprocate with the piston of the engine, the motion being of course reduced to a convenient amount, and to make the tangent of the inclination of the disk vary with the difference in pressure on

the two sides of the piston of the engine. Then, at any moment, the cylinder will turn with a speed which is proportional to the rate at which work is being done, and the number of revolutions, as measured by a counter, will be a measure of the work done in foot-pounds or other units during any time. Figs. 5 and 6 are views of an engine-power meter, each partly in section. A, A are two boxes with flexible covers, like the corrugated plate in an aneroid barometer. They may be filled with a mixture of glycerine and water or other liquid, and connected each with one end of the cylinder of the engine. Each diaphragm will feel the pressure, but not the heat, of the steam or gas in the cylinder. The two diaphragms are connected by the rod *r*; and so the effective force tending to bend the diaphragms is the difference of pressure at the two ends of the cylinder. This is the force acting on the piston of the engine. Inclination is given to the tangent-wheel *t* by the rod *r* by a pin working in a radial slot, as is better shown in fig. 7. This arrangement causes the tangent of the inclination of the tangent-wheel to be proportional to the displacement of the rod *r*, and so to the force acting on the piston. C is the integrating-cylinder, which is capable of sliding along, but of turning with a wire W, which may be grooved or polygonal; pinion-wire is very suitable. The integrating-cylinder is caused to reciprocate, by means of a yoke Y and lever L, in time with the piston of the engine. The stroke is reduced to a convenient amount by attaching a string from the piston-rod to a suitable part of the lever L. Now, as the rate at which the cylinder turns is proportional to the longitudinal motion of the integrating-cylinder multiplied by the tangent of the inclination of the tangent-wheel, and as this is proportional to the motion of the piston multiplied by the force urging it, *i. e.* to the work being done, the whole number of the revolutions of the cylinder will measure the whole amount of work done. If the instrument gets out of adjustment so that the tangent-wheel is not parallel to the axis of the cylinder when there is no force, then whatever error it makes in a forward stroke it will take off in the return stroke; so that no accumulating error will be produced. The diaphragms may either be made of steel or highly elastic metal, in which case they form their own springs; or a softer metal, controlled by an external spring, might be used. If a diagram is required, one may be drawn as described on page 81. But it will not be a diagram such as is drawn by an ordinary indicator, but the integral curve of such a diagram; so that force, instead of being represented by the length of an ordinate, will be represented by steepness. Instead of diaphragms as described,

spring pistons or Bourdon pressure-gauge tubes might be employed to give inclination to the tangent-wheel.

Integrating Dynamometers.

The disk-cylinder integrator may be applied to measure the whole amount of work transmitted by shafting or belting. In the case of shafting, what is called a differential coupling—that is, a contrivance which transmits any motion, but measures the force causing such motion—is employed to give inclination to the tangent-wheel. In the case of belting, any of the known dynamometers may be employed for the same purpose; while a mangle-motion driven by the revolving shaft or travelling band, causes the cylinder to reciprocate. Either of the methods given on page 79 may be employed to produce continuous growth of the integral in one direction. As the work transmitted at any moment is force multiplied by motion, and as the tangent of the inclination of the tangent-wheel is proportional to the force, while the reciprocating motion of the cylinder is proportional to the motion, the rate of revolution of the integrating cylinder will be proportional to the rate at which work is being done, and the whole number of revolutions will give the whole amount of work done. If at any time the force causing the motion should change sign and so resist it, as is the case in an engine when there is much cushioning, then the tangent-wheel will incline the other way and take off from the record a corresponding amount of work.

Electric-Current Meters.

The application of the disk-cylinder integrator to an electric-current meter is very obvious. Figs. 8 and 9 are two views of an electric meter, in which the inclination of the magnet M is effected by the electric current passing in a large coil surrounding the instrument. The magnet M and the tangent-wheel *t* are each fixed on the same spindle, which is vertical, and which is very light and delicate. The weight of the magnet produces the necessary pressure between the tangent-wheel and the integrating-cylinder C; and as the surface of each is convex, the friction resisting the turning of the tangent-wheel by the magnet is very small. The cylinder is supported in a bell-crank frame F, which can be made to reciprocate along the wire W by means of the mangle-motion *m m*. The mangle-motion is actuated by clockwork, which may be wound by the current itself when necessary, should such a course be desirable. Fig. 10 shows the construction of a suitable mangle-motion. The pinion can turn, but not move otherwise,

while the frame carrying the racks can move either longitudinally or laterally. A pin projecting centrally from the pinion enters the slot, which is shaded in the figure, and so causes the pinion to gear with the two racks alternately. This lateral movement of the rack-frame is made use of to depress the integrating-cylinder during its back stroke, at which time the magnet rests on the shoulder S. As the tangent of the inclination of the tangent-wheel is proportional to the current-strength, and as the rate of rotation of the cylinder is proportional to the tangent of the inclination of the tangent-wheel, the cylinder will turn with a speed which is proportional to the current-strength, and the whole number of turns, as shown by the counter, will be a measure of the quantity of electricity that has passed. In the arrangement described, time is divided into a great number of equal intervals, and the current-strength during each alternate one considered. After any considerable time, such a sampling of the current would give just as exact a result as would be obtained by integrating continuously. A quick-return mangle-motion might be employed to diminish the proportion of ineffective time; or the whole time could be made effective by keeping the cylinder in continuous contact, and actuating a reversing-gear between the cylinder and a counter by means of the lateral movement of the mangle-motion. It would be well to employ a catch on the armature of a subsidiary electromagnet, so as to stop the clockwork, except when a current is passing. This current-meter, like Edison's electrolytic meter, is a direction-meter. If the current is passing in one direction, it counts it positive; if in the opposite direction, it counts it negative. A reverser actuated by a polarized armature could be employed to make the meter count as positive, a current passing either way, and so make it applicable to the case of alternating currents. Another kind of current-meter, which is by its nature independent of the direction of the current, would be preferable to the magnetic-needle meter and reverser combined.

Figs. 11 and 12 represent an electric-energy meter, which will be described later; but they will serve as diagrams to illustrate a description of the second current-meter. *m m* is the mangle-motion, which causes the cylinder C to reciprocate and bear alternately against the two tangent-wheels *t t*. These wheels are mounted in a common swivelling-frame, which ordinarily is kept vertical by the weights X X, but which may be inclined by a force due to any cause tending to turn the beam B. Now the turning-power of the weights X X varies as the sine of the inclination; while the turning-power of a force acting downwards applied to the point *p* varies

as the cosine of the inclination; therefore the beam will set itself at such an angle that the tangent of the inclination is proportional to the force. If, therefore, the point p of the beam can be pulled downwards by a force which is proportional to the strength of the current, an electric meter will be the result. The coils shown in the diagrams, which belong to the energy-meter, must be removed and replaced by an electromagnet and armature of peculiar construction. Let there be an electromagnet with pole-pieces a certain distance apart, and let there be between them a wedge of iron at its narrow end increasing in thickness or width rapidly, and towards its thicker parts much more slowly; then, on moving such a wedge forwards between the poles, but without touching them, it will at first facilitate by its movement magnetic induction at a great rate; and as it fills up the space, even though the induction through it is greater, yet the increase of that induction is less. Now, as the rate at which magnetic induction is increased by movement measures the force with which such a wedge is pulled forwards, the wedge will, if suitably formed, experience a force with a given current-strength which is less as its entrance is greater, except over a small space near its starting position, where the force should, if possible, be infinite. Also, if the wedge is fixed in position and the current made to vary, it will, so long as the magnetic limit is not approached, experience a force which varies as the square of the current; therefore, if the motion of the wedge is resisted externally by a force which varies as its displacement, it will enter to such an extent that the amount of its entrance is proportional to the current. Let such a wedge be carried by the beam B, so that when it is at its zero position the beam is horizontal; then the inclination of the beam will be greater when the current is greater, and, except with very weak currents, may be made to vary so that its tangent is proportional to the current. The same principle might be applied in another way by causing rotation instead of linear movement of the armature to facilitate induction, and by resisting such motion by a pendulum-weight as before, or by a hair-spring. The armature then would have to be shaped something like an S, and adjusted experimentally so as to give, except with very small currents, a deflection whose tangent is proportional to the current. In Sprague's or Edison's electrolytic meters (the only ones of which I have heard which can lay any claim to being called current-meters at all) a portion only of the current is sent through the meter, and the rest passed by in a shunt. Now, if it were certain that the same proportion of the whole current always passed the meter, there would be

no objection to such a course ; but as a rise in temperature makes an electrolyte a better, and a metal a worse conductor, any electrolytic meter combined with a shunt would have a tendency to show too much in warm weather or if warmed by the current. If, again, there is any polarization, and that polarization is not strictly proportional to the current, then another error will be introduced. It would seem therefore necessary, if accurate indications are required, to make the whole current pass through an electrolytic meter.

Electric-Energy Meters.

Since the energy expended by an electric current between any two points is equal to the current multiplied by the difference of potential of those points—that is, to the main current multiplied by a shunt current passing in a wire of high resistance between the two points—an electric-energy meter may be made by combining two electric-current meters, which take account of the direction in which the electricity passes, in such a way that the first integrates the main-current strength, and that the mangle-motion of the second is driven by the integrating-cylinder of the first : then, if the needle of the second is deflected by the shunt current, the rotation of the second cylinder will give the integral of the energy expended; This is obvious ; for the rate of rotation of the second cylinder is proportional to its rate of reciprocation multiplied by the tangent of the inclination of its tangent-wheel—that is, to the strength of the current in the first machine multiplied by the strength of the current in the second ; so its rate of turning is proportional to the rate at which energy is being expended, and its whole rotation is a measure of the total energy. If at any time either the main or the derived current, but not both, changes sign, then the second cylinder will begin to turn the other way, showing that the current is not doing work in the portion of the conductor between the points, but is being caused to flow by an action of some kind taking place in that portion. If both currents change sign, then, as before, work is being done ; and though the first machine is working backwards, the second is working forwards. A combination of two machines, as described, would integrate both the current and the energy. A more simple and practicable machine for integrating energy alone is shown in figs. 11 and 12. The integrating mechanism has been already described; the electrical principles employed must now be explained. If two wires, one conveying the main current and the other the derived current, are near one another, they will attract or repel one another with a force which is proportional to the product

of the two currents—that is, to the energy being expended; but if the wires are allowed to move, the force will depend also on the position. The question then is, how can the wires be so arranged that the force exerted may be used to incline the tangent-wheels and yet be independent of their motion. The arrangement of solenoids shown in section in fig. 12 is a complete and perfect answer. S_1 and S_2 are two fixed solenoids concentric with one another; and the main current is made to pass through each in the same direction. S_3 is a solenoid made of a very great length of fine wire, preferably of aluminium silver, as suggested by Mr. Imray; and the upper half is wound in one direction and the lower half in the opposite direction; and the derived current is made to pass through it so as to pass in the upper half in the same direction as the main current passes in the fixed solenoids, and in the lower half in the opposite direction. This solenoid is hung in the annular space between the fixed ones by a band passing over the arc of the beam B. The tubes within and without the fixed solenoids and the rings above and below them, all of which are shown dark in the figure, are made of iron, and may or may not be used according as the currents employed are in general weak or strong. But whether the tubes are or are not retained, the use of the rings has certain advantages, which I now proceed to explain. Fig. 13 is a diagram taken by iron filings, which shows the distribution of the lines of force due to the fixed solenoids alone, without the tubes or rings. Here the lines of force cut the walls of the solenoid at an oblique angle, so that the force, which is at right angles both to the wires and the lines of force, tends to stretch the movable coil, and only a portion of it is effective in dragging down the solenoid: moreover the lines of force are very widely distributed over the solenoid, so that, unless it is of very great length, the upper part will leave by its motion many lines. It is true that on the lower end the movable solenoid will, when at its central position, enter as many lines in an element of motion as it leaves at its upper end; but after any considerable movement the upper end will leave many more than the lower end enters; and the force, as it is proportional to the number of lines enclosed, will become less as the solenoid moves from its central position. This will be referred to again later. Now the iron rings act as traps, so to speak, and catch nearly all the lines of force which without them stray over so great a space. Fig. 14 shows the field produced when the iron is present. It will be seen at once that nearly the whole of the induction takes place across a very narrow band of the solenoid, and that the lines of force,

where they cut the solenoid, are nearly horizontal; so that practically the whole of the force developed tends to drag the solenoid downwards, instead of being partly spent, as before, in producing a stretching strain. The consequence is that the movable solenoid may be made very much shorter than would be necessary if no rings were present. The tubes, having a higher coefficient of induction than air, merely serve to increase the number of lines of force, rather than affect their distribution.

If a movable solenoid, arranged as described, is hung by a band passing over the arc of the beam B, then the turning moment due to a down-pulling force is, within certain limits, constant, while the force restraining motion varies as the sine of the inclination of the beam; but had it been hung from the point p, the turning moment would have been proportional to the cosine of the inclination, and the tangent of the inclination would have measured the force. What is wanted is to make the tangent of the inclination proportional to the product of the two currents; and, as explained, this could be attained by using a long solenoid hung from the point p. But such an arrangement causes a double inconvenience; for not only is a long solenoid itself inconvenient, but the fact that it is hung from an arm and not from an arc causes a lateral shifting of the solenoid when the beam B is inclined, thus necessitating a wider annular space for it to pass through. Now the use of a comparatively short solenoid hanging from an arc introduces two errors which are almost absolutely equal and opposite. The error due to the arc is an increase of moment in the ratio of the cosine of the inclination of the beam to 1; that is, the error is equal to the versed sine of the inclination; and, like a thing that increases as the square of a quantity, it is at first quite inappreciable, and it increases in amount with increasing speed as the quantity grows. Now the error due to a short solenoid, such as shown in the figure, is at first nothing; for a given movement will cause the solenoid to enter as many lines of force at one end as it leaves at the other, but as it gets displaced it enters rather fewer than it leaves; and this difference in the number of lines of force increases in amount with increasing speed. Now, if the proportions are so taken that when the inclination of the beam is a little less than the greatest amount permitted to it the actual magnetic error is equal and opposite to the versed-sine error, then, since each is a quantity which grows according to the same kind of law, those errors will be always approximately equal, and their differences very small compared with the errors, and absolutely inappreciable in comparison with the

quantities of which the errors themselves are small. The geometrical representation (fig. 15) makes this clearer. The two sets of errors may be considered as ordinates of two curves drawn to the same abscissa. The point p, where the curves intersect, corresponds to that inclination at which the two errors are made equal, and the origin O to the central position. Since both errors are of a kind which are inappreciable at first, the two curves will have the axis of x as a tangent; and since the two errors are of a kind which grow in the same kind of way, *i. e.* at first slowly, and at an increasing rate as they grow, the curves must be nearly similar; and since they have the common point p, they must be nearly identical. The distance between the curves at any part gives the actual error there; and this being small in comparison with the ordinates at that part, is inappreciable in comparison with the whole distance of that part from the line $z\,z$. In the particular case the ordinate $p\,m$ is about 4 per cent. of $p\,Q$; so the actual error may be safely neglected. But, finally, should there be any error that can be detected at all, such error may be almost entirely eliminated by putting a few extra turns of wire near the ends or the middle of each half of the movable solenoid, according as the curve of the magnetic error between O and p is below or above the curve of the versed-sine error.

Assuming the truth of what is stated in the last paragraph, we find the tangent of the inclination proportional to the rate at which energy is being expended. But the speed of rotation of the cylinder is proportional to the tangent of the inclination; therefore the number of turns of the cylinder, given by the counting-mechanism in the box over the cylinder, is a measure of the total energy expended. As in the other energy-meter, so in this, if the electric current is helped at any time instead of being used, then the integrating-cylinder will turn the other way, and take off from the record an amount which is a measure of the work expended on the current.

There is a point about the solenoid energy-meter which is worthy of notice. The movable solenoid has an equal number of turns in opposite directions; so it is independent of the magnetic field in which the instrument is placed, and so this meter may be used in workshops or near dynamo-machines without its action being interfered with. For the same reason, when used in houses as gas-meters are for gas, it will be impossible for the householder to tamper with its indications by placing magnets round the instrument.

When very powerful currents are employed, it is well to shunt a certain proportion of the main current past the meter, or, when the electromotive force is very great, to introduce

into the secondary circuit resistance-coils. To prevent waste of clockwork energy or of electricity, the main current is made to pass round a subsidiary electromagnet whose armature allows the clockwork to go only when the main current is passing. This armature also, on being attracted, completes the secondary circuit, so that the derived current can only flow when the main current is passing.

The meter may be used as an energy measurer or indicator (not meter) with advantage when setting the carbons of an electric arc. If the two poles are made to touch, though the current is enormously increased, the energy is reduced, owing to a greater falling-off in the difference of potential between the poles. As the carbons are gradually separated the current diminishes, but the difference of potential increases in a higher ratio; so their product increases; that is, the energy expended, and so the heat and light produced, increases. This increase is shown by an increased inclination of the beam. After a time a point is reached at which the current decreases in the same ratio that the difference of potential increases; at this point the inclination of the beam attains a maximum; beyond this the decrease of the current is in a higher ratio than the increase in the difference of potential, so the energy, heat, and light fall off, as is indicated by the diminution of the inclination of the beam. If, therefore, the carbons are placed so that the inclination of the beam is a maximum, then the best effect is being obtained. In the same way, if the current is being employed to drive a machine, the most effective speed for that machine may be found by observing at what speed the inclination of the beam is greatest.

The various meters described depend for their numerical results on two things—(1) the horizontal intensity of the earth's magnetism, (2) the force of gravity. The indications of the first current-meter are inversely proportional to the horizontal intensity, and of the first energy-meter to the square of the horizontal intensity. Each of them is disturbed by changes in the direction of the earth's magnetism.

The second current-meter and the second energy-meter are independent of the magnetic field altogether. Their indications vary inversely as g when the clockwork is regulated by a balance-wheel, or inversely as $\sqrt{g}$ when a pendulum-clock is used. This dependence on gravity is a point of very great importance; for over any one country gravity does not change appreciably, nor does it matter in what direction the machine is placed so long as it is level. By screwing the weights X X up or down, so as to decrease or increase the influence of gravity on the meter, its indications may be regulated to a

standard measure. Therefore, in making the coils, there is no necessity to count the number of turns exactly, or to lay them with the utmost accuracy: they may be wound in the ordinary way, and then a hundred machines or more connected together, with the main circuits in series and with the derived circuits in series, and a current sent from a suitable source through each series; then, if there is one meter which has been standardized by careful experiment, all the rest can be regulated, just as clocks are, by screwing down the weights X X of those that are going fast, or screwing up the weights of those that are going slow.

If in the foregoing paper any of the apparatus is not as fully described as it might be, I must plead as an excuse an endeavour to occupy a reasonable space with an account of what is essentially one invention.

XI. *The Experimental Comparison of Coefficients of Induction.* *By* Herbert Barfield, *B.Sc.**

OF all electrical investigations, the experimental comparison of coefficients of induction is one of the most difficult to carry out with accuracy; and it is doubtless most desirable that we should be able to compare such quantities much more accurately than has been hitherto possible. The usual methods, as far as their general arrangement is concerned, are in all probability not capable of improvement; but, as carried out, there is one modification applicable to all of them, by which the sensitiveness can be vastly increased. This modification is in the use made of the galvanometer. Hitherto it has been usual to observe the throw of the needle due to the passage of the quantity of electricity in one transient current; but if, instead of doing this, we pass through the galvanometer a number of such quantities in rapid succession and (after the manner of Siemens and others) observe the permanent deflection of the needle, we shall find this far greater than any throw attainable by even the most suitable apparatus.

In fact, the permanent deflection due to a certain number of transient currents per second may be compared with the throw of the needle due to one of them, in the following manner:—Let

H represent the magnetic force acting on the needle,
G the galvanometer-constant,
M the magnetic moment of the needle,
τ its time of vibration,

* Communicated by the Author.

A the moment of inertia of mirror and needle,
Q the quantity passing in each transient current,
n the number of such currents per second,
ϕ the permanent deflection due to their passage,
θ the throw of the needle due to one transient current.

Then

$$Q = \frac{H\tau}{G\pi} 2 \sin \tfrac{1}{2}\theta,$$

$$nQ = \frac{H}{G} \tan \phi;$$

or, with sufficient accuracy for the purpose of this comparison,

$$\theta = \frac{G\pi}{H\tau} Q, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\phi = \frac{G}{H} \times n \,.\, Q \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Thus

$$\frac{\phi}{\theta} = \frac{n\tau}{\pi}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

And it may be observed, in passing, that these equations hold rigidly true when ϕ and θ become very small; and thus, in the use of a null method, the equation (3) accurately expresses the relative sensitiveness of the two methods.

Now, since $\frac{\pi}{\tau} = \sqrt{\frac{MH}{A}}$, equation (1) may be written

$$\theta = Q \,.\, G \,.\, \sqrt{\frac{M}{HA}}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

So that, with Q and G given, to obtain the largest value of θ, we must make $\frac{M}{A}$ as large as possible and H as small as possible. The former condition is provided for in the Thomson's galvanometer; and, using such an instrument, we have only to make H as small as we are able. In doing this τ is increased, and equations (2) and (4) show that, whilst we are increasing θ in the ratio $\frac{1}{\sqrt{H}}$, ϕ increases in the square of that ratio.

Now, by diminishing H, τ may readily be made as large as 2π, in which case the relative sensitiveness of the two methods would be given by the equation

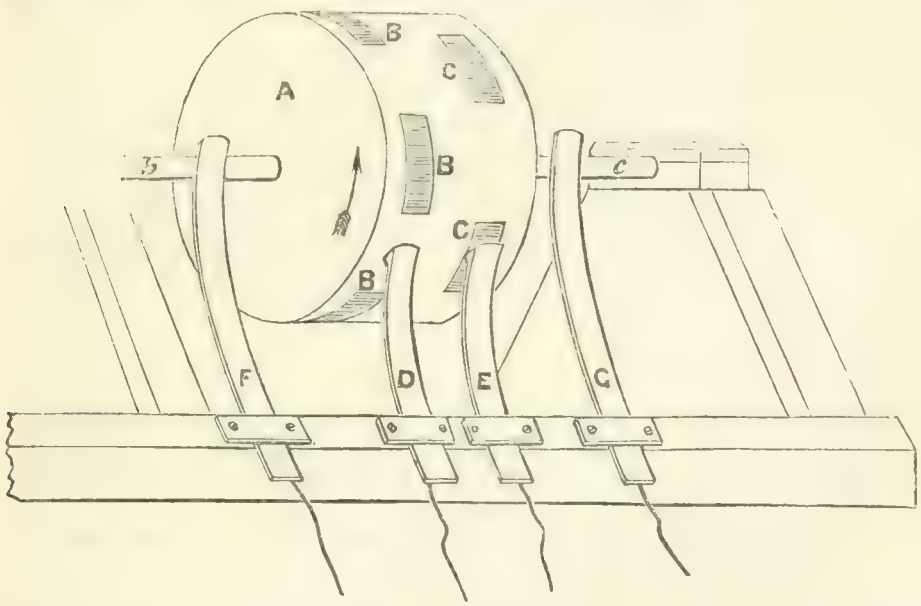
$$\phi = 2n\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

From this it would appear that the value of ϕ is only limited by that of n. This latter quantity, again, cannot exceed a limit determined by the duration of the transient current. This duration Blaserna gives as $\frac{1}{300}$ second, or thereabouts, in one of the cases investigated by him; but the capabilities of

the apparatus would fix a limit far before any such speed was reached. It does not, however, appear to be beyond the mark to say, that the sensitiveness could be in this method made fifty times as large as that of the ordinary mode of experimenting.

The conclusion expressed in equation (3) has been *experimentally* verified by comparing ϕ and θ with the following arrangement:—A single Leclanché cell sends a current through the primary of a certain pair of coils. The secondary is connected up to the galvanometer; and the induced current at breaking is used to deflect the needle.

To obtain a constant succession of transient secondary currents in the same direction, the apparatus shown in the sketch is used:—A is a cylinder of hard dry wood $5\frac{1}{2}$ inches in diameter, which rotates on its axis $b\,c$ in the direction of the arrow. On the surface of A are fixed the brass strips B, C, five in each row : the pieces B are in metallic connexion with each other and also with b; similarly the pieces C are con-



nected to the other end of the axis c. F, G are springs keeping an electrical contact with $b\,c$ respectively whilst the axis rotates. D, E are springs pressing firmly on the cylinder, and making contact with the brass strips when they pass underneath. F, D are inserted in the primary circuit; E, G in the secondary. Thus the secondary circuit is only closed between each break and make of the primary ; and only the induced current at break flows in it.

It is found that when the cylinder is rotated, a steady deflection of the needle is obtained, which increases with the speed of rotation, as equation (2) leads us to expect.

With the coils mentioned above and with one Lechanché in the primary, θ was 13 scale-divisions (the galvanometer being shunted), τ was $7\frac{1}{2}$ seconds, or about 2π. When the cylinder was making about 4 revolutions per second (giving 20 breaks per second), ϕ was found to be a little over 500; that is, the light was off the scale.

Now equation (3) gives

$$\phi = 2n\theta = 520.$$

This shows that a value of ϕ enormously in excess of θ may be obtained; and though the experiment is only a rough proof of equation (3), it serves to show the greatly increased sensitiveness obtained.

Other experiments of a similar nature were made; but, being merely confirmatory of this, they need no further mention. It is possible that a powerful vibrator might with advantage be substituted for the rotating cylinder; and this would be completely self-acting; but it is doubtful whether it would be easier of manipulation than the cylinder, which can be very conveniently worked by a treadle, thus leaving the hands quite free to adjust the resistances.

In using the rotating cylinder with the bridge, E G would be inserted in the "bridge wire;" and since that is thereby broken whenever the battery is on, the effect due to the induced current is separated from that due to any very slight derangement of the balance—a great advantage when it is remembered how troublesome the presence of this latter effect is liable to be with the ordinary arrangement.

Derby, December 30, 1881.

XII. *On Sound-Shadows in Water.* *By* JOHN LECONTE*.

1. MORE or less perfect sound-shadows thrown by hills, buildings, piers, and other obstacles to the transmission of aerial vibrations, must be within the experience of all. Nevertheless the boundaries of such shadows are so imperfectly defined, that they can hardly be compared, except in a general way, with those of light. Moreover, in some cases the obstacles placed in the route of the sound-waves, being elastic, propagate more or less perfectly the sonorous vibrations of the air through their thickness; so that, under these conditions, it is similar to producing a light-shadow by means of a transparent or translucent body.

2. But even in cases in which the sound-vibrations in air are not sensibly transmitted through the intervening obstacle

* From advance sheets of the 'American Journal of Science,' vol. xxiii. January 1882. Communicated by the Author.

the boundaries of the sound-shadows are necessarily very imperfectly defined; for the amount of diffractive divergence proceeding from the secondary waves, originating at the boundaries of the obstacle and propagated within the geometrical shadow, is usually so considerable that the diminution of intensity of sound behind it, although quite perceptible, is by no means so conspicuous as might be expected.

3. The contrast, in this respect, between sound and light is well expressed by Lord Rayleigh:—"When waves of sound impinge upon an obstacle, a portion of the motion is thrown back as an echo, and under cover of the obstacle there is formed a sort of sound-shadow. In order, however, to produce shadows in any thing like optical perfection, the dimensions of the intervening body must be considerable. The standard of comparison proper to the subject is the wave-length of the vibration; it requires almost as extreme conditions to produce rays in the case of sound, as it requires in optics to avoid producing them" *. In other words, the difference between sound and light results from the well-known fact, that an ordinary obstacle bears an immense ratio to the length of a wave of light; but does not bear a very great ratio to the length of a sound-wave. Hence it follows, from the mathematical theory of undulations, that the waves of sound bend around obstacles, and produce more or less effect within the geometrical shadow, whereas light-shadows have definite boundaries and are more sharply defined. For, in the case of light, calculation shows that at any point decidedly within the geometrical projection of the obstacle from the luminous source the disturbance vanishes, while at any point outside of the geometrical projection the disturbance is the same as if the primary wave had passed the screen unimpeded. But this is only partially true in the case of ordinary sound-waves, in consequence of their considerable length; it is rigorously true only when, as in optics, the diameter of the obstacle is large in comparison with the wave-length.

4. There are, however, other causes, depending upon the differences between the sense of hearing and of sight, which doubtless render the appreciation of the shadows of sound much less distinct than those of light. On this point, Lord Rayleigh justly remarks:—"In many cases sound-shadows appear much less perfect than theory would lead us to expect. The anomaly is due in a great measure, I believe, to an error of judgment, depending on the enormous range of intensity with which the ear is capable of dealing. The whistle of a

* 'Theory of Sound,' vol. ii. p. 106, art. 283. London, 1878.

locomotive is very loud at a distance of ten yards. At a mile off the intensity must be 30,000 times less; but the sound still appears rather loud, and would probably be audible, under favourable circumstances, even when enfeebled in the ratio of a million to one. For this reason, it is not easy to obtain complete shadows"*. In other terms, the range of audition is so extensive, that the diffractive secondary waves originating at the boundaries of the obstacle and propagated into the geometrical shadow, though comparatively feeble, produce decided sensuous impressions upon the auditory apparatus of the ear.

5. The mathematical theory of undulations indicates that acoustical shadows should be more distinct for short waves than for long waves. This prevision has been verified by the experiments of Lord Rayleigh†, who found that the sound-shadows cast by acute sounds were more distinct than those produced by grave sounds.

6. Another prevision of theory has been experimentally verified both in the case of light and of sound. It is well known that the great geometer Poisson, in applying Fresnel's integrals to the case of the diffraction of light produced by a small opaque circular disk, was led to the startling result that the illumination of the centre of the shadow was precisely the same as if the disk had been altogether removed. This deduction from theory was experimentally verified by the illustrious Arago, by means of an opaque circular disk 2 millim. in diameter; for he observed a bright point in the centre of the shadow of the disk on which waves of light were directly incident. In this case the secondary waves originating at the disk find the conditions of complete concurrence at a definite point in the axis of the geometrical shadow.

7. The difficulties to be overcome, in the experimental verification of the acoustical analogue of this beautiful phenomenon, are entirely different from those of optics, on account of the immense disproportion of wave-lengths. In light, the disk must be small, and the luminous source must have a very small angular magnitude. In sound, the disk must be comparatively large, and the sounds must be acute. Lord Rayleigh has recently succeeded in experimentally verifying this prevision of theory in the case of sound, by means of a circular disk about fifteen inches in diameter, with a bird-call as the source of sound, placed at a distance of twenty inches from the centre of the plane of the disk. At twenty-four

* Phil. Mag. 5th series, vol. iii. p. 458 (1877).
† Ibid. pp. 458, 459 (1877).

inches on the further side of the disk, the augmentation of the intensity of the sound, in the axis of the acoustical shadow, was obvious both to the ear and to a sensitive flame*.

Sound-Shadows in Water.

8. It is a significant fact in relation to the phenomenon of acoustical shadows, that they seem to be more perfect or more sharply defined in water than in air. Thus, during the progress of the classical experiments of Daniel Colladon, in November 1826, on the velocity of sound in the waters of the Lake of Geneva, this physicist incidentally observed that when the end of the hearing-tube (*cornet acoustique*), plunged into the water, was screened from rectilinear communication with the bell by a projecting wall running out from the shore, whose top was above the surface of the lake, there was a very remarkable diminution in the intensity of the sound, in comparison with that observed at a point equally distant from, but in direct communication with, the source of sound, or out of the "acoustic shadow," thus indicating the relative non-divergence of the rays of sound around obstacles in water as compared with those in air†.

9. Another fact observed by Colladon during these famous experiments is, in this connection, no less significant. He found that the sound of the bell struck under water, when heard at a distance, has no resemblance to its sound in the air. Instead of a prolonged tone, a short sharp sound is heard, like two knife-blades struck together. It was only within 200 metres that the musical tone of the bell was distinguishable after the blow. In air, it is well known, the contrary takes place—the shock of the first impulse of the hammer being heard only in the immediate neighbourhood of the bell, while the continued musical sound is the only one that affects the hearing at a distance‡. Sir John Herschel, in his Treatise on Sound §, promised to explain this curious difference, but has not, so far as I can find, done so. Colladon‖ explains this phenomenon by the nature of the sonorous vibrations in water, showing that the duration of the sound will be much less when transmitted by water than when propagated by air.

Experiments of L. J. LeConte in 1874.

10. The preceding remarks show that comparatively few exact observations have been made on the obstruction pro-

* Phil. Mag. 5th series, vol. ix. pp. 281, 282 (1880).
† *Ann. de Chim. et de Phys.* 2nd series, vol. xxxvi. pp. 256, 257 (1827).
‡ *Op. cit. suprà*, p. 254.
§ Encyc. Metrop. art. 101. ‖ *Op. cit.* p. 255.

duced by interposed obstacles to the propagation of sound-waves in different media. The following experimental results in relation to acoustical shadows in water may be of interest to physicists. The experiments were executed in 1874*, at my suggestion, by my son, L. J. LeConte, during the engineering operations incident to the removal of "Rincon Rock," a sandstone reef in the harbour of San Francisco (near the south-eastern water front of the city), by means of "surface blasting" with "giant powder" or dynamite. The depth of water on the reef was about fifteen feet at low tide, with an extreme tidal range of about six feet. The "cans" or "cartridges" of "giant powder" used contained each about fifteen pounds of the explosive compound, comprising about 75 per cent of nitro-glycerine.

11. *Effects of the Explosive Shock.*—It was observed that the suddenness of the shock imparted to the water by this explosive agent produced the most remarkable and astonishing effects. At the distance of 300 feet or more from the detonating cartridge, two distinct shocks were experienced. The first shock came through the intervening water, and was felt as a short concussion or click before there was any sensible elevation of the column of water resting over the point of explosion. The second shock came a little later by the air, and was heard. It was evidently communicated to the air by the water, at the time the elastic pulse transmitted by this liquid (the first shock) emerged, in a direction nearly normal to its surface, over a limited area around a point vertically above the exploding cartridge. This was obvious from the fact that aerial sound came from this region. The area which was the source of the sound transmitted by the air, was the same as that from which the small jets of water (noticed hereafter, 14) were projected. The gases generated during the explosion came to the surface much later than this shock, and after elevating the column of water over the position of the cartridge to the height of twenty-five or thirty feet.

It is the character of the first shock that deserves special notice. To a person sitting in a small boat floating on the water at a distance of 300 feet or more from the point of explosion, with his feet resting on its bottom, the shock was felt as a sudden blow applied to the soles of the feet. In fact, it drove out the oakum from the seams in the bottom of the boat. When the observer stood on the top of a vertical wooden pile, this shock was felt as a sudden concussion coming up from the

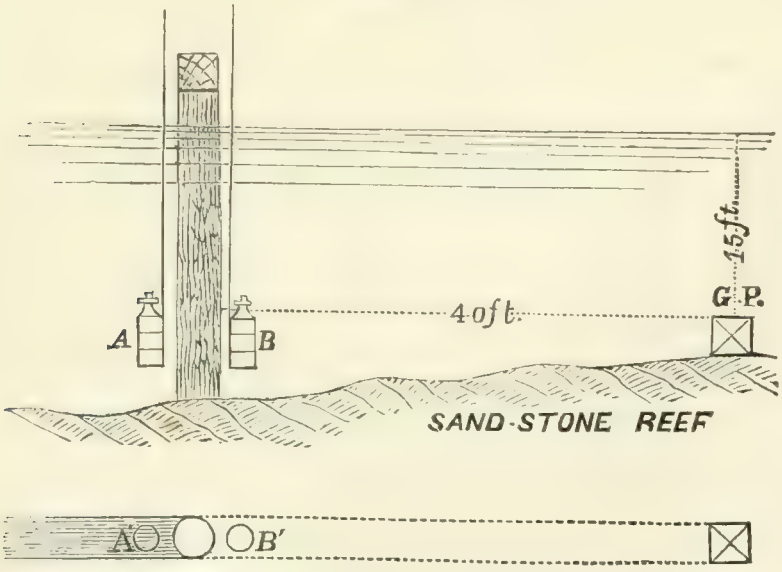
* The long delay in writing out the notes of these experiments in form for publication has been due to domestic affliction and the subsequent pressure of perplexing duties.

water along the cylinder of wood. The concussion produced by such an explosion was so violent that it killed or stunned the fish in the water within a radius of 200 or 300 feet from the explosive centre. They rose to the surface in a helpless condition, and were secured by the boys.

Experiments on Sound-Shadows.

12. *Experiments with stout glass (soda-water) Bottles.*—In these experiments the observer stood on the top of a vertical cylindrical pile (the trunk of an Oregon pine) about one foot in diameter, situated about forty feet horizontally from the explosive cartridge. The bottle, being secured to a rigid rod, was first plunged under the water from ten to twelve inches behind the pile (fig. 1, A), that is within its geometrical shadow. The shock of the explosion did not injure the bottle. It was then plunged into the water in front of the pile (fig. 1, B), or outside of its geometrical shadow. In this position the bottle was shivered to atoms by the concussion due to the explosion. As viewed from the experimenter's situation on the top of the pile, fig. 1, A′ and B′ indicate the two positions of the bottle in the preceding experiments.

Fig. 1.

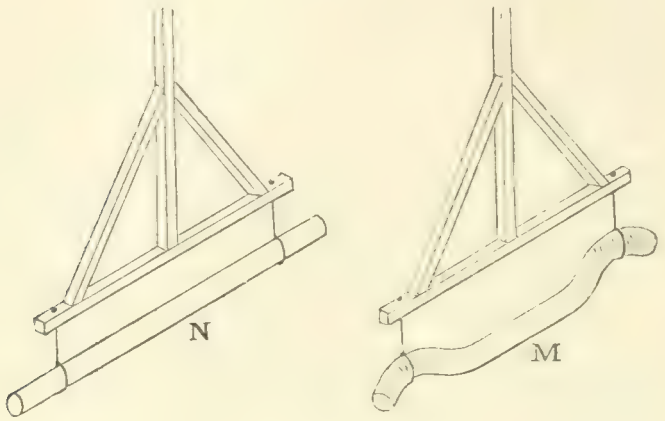


The experiments were varied by plunging bottles into the water in various positions around the pile, within and outside of its geometrical projection from the explosive centre ; and in all cases they were protected from injury when within the geometrical shadow, and were shivered when outside of the same. The same results took place whether the bottles were filled with water or with air.

The breaking of a glass vessel by a sudden shock communicated by means of water is a fact long known, and is illustrated by the old familiar class experiment of exploding a "Prince-Rupert drop" while its bulb is plunged into an ordinary apothecary's phial filled with water.

13. *Experiments with stout glass Tubes.*—The cylindrical glass tubes employed were about 6 feet long and 1·5 inch

Fig. 2.



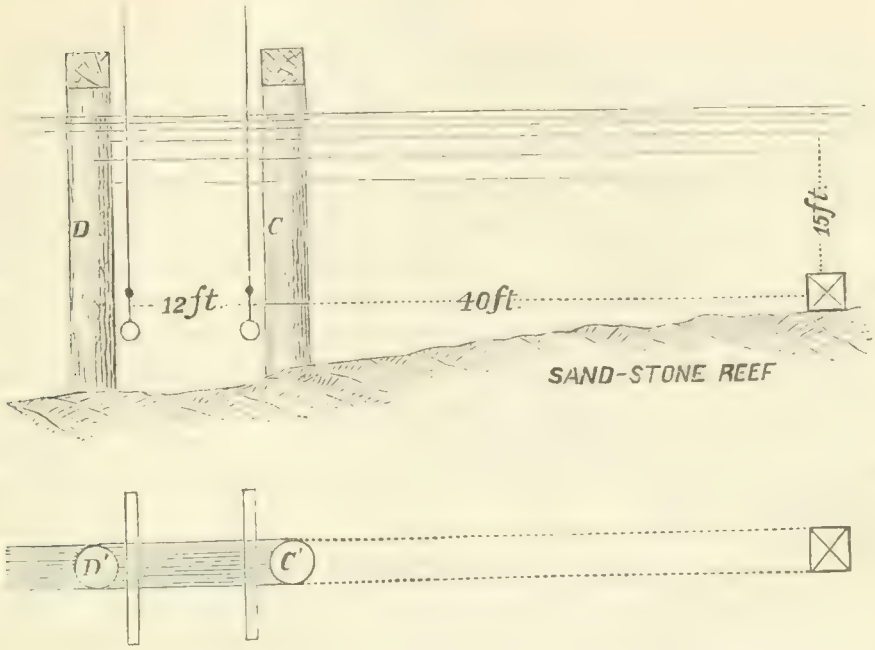
in diameter, the glass being about 0·5 of an inch in thickness. They were covered by pasting cartridge-paper over them, so as to prevent the loss of fragments when breakage occurred. (See fig. 2, M.)

The tubes were adjusted to a framework of wood so arranged (fig. 2, N) that they could be plunged in a horizontal position beneath the surface of the water behind the pile, the axis of the tube being at right angles to the plane of its shadow, and held there (the observer standing as before on the top) with the middle of the tube in the geometrical shadow, while the two extremities projected on either side about 2·5 feet beyond the boundaries of said shadow. (Fig. 3, C and C′.) In every case the shock of the explosion shivered the projecting portions of the tube, and left the portion within the shadow uninjured. The boundaries between the broken and the protected portions of the glass were sharply defined.

By standing on the top of a second pile, in the direction of the axis of the shadow of the first pile, and distant about 12 feet, the experiments were varied by plunging the framework and tubes—adjusted at right angles to the plane of the prolonged shadow—into the water at this distance (12 feet) from the obstacle which obstructed the sound-wave transmitted by the liquid. (Fig. 3, D and D′.) The shock of the explosion produced sensibly the same results as when the tube was near to the obstructing obstacle. The protected portion of the horizontal glass tube was sensibly equal in length to the diameter

of the pile casting the shadow; hence the shadow of the cylindrical pile extended back for about 12 feet between sensibly parallel vertical planes, and its boundaries, at this distance, were still sharply defined.

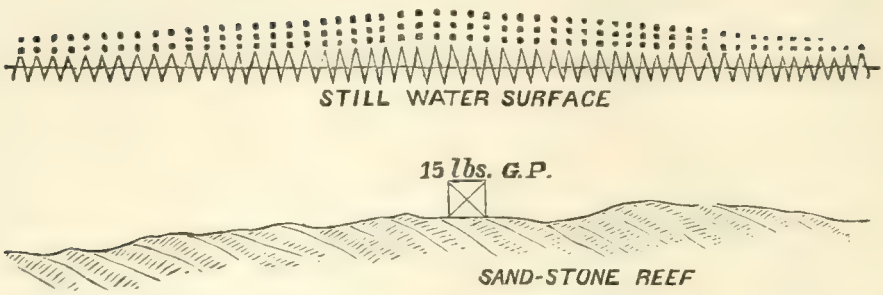
Fig. 3.



It is evident that, if the explosive centre were of insensible magnitude, the horizontal thickness of the geometrical shadow of the pile, at a distance of 12 feet beyond it, would be augmented in the ratio of 40 to 40+12, or of 40 to 52, these numbers being the distances in feet from the centre; so that, if the thickness of the shadow at the pile were 12 inches, its thickness at 12 feet beyond would be 15·6 inches. If, however, the explosive energy occupied more or less space (as was the case in relation to the "giant-powder" cartridges), the thickness of the geometrical shadow or umbra cast by the pile might not increase sensibly with augmenting distance; and, indeed, in case the exploding body exceeded 12 inches in diameter the thickness of the shadow would diminish with increasing distance from the obstructing pile, as in the case of the umbra cast by an opaque body which is smaller than the luminous source.

14. *Another Phenomenon observed.*—Another interesting phenomenon came under our notice during the execution of these experiments. It was the singular effects observed on the surface of the water (when perfectly calm and glassy) for a certain area around the point immediately over the exploding cartridge. Simultaneous with the first shock (11) trans-

Fig. 4.



mitted by the water—and before the ascending gases of explosion disturbed it—the surface of the liquid exhibited numerous jets of water, rising to the height of about 3 inches over the centre of the area, and diminishing in height with augmenting distance from the centre. The appearance presented was not unlike that produced by a heavy shower of rain falling on the calm waters of a lake (fig. 4). To an observer in a boat floating on the adjacent water, and consequently viewing the phenomenon from a point near the water-level, there seemed to be a curious quincunx-like arrangement of the jets.

Explanation of the Phenomena observed.

15. *Greater Distinctness of Sound-Shadows in Water.*—The much greater distinctness of acoustical shadows in water, as compared with those in air, appears to be pretty clearly established by the experiments of Colladon; and the fact seems to be abundantly confirmed by those which we have recorded in the preceding pages. This is an interesting and significant phenomenon in relation to the theory of sound. At first sight it might be supposed that the difference is due to the greater velocity of propagation of the sound-wave in water. This may have been Sir John Herschel's idea when he explained Colladon's results by reference to the greater elasticity of water*.

But, as already indicated (3), according to the mathematical theory of undulations, the intensity of the effects due to the secondary waves progagated into the geometrical shadow from the borders of the obstacle is not directly dependent upon the velocity of propagation, but is properly a function of the wave-length, the diffractive divergence being less for short than for long waves. Hence it follows that the distinctness of sound-shadows, like those of light, should depend upon the shortness of the wave-length. We have already seen (5) that experiment verifies this prevision of theory in the case of sound-waves in air, by demonstrating that acute sounds cast more distinct shadows than grave

* "Treatise on Sound," Encyc. Metrop., art. 102.

sounds. Does this principle apply to sound-shadows in water?

Some physicists have attempted to explain the phenomenon of the great distinctness of sound-shadows in water, as indicated by the observations of Colladon (8), by assuming that the lengths of the sonorous waves propagated through water are much shorter than those transmitted through air*. But no reason is given for this fundamental assumption, other than that it is required by the demands of the theory of undulations, in order to account for the more perfect shadows in water. It evidently would be vastly more philosophical to establish as a matter of fact the greater shortness of the sound-waves in water, and thus to verify the deductions of theory. This we shall endeavour to accomplish.

16. *Measurement of Wave-lengths.*—With regard to continuous or musical sounds, we have the means of very readily determining the wave-length; for it is equal to $\frac{\text{Velocity of Sound}}{\text{Number of Vibrations}}$. It is evident, therefore, that, the number of vibrations or musical pitch of the sonorous body remaining the same, the wave-length in water, so far from being shorter, must be more than four times as long as they are in air. Hence, according to theory, if Colladon's observations had been within the radius (200 metres) at which the musical tone of the sonorous bell was heard, the sound-shadows would have been less distinct than in air. Unfortunately, Colladon does not inform us at what distance from the vibrating bell his observations in relation to the acoustical shadows were made; so that it is impossible to apply this critical test of the theory of shadows. But the presumption is, that the observations were made in the neighbourhood of Thonon (while the sonorous bell was placed at Rolle), at a distance of 13,487 metres from the source of sound, these being the arrangements during the execution of the experiments for determining the velocity of sound in the waters of the Lake of Geneva. At all distances from the bell greater than 200 metres, as we have seen (9), the sound lost its *musical* character and became short and sharp, like two knife-blades struck together. Hence, under the assumption that the observations on sound-shadows were made very far beyond the limits at which musical tones were transmitted, we are precluded from determining the wave-length by the number of vibrations. It appears that in water grave sounds are more rapidly suppressed or damped than acute sounds; so that at

* Vide W. H. C. Bartlett's 'Elements of Natural Philosophy,' "Acoustics and Optics," 4th ed., N. Y. 1866, p. 75, art. 66.

moderate distances from the sonorous centre only the short and sharp sound due to the shock of the striking hammer was transmitted to distant points through the water. It is obvious that the wave-lengths of sounds of this character must be determined by other considerations than those relating to the musical pitch.

In relation to solitary waves generated by sudden blows and explosions*, it may be more difficult to form a just estimate of the wave-length than in the case of musical sounds. Nevertheless it is evident that the wave-length must be directly proportional to the time occupied by the displacing impulse, multiplied by the velocity of transmission of the elastic pulse. In algebraic terms, if L = wave-length, t = time of the generating impulse, and v = velocity of sound in the elastic medium; we have, L varies as $t \times v$, or $L = t \times v$. Consequently, in a given medium, in which v remains constant, L will be a function of t, or the duration of the generative impulse ; so that when the factor t is indefinitely small, the value of L will be

* It may be questionable whether the elastic waves generated by momentary explosions or detonations can be properly regarded as strictly solitary. It is possible that, in such cases, groups of waves are generated similar to those discussed by Lord Rayleigh in the Appendix to his work on the Theory of Sound, vol. ii. pp. 297–302.

Furthermore, the admirable and exquisitely refined arrangements devised by Regnault for investigating the phenomena of sound enabled him to submit this question to a more or less satisfactory experimental test, so far as aerial waves are concerned (*Mémoires de l'Acad. des Sciences*, vol. xxxvii. pp. 45–49 et pp. 278–282: Paris, 1868). He found that the explosion of a pistol charged with ordinary powder does not produce only a single wave, since it gave rise to a recognizable musical tone of very brief duration (p. 45); and the same was the case with the sound produced by the detonation of a mixture of oxygen and hydrogen gases (p. 48). But the explosion of a small charge of fulminate of mercury was so sudden that the wave generated in the air was much shorter, and the "coup" was much more "sec," than that which was produced by a pistol charged with ordinary powder (pp. 47, 48, and pp. 278–281). He adds, "Il est encore difficile de décider si l'on a produit ainsi une onde unique, mais il est certain que si plusieurs ondes prennent naissance, elles doivent se suivre de très près" (p. 48).

Again, the observations of General H. L. Abbot (U. S. Engineers), incident to the heavy blastings at Hallet's Point, Hell Gate, in the harbour of New York, seem to render it at least probable that the waves transmitted by the earth reached the distant observers in groups, or as a train of waves (Silliman's Journal, vol. xv. (1878), p. 178 *et seq.*).

Sir G. B. Airy maintains that "there is reason to think that a single wave in air or in the medium of light would not produce the sensation of sound or colour" (Undulatory Theory of Optics, p. 15, art. 18, 1866). No reason is assigned for this somewhat extraordinary assertion. In the case of light, it is obviously impossible to experimentally test the validity of this idea of the former Astronomer Royal; and it does not appear to be sustained by the results of acoustical experiments.

correspondingly small. Hence, when the time of the blow or explosive impulse is exceedingly brief, the wave-length must be proportionately short.

17. *Application to Sound-Shadows.*—In the experiments of Colladon, if we assume that the brief shock of the hammer on a limited portion of the bell was alone transmitted to the distant observer, it is clear that only the short sound-waves thus generated would reach the distant obstructing wall or screen in the water; and consequently, the greater definiteness of the acoustical shadows in water as compared with those in air would be the necessary result of the greater shortness of the sound-waves in water. Under the foregoing assumptions, the theory of undulations appears to afford a satisfactory explanation of the phenomenon observed by Colladon. Nevertheless it would have been extremely interesting and instructive, as a very severe test of theory, had this distinguished physicist made observations on the relative distinctness of the sound-shadows in the water within the musical range of the submerged bell, as compared with those observed at points so remote that only the sharp blow of the hammer was audible in the water.

In like manner, the application of the principle of briefness of elastic-wave-genesis to the explanation of the phenomena observed by my son in his "Dynamite" experiments, is sufficiently obvious. In fact, all the phenomena incident to the explosion or detonation of the nitro-glycerine compounds indicate that the impulse generated is of indefinitely brief duration; indeed its suddenness is almost beyond conception. Thus, a dynamite cartridge placed upon a log of wood, unconfined and free, with nothing above it except the atmosphere, will, when exploding, shiver the portion of timber under it to atoms—the detonation being so instantaneous that the superincumbent air, as well as the gases generated, having no time to be displaced, become a veritable tampion.

The efficiency of surface-blasting under water by means of these explosive compounds, depends upon this extraordinary suddenness of detonation, which renders the effect akin to that of the sudden blow of an enormous unyielding mass. It is evident, that the wave generated in an elastic medium like water by an explosion of this character must be very intense and very short. Hence the acoustical shadow produced by an obstacle placed in its path of propagation must, as in the case of light, be sharply defined and definite in its boundaries. Thus the striking fact, that the protecting influence of the piles on the glass vessels plunged in the water was narrowly circumscribed within the limits of the geometrical shadow

may be rationally traced to the extreme shortness of the elastic wave, due to the inconceivably brief duration of action of the generative detonations. This view seems to afford a satisfactory explanation of the remarkable results revealed by the experiments in the harbour of San Francisco.

18. *Gunpowder Explosions.*—If the foregoing is the true explanation of the definiteness of the sound-shadows cast in the preceding experiments, then the waves generated by the explosion of ordinary gunpowder, being less sudden, should not produce as sharply-defined shadows as those due to the detonation of dynamite. We have, so far as known, no specific experiments testing this point; but it seems to be quite reasonable that such will be found to be the case whenever the test of experiment is applied; for it is well known that the subaqueous explosions of ordinary powder do not give rise to the remarkable concussions so characteristic of the detonations of the nitro-glycerine mixtures.

19. *Dynamite Explosions in Air.*—Moreover, if my explanation is correct, the acoustical shadows produced by nitro-glycerine detonation in air ought also to be more sharply defined than those due to sounds less suddenly generated. In other words, if the distinctness of sound-shadows depends upon the duration of the impulse which produces the accompanying sound-wave, then the definiteness of the shadows cast by sounds propagated through the air should vary with the suddenness of the action of the generating cause.

Inasmuch as the variations in the duration of the genesis of audible sounds in the atmosphere must, in ordinary experience, be very great, it may, at first sight, appear incredible that the corresponding differences in the perfection of sound-shadows cast by obstacles in the paths of different kinds of sounds should have escaped the most casual observation. But it must be recollected that, for the reasons already assigned (1), (2), (3) and (4), aerial acoustical shadows are not readily appreciated by the ear. Moreover, in the case of sounds transmitted by the air, the distinctness of such shadows is most seriously impaired by the numerous reflected waves which come from circumjacent objects. It should be borne in mind that it is only very recently (5) that the influence of acuteness of sounds on the distinctness of the resulting shadows has been satisfactorily verified by experiment. In like manner I venture to predict that careful experiments will verify the deduction that the shadows due to sounds generated by the extraordinarily brief detonations of dynamite are more sharply defined than those owing their origin to sounds less suddenly produced.

In confirmation of the foregoing view, the following observation may be cited : on the 16th of April, 1880, an explosion of about 2000 or 3000 pounds of a nitro-glycerine compound occurred at the "Giant Powder Works," situated under a bluff on the eastern shore of the Bay of San Francisco, at a distance (determined by triangulation) of 16,201 feet (4938 metres) in a direct line, in a north-west direction, from my room in the University building. About twenty-five adult men, a majority of them Chinamen, were literally blown to atoms, no one escaping to reveal the cause of the accident. The concussion at the University buildings—more than three miles distant—was sufficient to break about a dozen panes of stout window-glass on the side next to the explosive centre. Nearly every person about the University grounds experienced two distinct shocks—one transmitted by the air, and the other by the ground. The cottage occupied by my brother (Professor Joseph LeConte) was situated in the geometrical shadow of one of the buildings, being about 890 feet on the further side of it. No aerial shock was experienced by him or any member of the household ; and the concussion transmitted by the earth was alone felt as a shock emanating from the floor. In other terms, the acoustical shadow cast by the intervening structure completely cut off the sound-wave coming by the air. It is scarcely necessary to add, that for ordinary sounds such would not have been the result.

20. *Phenomenon of Jets.*—The singular phenomenon observed by my son (14), of numerous small jets projected from the surface of the water when the shock transmitted by the liquid reached the surface-area above the exploding cartridge, was probably due to the circumstance that when the short and intense elastic wave emerged in a direction normal, or nearly normal, to the aqueous surface, the tense superficial capillary film yielded to the sudden impulse more readily at some points than at others. The sensibly homogeneous character of such a tensile elastic film would naturally tend to group the points of rupture, or jets of water, into more or less perfect order, partaking more or less of geometrical symmetry. Hence the curious quincunx-like arrangement of jets as viewed by the observer near the water-level. According to this view, the phenomenon in question seems to find its counterpart or analogue in the more or less symmetrical forms produced by the intersection of the lines of rupture as the result of the tensional strains due to the contraction of homogeneous masses, during the process of cooling or of desiccation. Thus the columnar structure of certain igneous rocks seems to be due to the tensile stress of contraction by cooling after

solidification supervened ; while the analogous structure developed by the desiccation of homogeneous masses of moist clay, mud, or starch appears to be produced by a similar strain consequent upon shrinkage from loss of moisture. In a similar manner, the tense superficial capillary film of the water, when it experiences the sudden molecular impulse due to the emergence of the elastic pulse, is ruptured along lines more or less symmetrically disposed on the surface of the water ; and the liquid beneath is projected through these lines or points of least resistance.

Berkeley, California, Oct. 25, 1881.

XIII. *Chemical Symmetry, or the Influence of Atomic Arrangement on the Physical Properties of Compounds. By* Thomas Carnelley, *D.Sc., Professor of Chemistry in Firth College, Sheffield**.

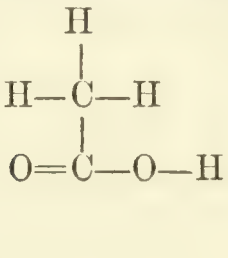
TWO years ago I published in this Magazine (Oct. 1879) a paper on the Influence of Atomic Weight, in which I endeavoured to trace the connexion between the atomic weights of the elements and their properties and those of their compounds. In the present paper I wish to consider this subject from a different point of view, viz. the influence which the atomic position or arrangement has on the properties of compounds.

This is a subject which has lately absorbed a great deal of attention, more especially as regards the connexion between the refractive indices of compounds and the arrangement of their constituent atoms in the molecule; and it will be my object to give as brief and as clear a sketch as possible of the chief results which have been obtained up to the present, tracing the influence which atomic arrangement has on (1) the melting-point, (2) the solubility, (3) the heat of formation, (4) the heat of combustion, (5) the action of polarized light, (6) the specific refraction, and (7) the tinctorial properties of chemical compounds. For this purpose I shall necessarily limit myself entirely to the consideration of carbon compounds, and to those which are isomeric with one another.

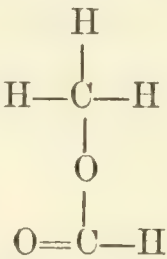
Those compounds are said to be isomeric which, though having the same percentage composition and the same molecular weight, have very different physical and chemical properties. This difference in properties must evidently depend on the different arrangement or linking together of atoms in the molecules of the several compounds. Thus, acetic acid

* Communicated by the Author, being the Opening Address of the Session to the Owens College Chemical Society, Nov. 18, 1881.

and methyl formate have both exactly the same composition, viz. $C_2H_4O_2$, and yet have very different properties,—the former being a well-characterized acid, which on cooling solidifies in the form of large transparent plates, which melt at $+17°$ and boil at 118° C., it possesses a pungent sour smell, and is highly corrosive; whilst the latter is a colourless liquid which has not yet been obtained in the solid state, and boils as low as 33°·4, or about 85° below the isomeric acetic acid, from which it also differs notably in emitting a pleasant ethereal smell. Since both compounds have exactly the same composition, this remarkable difference in properties can only be caused by the atoms being arranged differently in the two bodies; and that the atoms *are* differently arranged, we know from the various chemical decompositions which the two compounds undergo. This difference in atomic arrangement is represented by the following graphical formulæ :—



Acetic acid.



Methyl formate.

In the former the two carbon atoms are directly united, whilst in the latter they are combined indirectly by means of oxygen.

I. Influence of Atomic Arrangement on the Melting-point.

It seems strange that scarcely any attempt has been made to trace the connexion between the composition and melting-points of carbon compounds, notwithstanding that the melting-points of a very large number of such compounds have been determined. The most noteworthy of these attempts is that of Baeyer, in regard to the melting-points of the fatty acids, and that of Körner, who states that, amongst aromatic compounds, what are called para-derivatives usually melt higher than the corresponding ortho- and meta-derivatives.

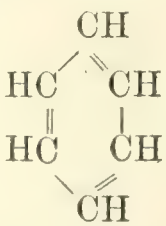
Attention has been directed almost exclusively to the boiling-points. Now the boiling-points are by no means so suitable for tracing the influence of atomic arrangement as the melting-points, since they are far from being such a fixed property as the latter; for, as is well known, the boiling-point is raised or lowered considerably by even small alterations in the pressure, whilst under ordinary circumstances the melting-

point of a pure compound is practically fixed and constant, and hence depends solely on the nature of the compound, whilst the boiling-point depends not only on the nature of the compound, but also on the superincumbent pressure ; and, further, given alterations of pressure by no means produce corresponding alterations in the boiling-points of different compounds. It is therefore far easier to trace the connexion between melting-point and atomic arrangement than the connexion between boiling-point and atomic arrangement.

Such considerations as these induced me, about three years ago, to commence the study of the influence of composition and atomic arrangement on the melting-points of organic compounds. And for this purpose I have made complete lists of all the melting- and boiling-points which have been determined up to the present date. This tabulation has taken three years, and was finished only a few weeks ago. During this time I have collected not far short of 30,000 melting- and boiling-points, about one half of which are melting-points. The systematic arrangement of this enormous quantity of data is now in progress; and I have already been able to trace certain relations between them, which I hope will throw light on the influence of atomic arrangement. Some of these I now propose to describe ; but at the same time I wish to say that, though these conclusions have been verified in a very large number of cases, I have not yet been able to test them in every possible case, because the systematic tabulation of my lists of melting-points is not yet completed.

Of the compounds of carbon, the aromatic compounds are perhaps the most important, and are certainly the most interesting. These compounds may all be considered as derived from benzene.

Benzene has the formula C_6H_6; and its constitution is generally represented as follows:—

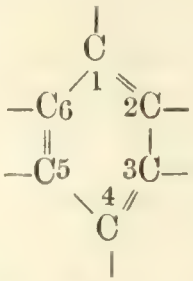


or it consists of a closed ring of six atoms of carbon united alternately with one and two combining-powers, whilst the fourth combining-power of each carbon atom is combined with hydrogen.

The aromatic compounds are derived from this ring by

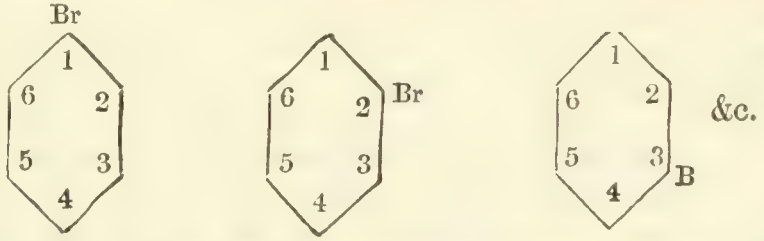
replacing one or more of the atoms of hydrogen by the atoms or groups of atoms of other elements.

The ring

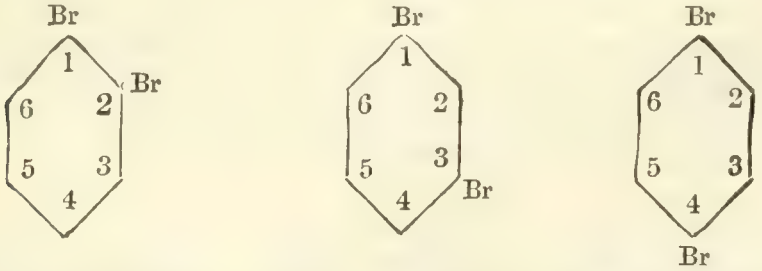


is called the benzene ring, or the benzene nucleus; and the carbon atoms in this ring we will number, for sake of distinction, as in the figure.

Now suppose we replace one atom of hydrogen by an atom of another element, say bromine; then we can only get one derivative, for an identical compound is obtained no matter which of the six atoms of hydrogen is replaced by the atom of bromine, thus :—



But when we replace two atoms of hydrogen by two like atoms of another element, or by two like groups of elements, then no less than three different isomeric bodies may be obtained; viz.—



Ortho- or 1.2-dibrom-benzene. Meta- or 1.3-dibrom-benzene. Para- or 1.4-dibrom-benzene.

It has been observed for some time past, that para-compounds always melt higher than ortho- and meta-compounds; whereas, as regards the ortho- and meta-compounds, the former sometimes melt higher, and sometimes lower, than the latter.

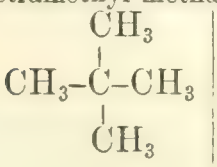
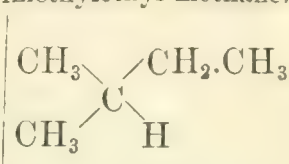
Now the arrangement of the atoms in the para-compounds is symmetrical, whilst in the ortho- and meta-compounds the

arrangement is asymmetrical; *i. e.* if we imagine the para-compound to be suspended from a point in the centre of the benzine ring, then the molecule would balance in any position, whilst this would not be the case with the ortho- and meta-compounds. Or, in other words, the centre of gravity of the molecule of the para-compounds corresponds more nearly with the centre of figure of the molecule than is the case with the ortho- and meta-compounds. This fact led me to conclude that a similar rule might hold good, not only with the primary derivatives of benzene, but also in the case of all isomeric compounds of carbon; and this appears to be true in a very large number of instances. I have therefore ventured to conclude, "*That of two or more isomeric compounds, those whose atoms are the* MORE SYMMETRICALLY *and the* MORE COMPACTLY *arranged melt higher than those in which the atomic arrangement is asymmetrical or in the form of long chains*"*.

After working out this subject at some length, I noticed that Mr. Henry Watts had already observed, in the third Supplement of his well-known 'Dictionary of Chemistry,' that "the more symmetrical the constitution of a benzene derivative, the greater is the resistance which it offers to the passage from the solid to the liquid state," or, in other words, the higher the melting-point. Subsequently, however, in a note to a paper by Neville and Winther (Journ. Chem. Soc. xxxvii. p. 450), Mr. Watts emphatically says that he intended this rule to apply only and solely to the derivatives of benzene containing but six atoms of carbon; whilst I believe that it holds good, with a very few exceptions, in the case of all carbon compounds whatever.

Let us now illustrate this rule by a number of examples taken almost at random. In the following tables the compounds are generally, though not always, arranged in the order of symmetry, those to the left being the most symmetrical. In the case where two compounds are equally symmetrical, that which is most compact is placed to the left.

I.

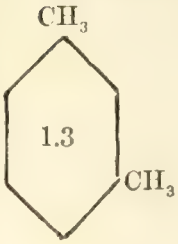
Tetramethyl methane.	Normal pentane.	Dimethylethyl methane.
	$CH_3.CH_2.CH_2.CH_2.CH_3$	
m.p. = −20.	Liquid.	Liquid.

* In the examples given below of this and of all the rules stated subsequently, the exceptions are in every case marked with an asterisk.

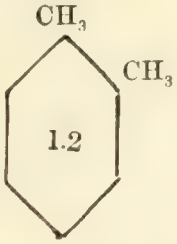
II.

Dimethyl benzene.

m.p. = 15.



= 2.



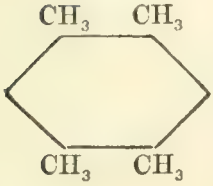
below −22.

Ethyl benzene.

Liquid.

III.

Durene.

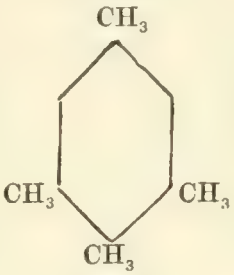


m.p. = 80.

1.4 Diethyl benzene.

Liquid.

Isodurene.



Liquid.

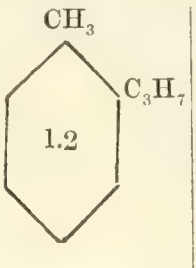
Dimethylethyl benzene.

$C_6H_3.(CH_3)_2.C_2H_5$

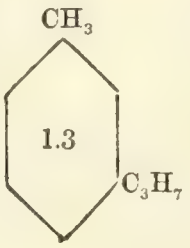
Liquid.

Methylpropyl benzene.

Liquid.



Liquid.



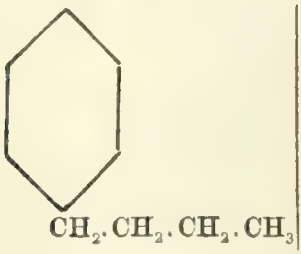
Liquid.

Methylisopropyl benzene.

$C_6H_4.CH_3.CH(CH_3)_2$

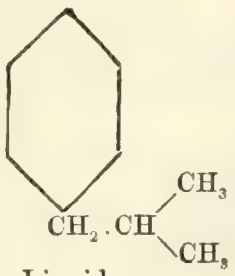
Liquid.

Normal butyl benzene.



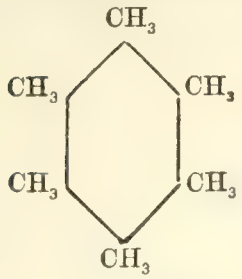
Liquid.

Isobutyl benzene.



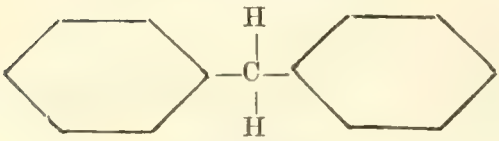
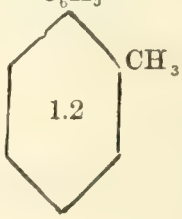
Liquid.

IV.

Hexamethyl benzene.	Dipropyl benzene.	Triethyl benzene.
		$C_6H_3.(C_2H_5)_3$
m.p. =150.	Liquid.	Liquid.

Amylmethyl benzene.	Propylisopropyl benzene.	Hexyl benzene.
$C_6H_4.CH_3.C_5H_{11}$	$C_6H_4.C_3H_7.CH(CH_3)_2$	$C_6H_5.C_6H_{13}$
Liquid.	Liquid.	Liquid.

V.

Diphenyl methane.	Tolylphenyl.	Tolylphenyl.
		
m.p. =26·5.	m.p. = −2.	Liquid.

VI.

1.4; 1.4 Ditolyl.	Dibenzyl.	1.2; 1.4 Ditolyl.
$CH_3.C_6H_4.C_6H_4.CH_3$	$C_6H_5.CH_2.CH_2.C_6H_5$	$CH_3.C_6H_4.C_6H_4.CH_3$
m.p. =121.	m.p. =52.	Liquid.

Benzyltoluene.	Phenylxylene.	Diphenyl ethane.
$C_6H_5.CH_2.C_6H_4.CH_3$	$C_6H_5.C_6H_3(CH_3)_2$	$CH_3.CH(C_6H_5)_2$ (?)
Liquid.	Liquid.	Liquid.

VII. Derivatives of Ethane.

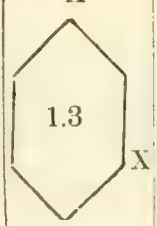
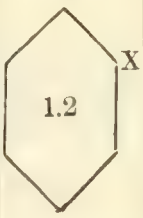
	XX.	Ethylene. XH_2C-CH_2X.	Ethylidene. H_3C-CHX_2.
Dibromide	Br . Br	m.p. =9	Liquid.
Diiodide	I . I	73	,,
Chloroiodide	Cl . I	Solid in freezing-mixture.	,,
Bromoiodide	Br . I	m.p. =28	,,
Diphenide	Ph . Ph	52 (dibenzyl)	,,
Sulphide	S″	112	95
Iodopropionic acid	COOH . I	82	Liquid.
Succinic acid	COOH . COOH	180	129

VIII.

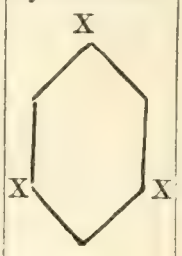
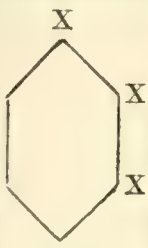
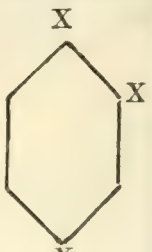
		XHC=CHX.	$H_2C=CX_2$.
Diphenide	Ph . Ph	120 (stilbene)	Liquid.
Ditolide	$C_7H_7 . C_7H_7$	176 (dimethyl stilbene)	Liquid.

IX.

Diderivatives of Benzene.

		Para	Meta 	Ortho 
Dichlorbenzene.....	Cl . Cl	53	L at −18	L at −14
Dibrombenzene.....	Br . Br	89	−28	−1
Diiodobenzene	I . I	127	40	94
Dicyanobenzene	CN . CN	222	158	
Chloroiodobenzene	Cl . I	56	...	L
Bromiodobenzene.....	Br . I	92	L	L
Dihydroxybenzene	OH . OH	172	99	112
Disulphydrobenzene.....	SH . SH	98	27	
Dinitrobenzene.....	$NO_2 . NO_2$	171	90	118
Diamidobenzene	$NH_2 . NH_2$	140	63	99
Nitrobromobenzene	$NO_2 . Br$	126	56	41·5
Nitrochlorobenzene	$NO_2 . Cl$	83	44	32
Nitroiodobenzene	$NO_2 . I$	171	36	49
Amidobromobenzene	$NH_2 . Br$	64	18	31
Amidochlorobenzene	$NH_2 . Cl$	70	L	L above −14
Amidoiodobenzene	$NH_2 . I$	60	25	
Amidonitrobenzene	$NH_2 . NO_2$	146	110	71
Dimethylbenzene	$CH_3 . CH_3$	15	L	L
Diphenylbenzene	Ph . Ph	205	1·3 or 1·2 L	
Tolyldipenylmethane	Me . ($CHPh_2$)	71	59·5 or L	L or 59·5
Methyl chlorobenzene	Me . Cl	6·5	L	L
Methyl bromobenzene	Me . Br	28	L	L
Methyl iodobenzene	Me . I	35	L	L
Methyl amidobenzene	Me . NH_2	45	L	L
Phenyl chlorobenzene	Ph . Cl	75	...	4
Phenyl bromobenzene	Ph . Br	89	...	L
Phenyl amidobenzene	Ph . NH_2	50	...	48
Di-(amidomethyl)benzene..	($CH_2 . NH_2$)($CH_2 . NH_2$)	99	89	
Hydroxy-carboxyl benzene.	OH . COOH	210	200	156
Methoxy-carboxylbenzene..	$OCH_3 . COOH$	175	106	99
Ethoxy-carboxylbenzene ...	OEt . COOH	195	137	20
Fluo-carboxyl benzene	F . COOH	182	123	
Chloro-carboxyl benzene ...	Cl . COOH	234	152	137
Bromo-carboxyl benzene ...	Br . COOH	250	155	148
Iodo-carboxyl benzene.....	I . COOH	267	185	159
Hydroxy-potassium-sulphobenzene.....	OH . SO_3K	not at 260	200	240
Bromhydroxybenzene	OH . Br	64	32	L
Nitro-carboxyl benzene ...	COOH . NO_2	240	141	143
Amido-carboxyl benzene ...	COOH . NH_2	187	174	144

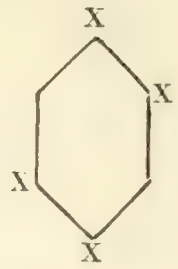
X. *Triderivatives of Benzene.*

		Symmetrical.  1.3.5	Consecutive.  1.2.3	Asymmetrical.  1.2.4
Trichlorobenzene	Cl . Cl . Cl	64	54	16
Tribromobenzene	Br . Br . Br	119	87	44
Methyl dibromobenzene	CH_3 . Br . Br	39	27	L
Nitrodibromobenzene	NO_2 . Br . Br.	104	...	62
	Br . Br . NO_2	...	59	
	Br . NO_2 . Br	...	83	85
Nitrodichlorobenzene	NO_2 . Cl . Cl	65	...	32
	Cl . Cl . NO_2	...	...	43
	Cl . NO_2 . Cl	...	...	55
Nitrochlorobromobenzene	NO_2 . Cl . Br	83	...	47
	Cl . NO_2 . Br	...	...	69
	Br . NO_2 . Cl	...	...	50
Amidodibromobenzene	NH_2 . Br . Br	57*	...	79
	Br . NH_2 . Br	...	...	52
	Br . Br . NH_2	...	...	80
Amidodichlorobenzene	NH_2 . Cl . Cl	51*	24	63
	Cl . NH_2 . Cl	...	...	50
	Cl . Cl . NH_2	...	...	71
Dibrombenzoic acid ...	COOH . Br . Br	209	...	
	Br . Br . COOH	...	...	228*
	Br . COOH . Br	...	...	151
Dinitrobenzoic acid ...	COOH . NO_2 . NO_2	205	...	179
	NO_2 . COOH . NO_2	...	202	140

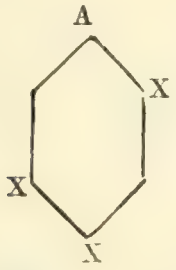
XI. *Tetraderivatives of Benzene.*

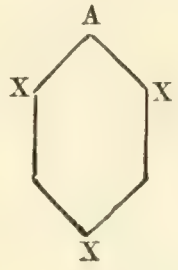
Order of symmetry.		m.p.	1.	2.	3.	4.	5.	6.
1.	Br . Br . Br . Br	138	Br	Br	...	Br	Br	
2.	,, ,,	99	Br	...	Br	Br	Br	
1.	Cl . Cl . Cl . Cl	138	Cl	Cl	...	Cl	Cl	
2.	,, ,,	50	Cl	...	Cl	Cl	Cl	
3.	,, ,,	46	Cl	Cl	Cl	Cl		
1.	NH_2 . Br . NO_2 . Br	202	NH_2	Br	...	NO_2	...	Br
2.	,, ,,	127	NH_2	Br	...	Br	...	NO_2
1.	NH_2 . Cl . Cl . NO_2	100	NH_2	Cl	...	NO_2	...	Cl
2.	,, ,,	66	NH_2	Cl	...	NO_2	Cl	
			or NH_2	Cl	...	...	Cl	NO_2
1.	CH_3 . Cl . Cl . Cl	138	CH_3	Cl	...	Cl	Cl	
2.	,, ,,	27–35	CH_3	...	Cl	Cl	Cl	
3.	,, ,,	46*	CH_3	Cl	Cl	Cl		
1.	CH_3 . Br . Br . Br	112	CH_3	Br	...	Br	Br	
2.	,, ,,	88	CH_3	...	Br	Br	Br	Br
3.	,, ,,	66	CH_3	Br	...	Br		
4.	,, ,,	59	CH_3	Br	Br	...	...	Br
5.	,, ,,	52	CH_3	Br	Br	...	Br	
6.	,, ,,	44	CH_3	Br	Br	Br		

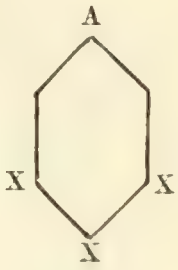
Note.—When all the side chains are of one kind the form



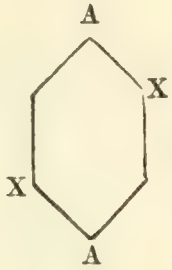
is the most symmetrical, and always has the highest melting-point. But when the side chains are not all of the same kind, then there seems to be a struggle between the three forms



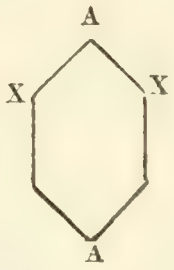




and between

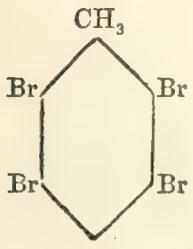


and

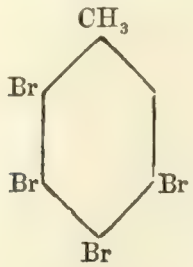


XII.

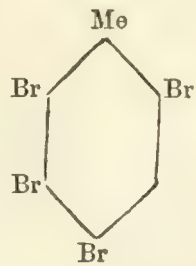
Pentaderivatives of Benzene.



m.p. =116.



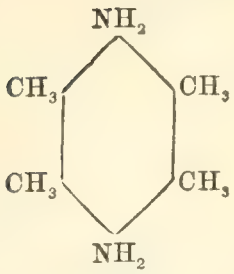
m.p. =111.



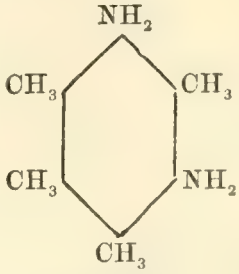
m.p. = 107.

XIII.

Hexaderivatives of Benzene.



m.p. = 51.



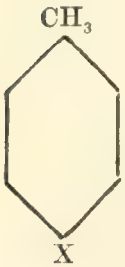
Liquid.

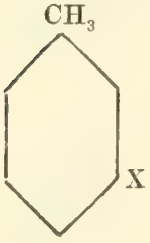
XIV.

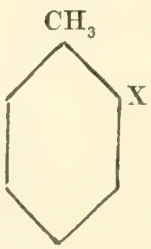
m.p. = −16.

Liquid at −16.

XV.



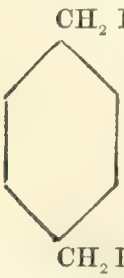




X = Cl	m.p. = 6·5	Liquid.	Liquid.	Liquid.
X = Br	m.p. = 28	Liquid.	Liquid.	Liquid.
X = I	m.p. = 35	Liquid.	Liquid.	Liquid.

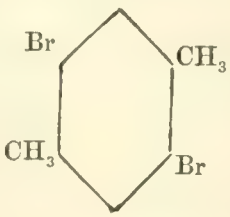
XVI.

Tolylene dibromide.



m.p. = 145.

Dibromparaxylene.



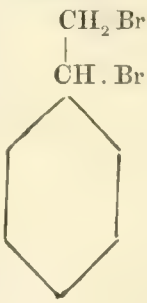
m.p. = 73.

Dibromometaxylene.

?

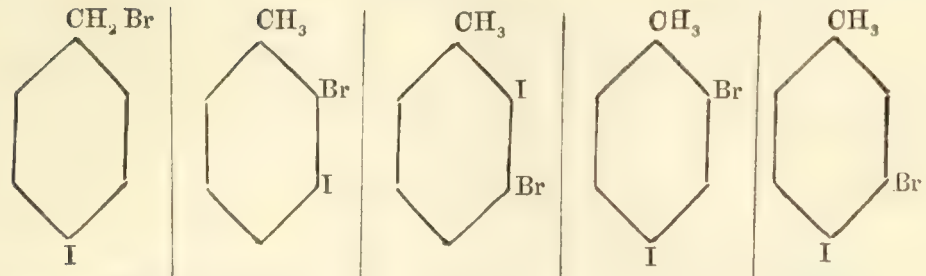
m.p. = 69.

Cinnamene dibromide.



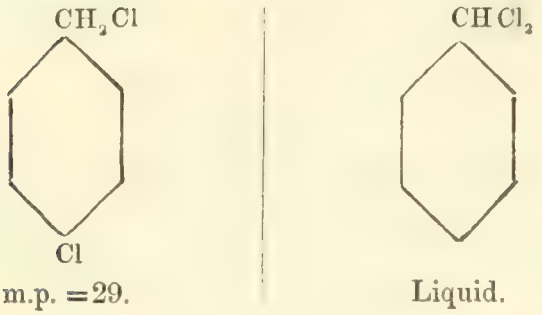
m.p. = 68.

XVII.



m.p. = 79.	Liquid.	Liquid.	Liquid.	Liquid.

XVIII.



m.p. = 29.	Liquid.

XIX.

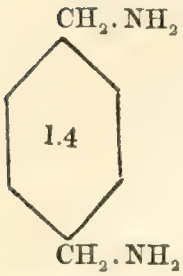
Diamidobenzene.			Phenylhydrazine.
1.4	1.3	1.2	$C_6H_5 . NH . NH_2$
m.p. = 140	63	99	23

XX.

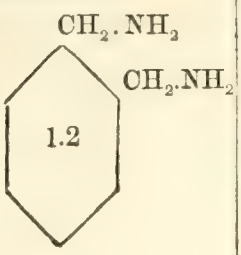
Amidotoluene, $CH_3 . C_6H_4 . NH_2$.			Benzylamine.	Methylaniline.
1.4	1.3	1.2	$C_6H_5 . CH_2 . NH_2$.	$C_6H_5 . NHCH_3$.
m.p. = 45.	Liquid.	Liquid.	Liquid.	Liquid.

XXI.

Tolylenediamine.

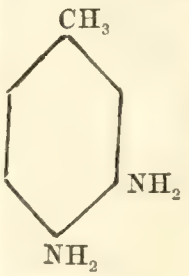


m.p. = 99



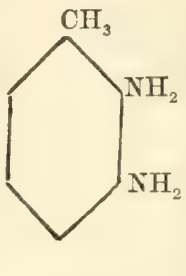
=89

Diamidotoluene.



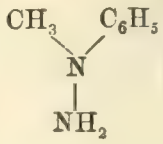
m.p. = 90

=88



=80

Phenylmethyl hydrazine.



Liquid.

XXII.

Ethenediphenylether.
$C_6H_5 . O . C_2H_4 . O . C_6H_5$
m.p. = 98.

Ethyldiphenyl carbonate (1.4).
$C_6H_5 . C_6H_4 . COOC_2H_5$
m.p. = 46.

XXIII.

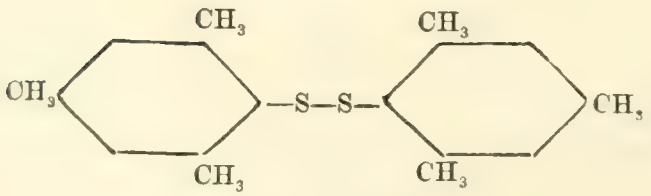
$C_6H_5.CH_2.S.S.CH_2.C_6H_5$
m.p. = 66.

$C_6H_5.S.CH_2.CH_2.S.C_6H_5$
m.p. = 65.

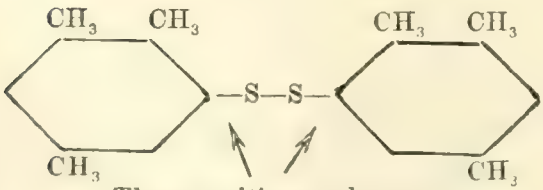
$CH_3.C_6H_4.S.S.C_6H_4.CH_3$
(1.4; 1.4)
m.p. = 43.

$CH_3.C_6H_4.S.S.C_6H_4.CH_3$
(1.3; 1.3)
Liquid.

XXIV.



m.p. = 125.

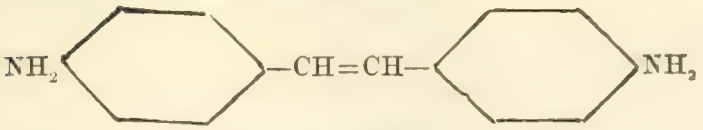


These positions unknown.

m.p. = 115.

XXV.

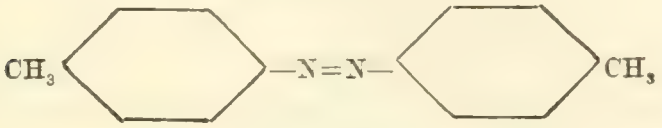
Diamidostilbene.



m.p. = 170*.

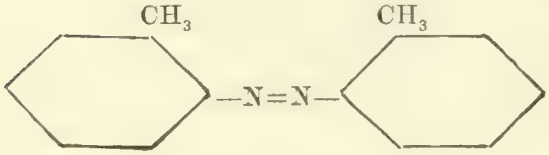
* This, though nearly, is not quite so symmetrical as (1.4; 1.4) azotoluene.

Azotoluene (1.4; 1.4).



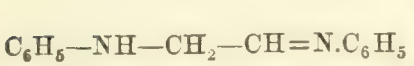
m.p. = 144.

Azotoluene (1.2; 1.2).



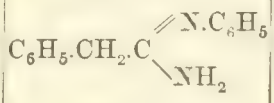
m.p. = 137.

Ethenyldiphenylamidine.



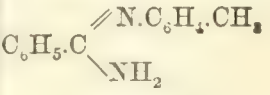
m.p. = 132.

Phenylacetophenylamidine.



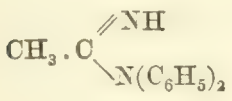
m.p. = 128.

Benzenyltolylamimide.



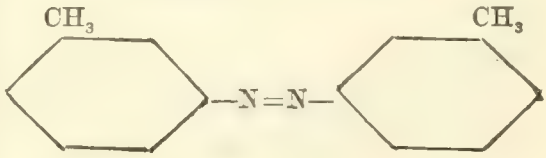
m.p. = 99.

Ethenylisodiphenylamidine.



m.p. = 63.

Azotoluene (1.3; 1.3).



m.p. = 54.

The influence of symmetry on the melting-point is still further shown by the following table, which contains the melting-points of the *most* symmetrical modification of a few of the mono-, di-, tri-, tetra-, penta-, and hexa-derivatives of benzene; from which it will be seen that the melting-points of the mono- and penta-derivatives are comparatively always very low, whilst those of the other derivatives are high; the reason being that benzene cannot form symmetrical mono- and penta-derivatives, whereas a symmetrical form of a di-, tri-, tetra-, and hexa-derivative is always possible.

	Benzene.	Mono-derivative.	Di-derivative.	Tri-derivative.	Tetra-derivative.	Penta-derivative.	Hexa-derivative.
Position of side chains...	0	1	1.4	1.3.5	1.2.4.5	1.2.3.4.5	1.2.3.4.5.6
The side chains being—							
CH_3.....	7	Liquid	15	Liquid	80	50	164
Cl	7	Liquid at −40	53	63·5	138	74	226
Br	7	b.−20	89	119	140	*a* 240	*a* 310
I	7	Liquid	127				
NO_2.....	7	Liquid	171				
NH_2.....	7	−8	147				

Influence of Compactness, or number of Side Chains on the Melting- and Boiling-points.

Lothar Meyer has pointed out, in his 'Modern Theories of Chemistry,' p. 280, "*that, of any number of isomeric compounds, those are the most volatile in which there are the greatest number of side chains, or in which the main chain of atoms is most branched, and therefore in which the main chain is the shortest.*"

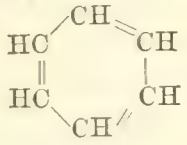
From the observation of a great number of compounds, I cannot help concluding that a rule the exact reverse of the above holds good as regards the melting-point, viz. *that those isomeric compounds melt the highest in which there are the greatest number of side chains.* This has already been indicated to some extent above, where it has been pointed out that the greater the compactness of the molecule the higher the melting-point. The above rules may be illustrated as follows:—

		Boiling-point.	Melting-point.
Pentanes.	$CH_3.CH_2.CH_2.CH_2.CH_3$	38	Liquid.
	$CH_3>CH.CH_2.CH_3$	30	Liquid.
	$CH_3-C(CH_3)_2-CH_3$	9·5	−20
Octanes.	$CH_3-CH_2.CH_2.CH_2.CH_2.CH_2.CH_2.CH_3$	125	Liquid.
	$CH_3>CH-CH_2.CH_2-CH<CH_3$	108	Liquid.
	$CH_3-C(CH_3)(CH_3)-C(CH_3)(CH_3)-CH_3$.....	105	96–97

Table (*continued*).

		Boiling-point.	Melting-point.
Pentyl alcohols.	$CH_3.CH_2.CH_2.CH_2.OH$	117	Liquid.
	$CH_3.CH(CH_3).CH_2.OH$	109	Liquid.
	$CH_3.CH_2.CH(OH).CH_3$	97	Liquid.
	$CH_3-C(CH_3)(CH_3)-OH$	84	25
Heptyl chlorides.	$CH_3.CH_2.CH_2.CH_2.CH_2.CH_2.CH_2.Cl$	159	Liquid.
	$CH_3.CH(CH_3).CH_2.CH_2.CH(Cl).CH_3$	136	Liquid.
	$CH_3-C(CH_3)(CH_3)-C(CH_3)(CH_3)-Cl$	.....	136
Butyric acids.	$CH_3.CH_2.CH_2.COOH$	162	0
	$CH_3.CH(CH_3).COOH$	154	L (?)*
Valeric acids.	$CH_3.CH_2.CH_2.CH_2.COOH$	184	Liquid.
	$CH_3.CH(CH_3).CH_2.COOH$	175	Liquid.
	$CH_3.CH_2.CH(CH_3).COOH$	173	Liquid.
	$CH_3-C(CH_3)(CH_3)-COOH$	164	35·4
Butyl cyanides.	$CH_3.CH_2.CH_2.CH_2.CN$	140	Liquid.
	$CH_3.CH(CH_3).CH_2.CN$	127	Liquid.
	$CH_3-C(CH_3)(CH_3)-CN$	105	15–16

Table (*continued*).

	Boiling-point.	Melting-point.
Dipropargyl, $CH{\equiv}C{-}CH_2{-}CH_2{-}C{\equiv}CH$	85	Liquid.
Benzene, 	81	7
Amyl benzene, $C_6H_5.CH_2.CH_2.CH(CH_3)_2$ ...	193	Liquid.
Propyldimethyl benzene, $C_6H_3(CH_3)_2(CH_2.CH_2.CH_3)$ ...	188	Liquid.
Diethylmethyl benzene, $C_6H_3(CH_2.CH_3)_2(CH_3)$	178 / 198–200	Liquid.
Pentamethyl benzene, $C_6H(CH_3)_5$	225*	50

	Melting-point.		Melting-point.
Propyl terephthalate	31°,	and Isopropyl terephthalate	56°
Butyl „	Liquid,	and Isobutyl „	53
Butyl hippurate	40·5,	and Isobutyl hippurate	45·5

So great indeed is the influence of the compactness of the molecule, that it sometimes overbalances the influence of increase of atomic weight: thus

			Melting-point.
Methyl terephthalate		$C_6H_4(COO.CH_3)_2$	140
Ethyl	„	$C_6H_4(COO.CH_2.CH_3)_2$	44
Propyl	„	$C_6H_4(COO\,CH_2.CH_2.CH_3)_2$ ...	31
Butyl	„	$C_6H_4(COO\,CH_2.CH_2.CH_2.CH_3)_2$	Liquid.

		Melting-point.
Methyl hippurate	$(C_8H_8NO).COO.CH_3$	80·5
Ethyl „	$(C_8H_8NO).COO.CH_2.CH_3$	60·5
Butyl „	$(C_8H_8NO).COO.CH_2.CH_2.CH_2.CH_3$	40·5

Isobutyl hippurate	$(C_8H_8NO).COO.CH_2.CH\langle^{CH_3}_{CH_3}$ ...	45·5
Isoamyl „	$(C_8H_8NO).COO.CH_2.CH_2.CH\langle^{CH_3}_{CH_3}$	27·5

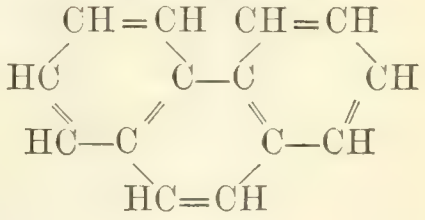
It will now be necessary to make a few remarks in regard to the examples which have been given in illustration of what is thought to be a true connexion between atomic structure and melting-point, and more especially as regards the influence of symmetry. It would, of course, be easy to support any theory if we merely gave those examples which were favourable and excluded all those which were adverse. This, however, has not been done in the examples which have been given above. For the purpose of this paper there were taken quite at random somewhere about 300 cases to which the rule of symmetry could be applied; and of these 278 were in agreement with the rule, whilst 22 were exceptions; the latter amounted therefore to only 7 per cent., which proves that the rule is of almost general application. Many of the exceptions may possibly be due to the constitution or arrangement of the atoms not having been correctly determined, and might thus on close examination be shown to agree with the rule.

The following are some of the most marked exceptions included in the above 22, and which are undoubtedly real exceptions, since the constitution is known with a considerable degree of certainty:—

Tolane { is more symmetrical than } Phenanthrene.

$C_6H_5.C\equiv C.C_6H_5$

m.p. = 60°.



m.p. = 100°.

Phenyl oxide	is more symmetrical than	Oxydiphenyl.
$C_6H_5.O.C_6H_5$		$C_6H_5.C_6H_4.OH$
m.p. = 28°.		m.p. = 164°.
$C_6H_5.S.C_6H_5$	is more symmetrical than	$C_6H_5.C_6H_4.SH$
Liquid.		m.p. = 110°.
Diphenyl	is more symmetrical than	Acenaphthalene.
$C_6H_5.C_6H_5$		$C_{10}H_6<\begin{matrix}CH_2\\ \vert\\ CH_2\end{matrix}$
m.p. = 70°.		m.p. = 95°.

Isomeric Pyrotartaric Acids.

(1)	(2)	(3)	(4)
$CH_3-C(COOH)(COOH)-CH_3$	$H-C(COOH)(CH_2COOH)-CH_3$	$COOH-CH(CH_2CH_3)-CH_2-COOH$	$COOH-CH_2-CH_2-CH_2-COOH$
m.p. = 170°.	m.p. = 112°.	m.p. = 111°·5.	m.p. = 97°·5.

No. 4, which is more symmetrical than No. 2 and No. 3, is less compact; and perhaps this is the cause of the low melting-point.

Bäyer has shown that, if we compare the melting-points of homologous series, we find that the melting-point is materially influenced by odd and even numbers, in such a way *that those compounds containing an even number of carbon atoms melt higher than the neighbouring compounds containing an odd number*, thus:—

	Melting-point. Even.	Odd.		Melting-point. Even.	Odd.
CH_2O_2	...	8·6	$C_2H_2O_4$	200	
$C_2H_4O_2$	+17		$C_3H_4O_4$	...	132
$C_3H_6O_2$	...	b.−21	$C_4H_6O_4$	180	
$C_4H_8O_2$	+ 1		$C_5H_8O_4$	...	97
$C_5H_{10}O_2$	...	b.−16	$C_6H_{10}O_4$	148	
$C_6H_{12}O_2$	− 2		$C_7H_{12}O_4$	...	103
$C_7H_{14}O_2$	...	−10·5	$C_8H_{14}O_4$	140	
$C_8H_{16}O_2$	+16		$C_9H_{16}O_4$	...	106
$C_9H_{18}O_2$	...	+12	$C_{10}H_{18}O_4$	127	
$C_{10}H_{20}O_2$	+30		$C_{11}H_{20}O_4$		108
$C_{16}H_{32}O_2$	+62		$C_{17}H_{32}O_4$	...	132
$C_{17}H_{34}O_2$	...	+60			
$C_{18}H_{36}O_2$	+69				

[To be continued].

XIV. *The Beats of Mistuned Consonances.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE just seen Prof. Silvanus Thompson's Note in your January Number. I have no books with me; but I think I can close the controversy, as far as I am concerned, without them.

As to the word "subjective," I am quite content to have Prof. Helmholtz and his translator (Mr. Ellis) as authorities for my use of it; and I fear I shall require some further authority before I shall feel called upon to give up a use now well established, sanctioned by custom and by the highest authorities.

As to the law that the beats of mistuned consonances of the form $h : 1$ consist of variations of the lower note, (the law is not true for imperfect consonances in general,) no doubt I expressed myself hastily, and, as was evident, without referring to Kœnig's papers.

Notwithstanding this, I cannot admit that the citations made by Prof. Silvanus Thompson contain any demonstration of the law in question, though undoubtedly the last quotation contains a suggestion of the law. I must point out that the sentence translated by Prof. Silvanus Thompson is not quite correctly given; the preceding qualifying clause showing that it is only an assumption, not a definite conclusion. Further, there is no such experimental establishment of the law as to amount to a demonstration.

I have in fact recently learned, through a private source, that Kœnig had, some time ago, convinced himself of this result. But the experiments which were shown to my informant all bore the character of beats still associated with variations of the upper note; and my informant, a most competent person, was wholly unable to verify the conclusion. This agrees entirely with my own observations made with similar means. I therefore consider that the quotations from Kœnig made by Prof. Thompson contain nothing but assumption or inference on this point; and, indeed, they do not purport to be more.

I consider that, in my experiments, the disturbing effects of the harmonics are entirely separated out, so that direct demonstration of the law in question is obtained.

Yours truly,

R. H. M. BOSANQUET.

XV. *On the Theoretic Determination of Vapour-pressure and the Volumes of Vapour and Liquid.* *By* Prof. R. Clausius*. (*Second Paper*†.)

§ 1. In the first paper on this subject, I formed for the determination of the pressure of a gas as a function of the temperature and volume the following equation, which is a generalization of that which I had previously employed for carbonic acid:—

$$\frac{p}{RT}=\frac{1}{v-\alpha}-\frac{27(\alpha+\beta)}{8\theta(v+\beta)^2}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Herein p denotes the pressure, v the volume, and T the absolute temperature—namely the sum $273+t$, if t represents the temperature reckoned from the usual zero-point. Further, R is the constant which already occurs in the usual expression of Mariotte and Gay-Lussac's law; and α and β represent two other constants, the sum of which will further on be denoted by γ. θ signifies a temperature-function which for $T=0$ has the value 0, and for the critical temperature the value 1, but otherwise is provisionally to be left undetermined.

It is self-evident that we can give this equation a simpler form if we combine the temperature-function with the constant factors occurring in the term into one symbol. Namely, if we put

$$\Theta=\frac{8\theta}{27(\alpha+\beta)}=\frac{8\theta}{27\gamma}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the equation is changed into

$$\frac{p}{RT}=\frac{1}{v-\alpha}-\frac{1}{\Theta(v+\beta)^2}. \quad . \quad . \quad . \quad . \quad (3)$$

The relation between the temperature-function Θ in this equation and that employed above, θ, becomes particularly evident when it is borne in mind that the value assumed by Θ for the critical temperature, and which may be denoted by Θ_c, is to be determined by putting the value 1 for θ in equation (2). Thence, namely, we get

$$\Theta_c=\frac{8}{27\gamma}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

* Translated from a separate impression, communicated by the Author, from Wiedemann's *Annalen*, 1881, vol. xiv. pp. 692–704.

† For the first paper see Wiedemann's *Annalen*, xiv. p. 279; translated in the 'Philosophical Magazine' for December 1881, vol. xii. p. 381.

and in consequence of this we can write

$$\theta = \frac{\Theta}{\Theta_c}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

In applying equation (1) to saturated vapour, the pressure of the saturated vapour was denoted by P, and the fraction P/RT by Π. The volumes of the saturated vapour and the liquid standing under the same pressure were denoted, in the usual manner, by s and σ; but for the differences $s-\alpha$ and $\sigma-\alpha$ the simplified symbols W and w were introduced. In order to determine the quantities Π, W, and w in their dependence on θ, first all four quantities were expressed as functions of $\lambda = \log(W/w)$; and then, to facilitate the further calculations, a table was added, from which for every value of θ the corresponding value of λ can be taken. At the same time I remarked that, after the calculation of the table for λ, there would be little further difficulty in calculating a similar table for Π, W, and w also; this I take leave to communicate *infrà*:—

$\theta = \frac{\Theta}{\Theta_c}$.	$\frac{\Pi}{\Pi_c}$.	Δ.	$\frac{W}{W_c}$.	Δ.	$\frac{w}{w_c}$.	Δ
0·20	0·0000059	67	672780	355440	0·033767	1947
0·21	0·0000126	123	317340	156660	0·035714	1978
0·22	0·0000249	213	160680	74175	0·037692	2008
0·23	0·0000462	352	16505	37365	0·039700	2040
0·24	0·0000814	553	49140	19877	0·041740	2072
0·25	0·0001367	834	29263	11097	0·043812	2106
0·26	0·0002201	1215	18166	6464	0·045918	2141
0·27	0·0003416	1715	11702	3912·3	0·048059	2176
0·28	0·0005131	2352	7789·7	2449·8	0·050235	2213
0·29	0·0007483	3144	5339·9	1581·6	0·052448	2253
0·30	0·0010627	4111	3758·3	1049·7	0·054701	2290
0·31	0·0014738	5264	2708·6	714·0	0·056991	2330
0·32	0·0020002	6619	1994·6	497·2	0·059321	2374
0·33	0·0026621	8187	1497·4	353·2	0·061695	2418
0·34	0·0034808	9972	1144·2	255·79	0·064113	2462
0·35	0·0044780	11983	888·41	188·44	0·066575	2508
0·36	0·0056763	14224	699·97	141·07	0·069083	2555
0·37	0·0070987	16693	558·90	107·17	0·071638	2607
0·38	0·0087680	19380	451·73	82·49	0·074245	2658
0·39	0·010706	2230	369·24	64·30	0·076903	2709
0·40	0·012936	2543	304·94	50·69	0·079612	2767
0·41	0·015479	2877	254·25	40·41	0·082379	2822
0·42	0·018356	3233	213·84	32·54	0·085201	2883
0·43	0·021589	3604	181·30	26·43	0·088084	2944
0·44	0·025193	3994	154·87	21·65	0·091028	3009
0·45	0·029187	4402	133·22	17·88	0·094037	3075
0·46	0·033589	4824	115·34	14·89	0·097112	3148
0·47	0·038413	5267	100·45	12·497	0·10026	321
0·48	0·043680		87·953		0·10347	

Table (*continued*).

$\theta=\frac{\Theta}{\Theta_c}$.	$\frac{\Pi}{\Pi_c}$.	Δ.	$\frac{W}{W_c}$.	Δ.	$\frac{w}{w_c}$.	Δ.
0·48	0·043680	5718	87·953	10·532	0·10347	329
0·49	0·049398	6179	77·421	8·945	0·10676	337
0·50	0·055577	6655	68·476	7·640	0·11013	345
0·51	0·062232	7140	60·836	6·561	0·11358	354
0·52	0·069372	7632	54·275	5·663	0·11712	362
0·53	0·077004	8135	48·612	4·914	0·12074	371
0·54	0·085139	8644	43·698	4·284	0·12445	381
0·55	0·093783	9157	39·414	3·750	0·12826	391
0·56	0·10294	967	35·664	3·295	0·13217	402
0·57	0·11261	1020	32·369	2·909	0·13619	412
0·58	0·12281	1072	29·460	2·576	0·14031	425
0·59	0·13353	1125	26·884	2·289	0·14456	437
0·60	0·14478	1179	24·595	2·046	0·14893	447
0·61	0·15657	1230	22·549	1·829	0·15340	463
0·62	0·16887	1284	20·720	1·642	0·15803	476
0·63	0·18171	1337	19·078	1·478	0·16279	491
0·64	0·19508	1391	17·700	1·335	0·16770	507
0·65	0·20899	1444	16·265	1·209	0·17277	524
0·66	0·22343	1494	15·056	1·098	0·17801	540
0·67	0·23837	1548	13·958	998	0·18341	560
0·68	0·25385	1600	12·960	911	0·18901	578
0·69	0·26985	1652	12·049	832	0·19479	600
0·70	0·28637	1703	11·217	763	0·20079	623
0·71	0·30340	1754	10·454	7007	0·20702	645
0·72	0·32094	1806	9·7533	6447	0·21347	672
0·73	0·33900	1855	9·1086	5946	0·22019	698
0·74	0·35755	1906	8·5140	5494	0·22717	728
0·75	0·37661	1956	7·9646	5085	0·23445	759
0·76	0·39617	2004	7·4561	4717	0·24204	793
0·77	0·41621	2051	6·9844	4377	0·24997	830
0·78	0·43672	2100	6·5467	4078	0·25827	870
0·79	0·45772	2147	6·1389	3801	0·26697	915
0·80	0·47919	2196	5·7588	3554	0·27612	961
0·81	0·50115	2242	5·4034	3323	0·28573	1014
0·82	0·52357	2258	5·0711	3115	0·29587	1072
0·83	0·54642	2333	4·7596	2927	0·30659	1138
0·84	0·56975	2377	4·4669	2755	0·31797	1207
0·85	0·59352	2422	4·1914	2597	0·33004	1289
0·86	0·61774	2463	4·9317	2452	0·34293	1382
0·87	0·64237	2508	3·6865	2324	0·35675	1485
0·88	0·66745	2552	3·4541	2208	0·37160	1604
0·89	0·69297	2592	3·2333	2102	0·38764	1744
0·90	0·71889	2634	3·0231	2005	0·40508	1914
0·91	0·74523	2677	2·8226	1926	0·42422	2106
0·92	0·77200	2713	2·6300	1850	0·44528	2360
0·93	0·79913	2755	2·4450	1793	0·46888	2664
0·94	0·82668	2793	2·2657	1751	0·49552	3066
0·95	0·85461	2833	2·0906	1730	0·52618	3607
0·96	0·88294	2870	1·9176	1740	0·56225	4394
0·97	0·91164	2909	1·7436	1801	0·60619	5684
0·98	0·94073	2945	1·5635	2008	0·66303	8239
0·99	0·97018	2982	1·3627	3627	0·74542	25458
1	1		1		1	

Here it must be remarked that the values taken by the quantities Π, W, and w at the critical temperature, at which $\lambda=0$, and which values may be denoted by Π_c, W_c, and w_c, can be obtained from the expressions above mentioned, contained in § 3 of my previous paper, if the series contained in § 4 be also taken into account, as follows:—

$$\Pi_c=\frac{1}{8\gamma};\quad W_c=2\gamma;\quad w_c=2\gamma. \quad . \quad . \quad . \quad . \quad (6)$$

These three values can hence be regarded as determined, just like the value Θ_c, directly by the constant γ; and accordingly the following fractions can be formed:—

$$\frac{\Pi}{\Pi_c},\quad \frac{W}{W_c}, \text{ and } \frac{w}{w_c}.$$

It is these fractions whose values are given in the Table (p. 133) by the side of the corresponding gradually increasing values of the fraction Θ/Θ_c denoted by θ.

§ 2. In the foregoing Table is exhibited a relation, equally holding true for all substances, of the quantities Π, W, and w to a temperature-function Θ which is still left undetermined. Now what the form of the relation between those quantities and the *temperature itself* is, whether and in what degree it also agrees for different substances*, depends on the behaviour of that temperature-function. In my investigation I started originally from the hypothesis that the temperature-function could be represented by an expression containing only one constant, dependent on the nature of the substance; but I found, on closer consideration, that a satisfactory accordance with experiment cannot be attained in so simple a manner. After various comparisons I obtained, as the most suitable form of an equation for the determination of the fraction which we have denoted by Θ/Θ_c, the following:—

$$\frac{\Theta}{\Theta_c}=\frac{a}{T^n}-b, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

* Two older propositions on this relation I have long since discussed (Pogg. *Ann.* lxxxii. p. 273, 1851, and *Abhandlungensammlung*, i. p. 119, 1864). If, namely, the temperatures which with different liquids belong to equal vapour-tensions be called *corresponding* temperatures, then, according to Dalton, *the differences between corresponding temperatures are equal.* Groshans, on the other hand, has set up an equation (Pogg. *Ann.* lxxviii. p. 112, 1849) which, supposing that the temperatures be reckoned from −273° C., expresses that for any two liquids *all corresponding temperatures are proportional.* Of these two propositions the second certainly does not deviate so far from experience as the first, but still it deviates too far for it to be possibly admitted as expressing an actual physical law.

in which a, b, and n are constants which have different values for different substances.

The question now is, to determine these constants for each substance.

In the first place, in regard to carbonic acid, in the formula which I constructed specially for it*, I have given the temperature-function Θ a very simple form, namely that which is obtained from equation (7) by putting in it $b=0$ and $n=2$, by which it is transformed into

$$\frac{\Theta^c}{\Theta} = \frac{a}{T^2},$$

for which we can also write

$$\Theta = T^2 . \text{const.}$$

I added, however, at the time of publishing that formula that I had tried to adjust certain differences still existing between it and Andrews's observations, but had again desisted on account of the uncertainty of the results of those observations.

What I said at that time of the then existing results of observation still holds good. In particular it must be remarked that Andrews's observations refer only to temperatures above 0°, while the formula must remain valid also below 0° down to the freezing-point of carbonic acid, −57°, and hence requires for the determination of its constants the observed values of a similar range of temperatures. Now we possess, it is true, a series of tensions of saturated carbonic-acid vapour, published by Regnault†, which extends to −25°, although not to −50°; but in the experiments by means of which Regnault found those numbers considerable sources of error appear to have prevailed. The tensions cited by Regnault for temperatures above 0° deviate considerably from those observed by Andrews‡; and it is particularly surprising that Regnault professes to have observed the tensions of saturated vapour of carbonic acid up to temperatures above 42°, while after Andrews's experiments it is now established that from 31° upwards there is no saturated carbonic-acid vapour, because condensation no longer takes place.

Under these circumstances I think it advisable to retain provisionally my above-mentioned formula for carbonic acid, as an approximately correct one, and to defer undertaking the more exact determination of the constants till reliable data of observation shall be available also for temperatures below 0° down to the freezing-point of carbonic acid.

* Clausius, Wied. *Ann.* ix. p. 337 (1880).
† Regnault, *Relation des Expériences*, &c. ii. p. 625 (1862).
‡ Andrews, Proc. Roy. Soc. xxiii. p. 516 (1875).

§ 3. A substance specially suited for the comparison of the theoretically determined with the observed vapour-tensions is ether. For this we possess the series of tensions determined by Regnault*, extending from −20° to 120°, of which the reliability cannot be questioned, and a series of tensions by Sajotschewsky†, which extends from 100° to the critical temperature 190°.

Of these observed tensions I have employed three for the determination of the constants occurring in equation (7), and found the following numbers:—

$$a=2665\,;\quad b=0{\cdot}76786\,;\quad n=1{\cdot}19233.$$

By employing these numbers, for every value of T the corresponding values of Θ/Θ_c can be calculated from (7); and then the value belonging to it of the fraction Π/Π_c can be taken from the Table. From this fraction, which can also be written PT_c/P_cT, the value of P is immediately obtained, since P_c and T_c are known. In this way I have calculated the tensions for a series of temperatures advancing by increments of 20° each, and arranged them in the following Table under the designation P calc. For comparison I have added, under P obs., the observed values—below 100° those observed by Regnault, above 100° those observed by Sajotschewsky, and for 100° and 120° the mean values taken from the data of both observers.

t	−20°	0°	20°	40°	60°	80°
P calc. ...	0·0881	0·2427	0·572	1·195	2·265	3·978
P obs.....	0·0907	0·2426	0 569	1·193	2 270	3·977
Δ.....	−0·0026	+0·0001	+0·003	+0·002	−0·005	+0·001

t	100°	120°	140°	160°	180°	190°
P calc. ...	6·557	10·27	15·41	22·33	31·41	36·90
P obs.....	6·549	10·28	15·42	22·34	31·90	36·90
Δ.....	+0·008	− 0·01	− 0·01	− 0·01	− 0·49	0

The accordance between the calculated and the observed tensions is seen to be almost complete. Only at 180° does a difference of inadmissible magnitude occur, which, in comparison with the rest of the differences, is very striking. This, however, is doubtless occasioned by an inaccuracy of the observation-value, as is most clearly to be inferred from the

* Regnault, *Relation des Expériences*, &c. ii. p. 393.
† Sajotschewsky, *Beiblätter*, iii. p. 741 (1879).

fact that Sajotschewsky himself, by means of an empiric formula deduced from the rest of the observation-values, has determined the tension at 180° to be 31·56 instead of 31·90, whereby the difference from the value derived from our Table is reduced from 0·49 to 0·15.

The values of W/W_c and w/w_c, as well as those of Π/Π_c, are given in our Table.

In order to deduce the values of W and w from these fractions, W_c and w_c and consequently, according to (6), the constant γ must be known, for the determination of which the constant R is again requisite. The latter is obtained in the following manner:—The quantity R is, in accordance with its signification, inversely proportional to the specific gravities possessed by the substances in the perfectly gaseous state. Now the value of R for atmospheric air is known*, namely 29·27; and from this follows for ether, if d denotes the specific gravity, referred to air, of the vapour of ether in the state of a perfect gas :—

$$R=\frac{29\cdot27}{d}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The question now is, which specific gravity is to be ascribed to ether vapour in the perfectly gaseous state? That, I believe, can be admitted as such, which is obtained when, in accordance with the accepted chemical formula for ether, it is assumed that 1 vol. oxygen and 10 vols. hydrogen, with the corresponding quantity of carbon, give 2 vols. ether vapour, namely the sp. gr. 2·5604. On employing this number for d, the preceding equation gives

$$R=11\cdot4318.$$

This value refers to 1 kilogram of the substance considered, and therefore, in the present case, of ether; and in it the unit of volume is 1 cubic metre, and the unit pressure the pressure exerted by a weight of 1 kilogram spread over a surface of 1 square metre. These units we will retain in the determination of the other constants and the quantities s and σ.

To determine γ, according to (6) we can put

$$\gamma=\frac{1}{8\Pi_c}=\frac{RT_c}{8P_c}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and from this, if, employing the values found by Sajotschewsky for the critical temperature and pressure, we put

$$T_c=273+190=463 \text{ and } P_c=36\cdot9\times10333,$$

* See Clausius, *Mechanische Wärmetheorie*, i. p. 55.

and for R the previously determined value, we get

$$\gamma = 0{\cdot}0017352.$$

Now, when with the help of the value of W_c and w_c represented by 2γ the quantities W and w have been obtained from the above-mentioned fractions W/W_c and w/w_c and we wish from those quantities to arrive at the volumes s and σ of the ether vapour and liquid, we must for that purpose know the constant α also, since $s = W + \alpha$, and $\sigma = w + \alpha$. For the determination of α it is best to employ any observed volume of the liquid, from which the calculated value of w for the same temperature is to be subtracted. At 0°, according to Kopp, liquid ether has the specific gravity 0·73658, from which, considering that the volume σ refers not to the pressure of 1 atm., but to that of 0·2426 atm., we get $\sigma = 0{\cdot}0013578$. In like manner we get for 20°, according to Kopp, $\sigma = 0{\cdot}0014001$. If with the help of these quantities we determine α and take the mean of two closely accordant values, we find

$$\alpha = 0{\cdot}0010876.$$

It may here at once be added, further, that after the determination of γ and α the value of β also immediately results, since $\beta = \gamma - \alpha$. There comes, namely,

$$\beta = 0{\cdot}0006476.$$

Employing the found values of the constants, we can now calculate from the fractions W/W_c and w/w_c the quantities s and σ, and obtain for the above-considered series of temperatures the following values:—

t	−20°	0°	20°	40°	60°	80°
s ...	3·182	1·238	0·5562	0·2793	0·1524	0·08883
σ ...	0·001318	0·001356	0·001402	0·001455	0·001520	0·001600

t	100°	120°	140°	160°	180°	190°
s ...	0·05417	0·03408	0·02175	0·01373	0·008016	0·004558
σ ...	0·001702	0·001837	0·002030	0·002335	0·002982	0·004558

In conclusion, the form which the general equation (3) assumes after the determination of Θ may now be added. According to (7) and (4) we must put

$$\frac{1}{\Theta} = \frac{1}{\Theta_c}(aT^{-n} - b) = \frac{27\gamma}{8}(aT^{-n} - b).$$

For this, by introducing new constants, we can write more briefly

$$\frac{1}{\Theta} = AT^{-n} - B; \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and thereby (3) is transformed into

$$\frac{p}{RT} = \frac{1}{v-\alpha} - \frac{AT^{-n}-B}{(v+\beta)^2}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

The values of the constants A and B for ether are obtained from the above-given values of a, b, and γ as follows:—

$$A = 15{\cdot}607\,; \quad B = 0{\cdot}0044968.$$

§ 4. The substances in which it has not yet been possible to extend the observations of the vapour-tensions up to the critical temperature, present difficulties, in the determination of the constants a, b, and n in equation (7), which are the greater the more distant the highest observed temperature still is from the critical temperature. If, namely, that distance is very great, the alterations of the constants which, in the temperature-interval for which observations are available, occasion but slight differences in the calculated vapour-densities, yet have for their result a considerable difference in the calculated critical temperature and the pressure corresponding to it.

To these substances belongs *water*. I have tried, from Regnault's observations of tensions, which extend to somewhat above 220°, to deduce the most probable values of the constants, and, after various comparisons, have arrived at the following numbers:—

$$a = 5210; \quad b = 0{\cdot}85; \quad n = 1{\cdot}24.$$

With the aid of these numbers, and on the supposition that for the temperature of 100° the vapour-tension amounts to 1 atmosphere, I have, as in the case of ether, deduced the vapour-tensions for a series of temperatures advancing by increments of 20° each, and found the following values. So far as the observation-values extend, I have added them also, for comparison, selecting those values which Regnault took direct from the curves constructed by him, and which he regarded as the most immediate result of his observations. Where he gives two values, obtained from different curves, I have taken their mean. Beneath the observed tensions are placed the differences from the calculated ones; and I have moreover given in this Table the calculated values of s, which will be spoken of further on.

t	0°	20°	40°	60°	80°	100°
P calc....	0·00574	0·02248	0·07183	0·1956	0·4665	1
P obs. ...	0·00605	0·02288	0·07225	0·1958	0·4666	1
Δ.....	0·00031	0·00040	0·00042	0·0002	0·0001	0
s	216·6	59·30	19·81	7·725	3·422	1·677

t	120°	140°	160°	180°	200°	220°
P calc....	1·962	3·571	6·106	9·907	15·37	22·97
P obs. ...	1·960	3·569	6·118	9·922	15·35	22·88
Δ.....	−0·002	−0·002	0·012	0·015	− 0·02	− 0·09
s	0·8927	0·5085	0·3060	0·1924	0·1253	0·08371

t	240°	260°	280°	300°	320°	332°·32
P calc....	33·23	46·73	64·15	86·27	113·9	134·1
s	0·05700	0·03912	0·02680	0·01796	0·01111	0·005892

Comparison of the calculated and observed tensions shows a very satisfactory accordance within the entire interval from 0° to 220°; accordingly it may well be assumed as probable that the calculated values of the tensions for the higher temperatures also, as well as the calculated critical temperature 332°·32, and the corresponding pressure of 134 atmospheres, do not deviate too far from the truth.

Now, as to the other constants which occur in the formulæ, for R, according to equation (8), if we put therein for d the value 0·6221, which results from the values found by Regnault for the specific gravities of hydrogen and oxygen, we get

$$R = 47{\cdot}05.$$

From this it follows, further, according to (9), if for T_c and P_c the values above found be employed, that

$$\gamma = 0{\cdot}002569.$$

To determine α, from an observed volume of liquid water the calculated value of w for the same temperature must be subtracted. Now liquid water, when heated from 0°, exhibits the peculiar phenomenon that at first it contracts, and only expands above 4°, and that even then, within a considerable interval of temperature, its coefficient of expansion is much more variable than those of other liquids. This phenomenon, which is unquestionably connected with those molecular forces

which at the freezing of water act as forces of crystallization, is not expressed in our equation employed for the determination of w, because in constructing that equation forces of this kind are not taken into consideration. Hence arises for the determination of α an uncertainty, since different values of α are obtained, according to the temperature to which the water-volume employed for the determination refers. If the observed volume at 20° is employed, there comes

$$\alpha = 0{\cdot}000754;$$

and when this number is subtracted from that above given for γ, we get

$$\beta = 0{\cdot}001815.$$

Upon the calculated values of the vapour-volume s the uncertainty existing in relation to the constant α has only a very slight influence, since the total quantity of α, in comparison with the vapour-volume at all temperatures which are not too near the critical temperature, is very small. The calculated values of s for the above-considered series of temperatures are, as already mentioned, annexed to the last Table; they exhibit the volume, in cubic metres, of a kilogram of vapour.

If, lastly, to the equation determining the pressure p be given the form adduced under (11),

$$\frac{p}{RT} = \frac{1}{v-\alpha} - \frac{AT^{-n}-B}{(v+\beta)^2},$$

the constants A and B herein occurring have for water the following values:—

$$A = 45{\cdot}17; \quad B = 0{\cdot}00737.$$

XVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 71.]

December 7, 1881.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "The Zones of the Blackdown Beds and their Correlation with those at Haldon, with a List of the Fossils." By the Rev. W. Downes, B.A., F.G.S.

The author, after some remarks on the inexact way in which

fossils had been collected from or referred to the Blackdown beds, and a sketch of the literature of the subject, passed on to a correlation of the Blackdown beds with deposits in other localities. He pointed out that they do not contain a sufficient number of species in common with the Marne de Bracquegnies to justify an identification with this. He compared them with the Haldon beds, and, by a comparison of the fossils bed by bed, showed that of 196 Blackdown species (omitting a few corals) 50 occur at Haldon; the latter section, however, represents not the whole, but only the upper part of the former, nine beds in the lower part of it being without representatives at Haldon. Here also the higher beds contain a thin band distinguished by a distinct and all but unique fauna (the zone containing the corals described by Prof. Duncan). Comparing the Blackdown beds with lists of Cretaceous fossils from other localities, it would appear that we have neither exclusively Upper-Greensand forms at the top, nor exclusively Lower-Greensand forms at the bottom, nor exclusively Gault forms in the middle.

2. "On some new or little-known Jurassic Crinoids." By P. Herbert Carpenter, Esq., M.A.

3. "Notes on the Polyzoa of the Wenlock Shales, Wenlock Limestone, and Shales over the Wenlock Limestone. From material supplied by G. Maw, Esq., F.L.S., F.G.S." By G. R. Vine, Esq.

December 21.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "The Torridon Sandstone in relation to the Ordovician Rocks of the Northern Highlands." By C. Callaway, Esq., M.A., D.Sc., F.G.S.

The author pointed out that the apparent difference in dip between the Torridon sandstone and the overlying quartzite in the neighbourhood of Loch Broom was due to some markings in the former which suggested a direction of bedding which was not the real one, and that the one rock passed gradually into the other. The two are perfectly conformable at Strath Auchall, where the junction can be well studied, also between Unaport and Strath Kennort, on the north side of Loch Assynt and south of Unaport. The author then considers that there is proof of the conformity of the quartzites with the Torridon sandstone, and that there is no unconformity as generally suggested. In this case it appears to him to follow that the Torridon sandstone must be either Ordovician or Upper Cambrian in age, and that the former is more probable, as the overlying quartzite is not more than some 500 feet thick, so that the united thickness of the two hardly exceeds at most that of the Arenigs of Shropshire or South Wales.

2. "The Precambian (Archæan) Rocks of Shropshire." Part II. By C. Callaway, Esq., D.Sc., F.G.S.

The author described the western axis of Archæan rocks in

Shropshire, which runs roughly parallel with the Wrekin and Caradoc ranges at a distance of from 6 to 7 miles, the intervening district being chiefly occupied by the Longmynd rocks. They are exposed over a line about 11 miles long, forming usually low hills or ridges trending S.S.W. Pontsford Hill, however, at the northern end, projects prominently into the Shropshire plain. In the lower part of this hill are purple rhyolites banded and spherulitic, the latter structure being exhibited in chalcedony filling cavities of the rock. Compact dolerite forms the upper part of the hill. At Lyd's Hole a junction of the Cambrian Grits with the underlying rhyolites is seen. Conglomerates containing the latter rock also occur. At Gatten Lodge are purple hornstones; at Knoll's Ridge somewhat similar rocks, with others that appear intrusive; at Cold Hill purple hornstone and breccia; and at Chissol a greenish hornstone. The rocks of Knolls Wood are probably about on the horizon of those of Caer Caradoc, grits perhaps derived from a gneissic area. Stratigraphical as well as lithological considerations lead the author to the opinion that the series is Precambrian.

3. "The Red Sands of the Arabian Desert." By J. A. Phillips, Esq., F.R.S., F.G.S.

The author described the general characters of the Nefûd, or great red desert of Northern Arabia, which consists of a series of parallel ridges of considerable elevation, no doubt at some period piled up by the action of strong winds, but now no longer undergoing much change of position, as is evidenced by the fact that sticks and stones remain for many days uncovered on the surface, and that the landmarks made use of in crossing the desert appear to be permanent. A specimen of the sand of this desert received by the author from Lady Anne Blunt, is composed of well-rounded red grains from $\frac{1}{50}$ to $\frac{1}{30}$ inch in their longest diameter, which are rendered colourless by treatment with hydrochloric acid, the material thus removed amounting to ·21 per cent., or a little more than $\frac{1}{500}$ of the total weight operated upon, and consisting of ferric oxide with a small quantity of alumina. The sand dried after the action of hydrochloric acid gave on analysis:—

Silica	98·53
Protoxide of iron	0·28
Alumina	0·88
Lime, magnesia, and alkalies	trace.
	99·69

The external coating of ferric oxide must therefore have been deposited subsequently to the rounding of the grains; it could not have been derived from an external decomposition of the grains themselves; and it becomes difficult to imagine in what manner the superficial red coating can have been produced. The author compared these grains with those of the millet-seed sandstones of

Triassic age, with which they closely agree in character, but remarked that the conditions of their occurrence were apparently quite different.

4. "Analyses of five Rocks from the Charnwood-Forest District." By E. E. Berry, Esq. Communicated, with Notes, by Prof. T. G Bonney, F.R.S., Sec. G.S.

These analyses are of hornblendic granite from Mount Sorrel, "syenite" from Markfield, Garendon, and Croft Hill, and "porphyroid" from Sharpley. In the main they confirm the views expressed by Messrs. Hill and Bonney in their communications on the subject, arrived at from microscopic examination, as to the nature and affinities of these rocks.

January 11, 1882.—R. Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Chalk Masses or Boulders included in the Contorted Drift of Cromer, their Origin and Mode of Transport." By T. Mellard Reade, Esq., F.G.S.

The author referred to the occurrence in the Contorted Drift of Cromer of large masses of chalk, and came to the following conclusions:—The travelled masses are derived from sea-cliffs and the result of landslips, aided by the penetration of water forming ice, intrusive in fissures which detached them from the parent rocks. Water rising from subterranean sources accumulated as ice around the fallen masses, and was then frozen into the shore-ice. Ice-floes brought into contact with these masses were also frozen into them, forming a raft capable of floating away the boulders. The incline of the shore and the rise and fall of the tides on the breaking-up of the ice would suffice to launch the rafts into the sea; and they probably floated in water not more than 200 feet deep. The frequency of these boulders and their accumulation in the Cromer deposit point to their having been arrested by the shoaling of the water and by stranding on a submarine bank. The nature of the chalk showed that they had not travelled far.

2. "Observations on the two Types of Cambrian Beds of the British Isles (the Caledonian and Hiberno-Cambrian), and the Conditions under which they were respectively deposited." By Prof. Edward Hull, LL.D., F.R.S., F.G.S.

In this paper the author pointed out the distinctions in mineral character between the Cambrian beds of the North-west Highlands of Scotland and their assumed representatives in the east of Ireland and in North Wales. In the former case, which included the beds belonging to the "Caledonian type," the formation consists of red or purple sandstones and conglomerates; in the latter, which included the beds belonging to the "Hiberno-Cambrian type," the formation

consists of hard green and purple grits and slates contrasting strongly with the former in structure and appearance.

These differences the author considered were due to deposition in distinct basins lying on either side of an Archæan ridge of crystalline rocks which ranged probably from Scandinavia through the central highlands of Scotland, and included the north and west of Ireland, with the counties of Donegal, Derry, Mayo, Sligo, and Galway—in all of which the Cambrian beds were absent, so that the Lower Silurians repose directly and unconformably on the crystalline rocks of Laurentian age.

As additional evidence of the existence of this old ridge, the author showed that when the Lower Silurian beds were in course of formation, the Archæan floor along the west of Scotland must have sloped upwards towards the east; but he agreed with Professor Ramsay that the crystalline rocks of the Outer Hebrides formed the western limit of the Cambrian area of deposition, and that the basin was in the form of an inland lake.

On the other hand, looking at the fossil evidence both of the Irish and Welsh Cambrian beds, he was of opinion that the beds of this basin were in the main, if not altogether, of marine origin, and that the basin itself had a greatly wider range eastward and southward, the old Archæan ridge of the British Isles forming but a small portion of the original margin.

3. "The Devono-Silurian Formation." By Prof. E. Hull, LL.D., F.R.S., F.G.S.

The beds which the author proposed to group under the above designation are found at various parts of the British Isles, and to a slight extent on the continent. The formation, however, is eminently British, and occurs under various local names, of which the following are the principal :—

England and Wales.

Devonshire.—The Foreland Grits and Slates lying below the Lower Devonian beds ("Lynton Beds").

Welsh Borders.—"The passage-beds" of Murchison, above the Upper Ludlow Bone-bed, and including the Downton Sandstone, and rocks of the Ridge of the Trichrug. These beds form the connecting link between the Estuarine Devonian beds of Hereford (generally, but erroneously, called the "Old Red Sandstone") and the Upper Silurian Series.

South-east of England (Sub-Cretaceous district).—The author assumed, from the borings at Ware, Turnford, and Tottenham Court Road described by Mr. Etheridge, that the Devono-Silurian beds lie concealed between Turnford and Tottenham Court Road on the south and Hertford on the north.

Ireland.

South.—"The Dingle Beds," or "Glengariff Grits and Slates,"

lying conformably on the Upper Silurian Beds, as seen in the coast of the Dingle promontory and overlain unconformably by either Old Red Sandstone or Lower Carboniferous beds, 10,000 to 12,000 feet.

North.—"The Fintona Beds," occupying large tracts of Londonderry, Monaghan, and Tyrone, resting unconformably on the Lower Silurian beds of Pomeroy, and overlain unconformably by the Old Red Sandstone or Lower Carboniferous beds, 5000 to 6000 feet in thickness.

SCOTLAND.

Beds of the so-called "Lower Old Red Sandstone," with fish and crustaceans, included in Professor Geikie's "Lake Orcadie, Lake Caledonia, and Lake Cheviot," underlying unconformably the Old Red Sandstone and Lower Calciferous Sandstone, and resting unconformably on older crystalline rocks. Thickness in Caithness about 16,200 feet.

The author considered that all these beds were representative of one another in time, deposited under lacustrine or estuarine conditions, and, as their name indicated, forming a great group intermediate between the Silurian on the one hand and the Devonian on the other. He also submitted that their importance, as indicated by their great development in Ireland and Scotland, entitled them to a distinctive name such as that proposed.

XVII. *Intelligence and Miscellaneous Articles.*

ON THE LUMINOUS INTENSITIES OF THE RADIATIONS EMITTED BY INCANDESCENT PLATINUM. BY PROF. J. VIOLLE.

I HAVE measured at different temperatures, and for various radiations, the light-intensities of incandescent platinum.

I have selected as temperatures some of the melting-points which I previously determined in degrees of the air-thermometer, and which I here repeat (correcting the melting-point of gold, which was set down by mistake at 1035°):—

Melting-point of	silver	.....	954°
,, ,,	gold	.....	1045
,, ,,	palladium	.....	1500
,, ,,	platinum	.....	1775

In order to get some platinum, for example, at 1045°, a button of platinum of about 200 grams weight was put into a crucible of unglazed porcelain, which was introduced into a second crucible, made of fireclay, and already containing at its lower part 500 grams of gold. The whole was placed in a large vertical Perrot furnace traversed along its axis by a long fireclay tube, through which the surface of the platinum could be seen. It was heated up to the fusion of the gold; the arrival gas-cock was then, to a slight extent,

partially closed, so as to induce a commencement of solidification; the cock was again touched; and by thus modifying the arrival of the gas, it was kept, without too much difficulty, just at the point of fusion. The great mass of the furnace secures a sufficiently extensive constant thermal field, and permits its constancy to be easily maintained.

The operation was precisely the same for bringing the platinum to and keeping it at 954°, the outer crucible containing silver instead of gold.

It is rather more difficult to accomplish the operation in the palladium bath; it can, however, be done by feeding the furnace with a Schlœsing blowpipe and protecting the crucibles by graphite casings. Palladium can also be made use of more simply, even at its melting-point, by employing Deville and Debray's furnace and blowpipe.

For the photometric measurements I employed successively the two methods by which luminous intensities are usually measured—the comparison of two contiguous luminous fields, and the extinction of the isochromatic lines called forth by a sensitive polariscope. I used for this purpose the Gouy spectrophotometer and a Trannin spectrophotometer, suitably modified for the present use; and although each of these apparatus suits better than the other in certain cases, I thus obtained a valuable control of the results. The source taken as the term of comparison, in all the experiments, was the Carcel standard lamp burning 42 grams of oil per hour.

The following Table contains the results thus obtained, plus those of a series made at 775° (temperature measured by the calorimetric method).

Temperatures.	Intensities. λ=656. C.	λ=589·2. D.	λ=535. (E=527)	λ=482. (F=486)
775°	0·00300	0·00060	0·00030	
954	0·01544	2·01105	0·00715 (?)	
1045	0·0505	0·0402	0·0265	0·0162
1500	2·371	2·417	2·198	1·894
1775	7·829	8·932	9·759	12·16

If, then, the luminous intensity of incandescent platinum at 954°, 1045°, and 1500°, in the various simple radiations, be successively taken for the unit, we get for the relative intensities:—

775° ...	0·19	...	...	0·05	...	...	0·04					
954 ...	1	...	...	1	...	...	1					
1045 ...	3·27	1	...	3·64	1	...	3·71	1	...	1		
1500 ...	154	47	1	219	60	1	307	83	1	117	1	
1775 ...	507	155	3·30	809	222	3·70	1365	368	4·44	752	6·42	

Various consequences follow from these numbers, both for the

law of radiation at high temperatures, and in regard to the measurement of those high temperatures by photometry. I shall shortly resume these two important questions, at present confining myself to indicating the formula

$$I = mT^3(1+\epsilon\alpha^{-T})^T$$

as well representing the results. I is the intensity of a simple radiation; T, the absolute temperature; m, ϵ, and α, constants which will have to be determined.—*Comptes Rendus de l'Académie des Sciences*, t. xcii. p. 866 (1881).

AN APPLICATION OF ACCIDENTAL IMAGES. (SECOND NOTE.)

BY J. PLATEAU.

In my first Note* I showed that the distance to which we refer the moon in the sky can be pretty exactly estimated by projecting upon a wall the dark accidental image which succeeds the contemplation of the moon itself, then receding from or approaching towards the wall till the diameter of the image is judged to be equal to that of the celestial object, and, lastly, measuring the distance at which the observer is from the wall when this condition appears to be fulfilled. I said then that one of my sons had by this means obtained about 51 metres for the distance in question. As this value seemed very small, I have more recently induced my son to make a control experiment, although with less precision: he sought a position, in front of the wall, such that the diameter of the dark image appeared to him as exactly as possible half of that of the moon; and this required him to be distant from the wall not more than 23·5 metres. Now, in virtue of the known proportionality between the diameter of the accidental image and the distance to which it is projected, double the above quantity, viz. 47 metres, consequently represented approximately the distance at which my son instinctively placed the moon; and it will be seen that this latter distance, though not coinciding with the 51 metres of the previous experiment less subject to error, is nevertheless of the same order.

M. l'abbé Thirion, Professor at the Collège de la Paix at Namur, has since published† an estimate at which he arrived by a quite different procedure. He assembled twelve of his pupils, and asked each of them to trace on a black board a circumference of the size he saw the moon in the sky. These twelve circumferences were very unequal; the smallest was only 19 centim. in diameter, while

* *Bull. Acad. Roy. Belg.* [2] xlix. 1880, p. 316; Phil. Mag. August 1880, p. 134.

† *La Lune—les préjugés et les illusions*: Bruxelles, 1881.

or, if the simultaneous modification of the charges and potentials is infinitesimal,

$$\Sigma M \delta V = \Sigma V \delta M. \quad \ldots\ldots\ldots\ldots \quad (2')$$

whence this theorem, a consequence of Gauss's principle explained according to § I.:—

In a system of fixed conductors, in which two distinct systems of equilibrium are considered, the sum of the products of the initial charge of each conductor and the variation of its potential from one state to the other is equal to the sum of the products of the initial potential and the variation of the charge.

III. *When conductors, maintained at constant potentials, are left to their mutual actions, the energy of the system tends towards a maximum.*

Clerk Maxwell has demonstrated this theorem* by means of the linear equations which exist between the potentials and the charges. The following process is more direct and speedy.

Let us suppose, at the beginning, each conductor A_1, A_2, ... insulated, and impress on the system an *infinitely small deformation*; the charges M_1, M_2, ... do not change; there are for the respective potentials the falls δV_1, δV_2, ...; the loss of energy, *equal to the external work accomplished*, is

$$\delta W = -\tfrac{1}{2} \Sigma M \delta V.$$

Now, the conductors being fixed, let us connect them to constant batteries, in order to restore the potentials to their original values. This restoration of the potentials cannot be effected without additional charges δM_1, δM_2, ..., regulated by the relation (2'). The *positive* variation of the initial energy will therefore be

$$\delta' W = +\tfrac{1}{2} \Sigma M \delta V.$$

We hence conclude that

$$\delta W + \delta' W = 0,$$

and

$$\delta' W - \delta W = \Sigma M \delta V.$$

Therefore (1) the work accomplished, during the displacement, by the electrical forces is equal to the augmentation of energy of the system; (2) the energy furnished by the sources is equal to twice one or the other of those quantities, and is expended *exactly*, half in mechanical work, half in electrical work or potential energy.

According to the equality (2), the preceding theorem applies to a *finite deformation*; but, for its application to the theory of electrometers, there is, as is known, only occasion to consider an elementary modification.—*Comptes Rendus de l'Académie des Sciences*, Jan. 9, 1882, t. xciv. pp. 74–76.

* 'Electricity and Magnetism,' vol. i. p. 96.

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and

$$\delta' W - \delta W = \Sigma M\delta V.$$

Therefore (1) the work accomplished, during the displacement, by the electrical forces is equal to the augmentation of energy of the system; (2) the energy furnished by the sources is equal to twice one or the other of those quantities, and is expended *exactly*, half in mechanical work, half in electrical work or potential energy.

According to the equality (2), the preceding theorem applies to a *finite deformation*; but, for its application to the theory of electrometers, there is, as is known, only occasion to consider an elementary modification.—*Comptes Rendus de l'Académie des Sciences*, Jan. 9, 1882, t. xciv. pp. 74–76.

* 'Electricity and Magnetism,' vol. i. p. 96.

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XVIII. *Electro-optic Experiments on various Liquids.* By JOHN KERR, *LL.D., Free Church Training College, Glasgow**.

THE following notes of experiments are intended as a sequel to two of my former papers on Electro-optics, which appeared in the Philosophical Magazine for August and September 1879. In those papers I gave an account of methods and results in connexion with twenty-seven liquid dielectrics; thirteen of them belonging to the class of fixed oils, and all clearly active as nonconductors, with the one exception of nitrobenzol. Shortly afterwards, Professor Röntgen repeated and varied my experiments, and produced a very interesting investigation on the subject; and, in particular, he extended the list of electro-optically active dielectrics by the important addition of three bodies—water, sulphuric ether, and glycerine†. To the list already published I have now to add more than a hundred new liquids, with specification of their actions as positive or negative, strong or weak, pure or impure. I shall give first a condensed account of the experiments, and then a summary of results (art. 29).

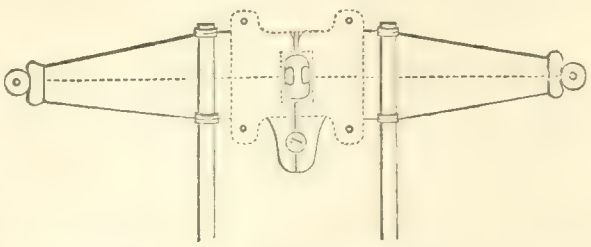
Apparatus and Methods.

1. The small plate cell is the principal instrument used in the present experiments. As I gave a full description of this

* Communicated by the Author.

† There is a good account of Professor Röntgen's paper in Mr. Gordon's Treatise on Electricity.

important piece in the first of my two papers already referred to, I think that here I need only reproduce the diagram.



Optical Arrangements.—The plate cell, charged nearly full with the dielectric liquid, is stationed between the polarizing and analyzing Nicols, the faces of the liquid plate being always perpendicular to the line of vision. In most cases, the object viewed in the polariscope is a narrow and sharply-outlined flame, a flat and thin paraffin-flame presented edgeways, and seen exactly through the centre of the electric field. The first Nicol is fixed with its principal section at 45° to the horizon, or to the lines of force; and the second Nicol is fixed at extinction. When the light is well restored by electric force, the character of the effect as positive or negative is generally detected at a glance, and with perfect certainty, by the use of the hand-compensator. This is a small rectangular slip of thin plate glass, held in the observer's hands between the two Nicols, and simply stretched or compressed in a direction parallel to the lines of force.

Electric Arrangements.—These vary with circumstances, and are sometimes all employed successively in the examination of one dielectric. The liquid may be tried (1) as a nonconductor, (2) as a conductor, but without the jar, (3) with the jar, (4) with intense discharges of a Ruhmkorff's coil.

Every new liquid is tested for insulation in the first place. The outer balls of the cell are connected respectively with prime conductor and earth by unbroken wires, and the machine is set in motion. The test consists in bringing up the knuckle or an earth-connected ball to contact with prime conductor, and *receiving*, or *not receiving*, a spark.

(1) To try a liquid electro-optically as a nonconductor. The cell is placed in the polariscope at extinction, its outer balls being connected with prime conductor and earth by unbroken wires; the machine is set in motion, and the polariscope is watched for a restoration of the light from extinction. When there is a clear effect, it is tested by the hand-compensator, and characterized as positive or negative. This method has given distinct results with every moderately good insulator yet examined.

(2) When the prime conductor gives no sensible spark in the test for insulation, the first method fails, and the liquid is then tried as a conductor, but first without the jar. The wire from prime conductor to cell is broken, and the ends are inserted in two metallic balls, which are supported on insulating pillars, and separated by an air-interval. The machine being set in motion, the two balls are put in contact at every turn or half turn of the plate, so as to discharge the prime conductor through cell to earth; and the polariscope is watched for restoration at the instants of discharge. When the optical effect is moderately strong and not very abrupt, the hand-compensator works as well here as in the former case. There are many liquids (nitrobenzol, benzonitrile, amyl alcohol, &c.) which give intense restorations purely positive or purely negative when tried in this way, though they give no sure trace of effect when tried as nonconductors.

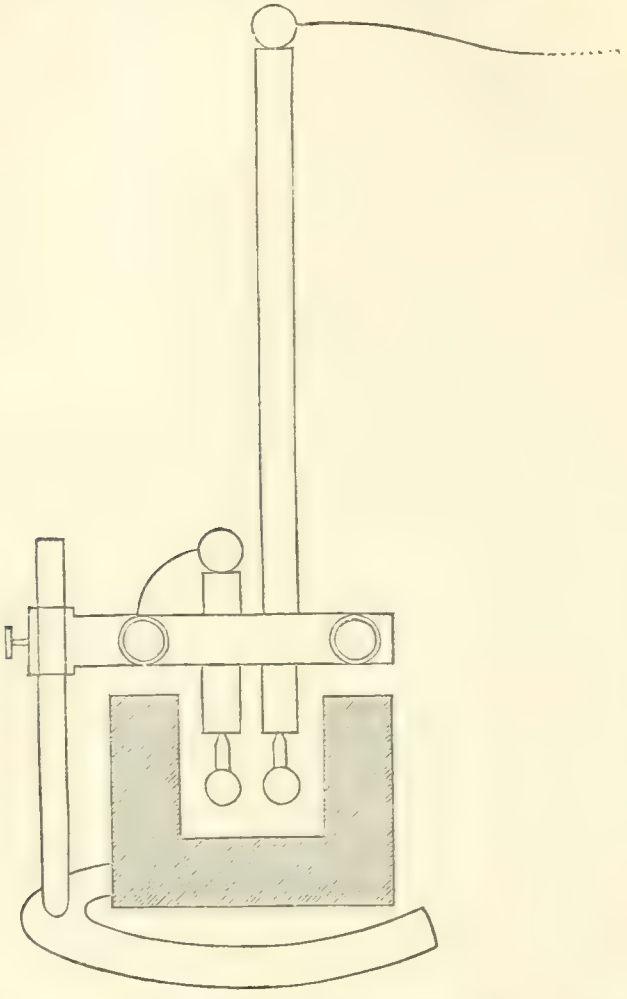
(3) When this method fails, the power is increased by the addition of a Leyden jar, whose knob and outer coating are connected respectively with prime conductor and earth. The machine is set in motion, and the discharging balls are brought into contact, first at every turn of the plate, then at every second turn, and so forward, till the optical effect is brought out with sufficient intensity. There are many liquids (butyl and propyl alcohols, propionic and acetic acids, &c.) which give pure and strong restorations at and above some point of this process, but no trace of effect without the jar.

(4) When these methods fail, the Winter's plate machine is abandoned, and a Ruhmkorff's coil is applied. The coil is worked by six large Grove's elements in series, and gives a powerful spark (8 to 10 inches in air). The cell is sometimes inserted in the circuit of the secondary coil by unbroken wires; in other cases, there are air-intervals left at the outer ends of the cell, which are traversed by spark discharges. It is found to be generally advisable, and sometimes necessary, to dispense with the automatic rheotome, and to work the instrument by hand, so as to obtain separate discharges at intervals of one or more seconds. With all precautions, the discharges are often too strong for perfect purity of electro-optic action, producing large disturbances in the liquid, and giving rise in a little time to intense heat-effects. There are several liquids (ethyl alcohol, distilled water, &c.) which have given good results by this method only.

2. *The Fusion-cell*, a small instrument similar to the plate cell, but adapted to higher temperatures, is represented in the adjacent diagram.

The shaded piece is a square of plate glass, half an inch

thick, with a square space worked out of it as shown in the figure. In contact with this frame, and covering its two faces, are two panes of thin plate glass, which are kept closely and firmly united to the frame by a small metallic press. The conductors in the cell are two balls of brass, electroplated with platinum, and attached each to a stiff wire, which passes upwards through an insulating tube of glass. These tubes



are supported by a stand (shown in the figure). The cell and stand are placed on an iron tripod, and enveloped by a wide muffle of sheet iron, with an aperture at the top for the long conductor, and two apertures at the sides for transmission of the light. Under the floor of the tripod is an Argand gas-burner, which supplies a current of hot air to the interior of the muffle.

In the course of some preliminary experiments on the fats and waxes, the fusion-cell was found to be not nearly so delicate as the plate cell, both instruments working under ordinary conditions—the former at high temperatures, the latter at the

temperature of the laboratory. The conditions of experiment with the fusion-cell are always in some degree unfavourable; so that a moderately strong effect obtained with this instrument may be accepted safely as an evidence of high electro-optic power in the dielectric. This remark applies to several important bodies (phosphorus, cetyl and cinnyl alcohols, spermaceti, &c.), which have given good effects in the fusion-cell.

Elementary Bodies.

3. Bromine, phosphorus, and sulphur, all in the liquid state, are found to be electro-optically active and purely positive.

Bromine was examined in the plate cell without effect; but this trial was indecisive, as the plate of liquid was almost opaque to the sunbeam. I therefore had a special cell constructed, shaped like the fusion-cell, but only a quarter inch thick. As inductive terminal I used the long conductor of the fusion-cell, supported by its proper stand; the other conductor was a rounded segment of a disk of brass, resting on the floor of the cell, and connected by a lateral wire to earth. All the metallic pieces were plated with platinum; and the fumes from the liquid were sufficiently confined by a covering of soft leather pressed lightly on the mouth of the cell. The optical conditions were now very favourable. The image of the sun in the mirror of a *porte-lumière*, viewed through the centre of the electric field, was of course deeply coloured and much weakened; but it still presented a fine object in the polariscope: the extinction also was pure and sharp; and the object was well restored by a very feeble strain of the hand-compensator.

Tried as a nonconductor, the plate of bromine gave no optical effect; nor did it give any sensible spark in the test for insulation. Tried then as a conductor, with moderate charges of the jar, it gave a regular and very fine restoration, vivid and undistorted, and not at all abrupt. When tested by the hand-compensator, the effect was found to be purely positive, strengthened by tension parallel to the lines of force, and weakened to extinction by compression.

Phosphorus was examined in the fusion-cell, at a temperature a little above its melting-point. Several trials were made before a sufficiently clean plate was obtained; but I succeeded at last, by pressing out the phosphorus through a bag of chamois-leather, under hot water. The cell, charged nearly full with phosphorus, and then filled up with water, was placed in the hot-air bath, between the two Nicols; and the experiment was carried forward as in the case of bromine. The conditions of optical observation were unfavourable, the cell-panes being strongly and not very regularly strained by the heat.

The initial extinction was very slow and impure ; but it was perfected by careful regulation of the temperature, and by the application of a stationary neutralizing plate of glass. Like difficulties occurred in most of the experiments with the fusion-cell, and they were generally overcome in the same way.

Tried as a nonconductor, the plate of phosphorus gave no optical effect. Tried then as a conductor, with moderate charges of the jar, it gave a good restoration,—not so strong, I thought, as that of bromine, but equally regular and pure, strengthened by tension parallel to lines of force, and weakened to extinction by compression. Phosphorus therefore, like bromine, is purely positive.

Judging from some facts that presented themselves in the course of my experiments with fused phosphorus, I am almost sure that, with better apparatus and proper arrangements, this body would give a good optical effect as a nonconductor.

Sulphur.—The fusion-cell was placed on its tripod between the crossed Nicols, and all the pieces were put in final position as for the optical experiment ; the cell was then filled with sublimed sulphur, and the temperature of the air-bath was raised gradually. As the sulphur melted, new quantities were supplied till the cell was nearly full of liquid. The initial extinction was now restored as quickly as possible by the use of a thick neutralizing plate of glass placed in front of the second Nicol, and the sulphur was tried as a nonconductor. When the machine was set in motion, the flame was restored strongly from extinction; and the effect was found to be purely positive, strengthened by tension parallel to lines of force, and extinguished by compression. As the experiment went on for a minute or more, the effect became weaker and weaker till it was imperceptible. The insulation had evidently failed, the temperature being too high for the apparatus.

The Leyden jar being now introduced quickly, the sulphur was tried as a conductor, and there was a strong restoration obtained by discharge at every fifth turn of the plate. The effect was still purely positive, strengthened by tension parallel to lines of force, and extinguished by compression. In a few minutes this effect also disappeared ; and it could not be recovered by regulation of the temperature. In the meantime the plate of sulphur had deteriorated very much from its first purity. Perfectly clean at first, it was now filled with black particles.

After this first experiment, I made several attempts to obtain a more constant effect, but without success. The conditions of the experiment were too trying for the apparatus : the cell was damaged in the first experiment, and was very soon de-

stroyed. Although I never recovered any thing like the very strong and fine effects which I have described, I have no doubt that fused sulphur is purely and strongly positive.

Hydrocarbons.

4. Those of the hydrocarbons that I have examined up to this time, the nine or ten mentioned in my former paper, and the eight which here follow*, are all nonconductors, electro-optically active, and purely positive.

Amyl hydride, C_5H_{12}, is interesting as one of the lightest of known liquids. Tested for insulation in the plate cell, it gave a spark hardly $\frac{1}{4}$ inch long. Tried optically as a non-conductor, it gave a sure but extremely faint restoration from pure extinction. When the hand-compensator was introduced beforehand, and strained steadily, the effect of electric force was brought out more strongly, and with great purity—an increase of light in the case of tension parallel to lines of force, and a decrease to extinction in the case of compression. There was no increase of effect when the liquid was tried as a conductor, with or without the Leyden jar.

Hexyl hydride, or trityl, C_6H_{14}. For a specimen of this liquid, prepared from petroleum-oil, I am indebted to the kindness of Dr. Gladstone. It was examined immediately after amyl hydride, for the purpose of comparison. Of the two, the hexyl compound appeared to be somewhat stronger both in insulating and in electro-optic power. Both bodies exert very faint but perfectly pure actions as positive non-conductors.

Cinnamol, C_8H_8, tried in the plate cell as a nonconductor, gave an intense and purely positive effect. The action was strong enough to give definite and very black extinction-bands, like those in my old experiments with CS_2 and the stronger hydrocarbons.

Caprylene, C_8H_{16}, acts in the plate cell as a positive non-conductor. Most of the plates of this liquid were somewhat dirty, and easily traversed by spark discharge; but the effect was always manifest, and always purely positive. In the cleaner plates the action was very fine, and apparently a good deal stronger than that formerly observed in the lighter olefine, amylene.

Caoutchoucin, or empyreumatic oil of caoutchouc.—My specimen of this liquid was of a deep red colour, and a moderately good insulator; so that it could be tried in the plate cell

* All of them pure hydrocarbons, except caoutchoucin and oil of resin, which contain a little oxygen.

as a nonconductor. The optical conditions were unfavourable, and the effect of electric force could hardly be detected from pure extinction; but when the hand-compensator was used (as with amyl hydride formerly), the effect came out very clearly—an increase of light in the case of tension parallel to lines of force, and a decrease to sensible extinction in the case of compression.

Oil of Resin is interesting for its great density, ·9989. It is a distillate of common resin, redistilled from caustic potash, and is now largely used in the jute-manufacture. My specimen was a very clean trade-sample, of a clear amber-colour, a pretty strong photogyre, and a good insulator. Tried in the plate cell as a nonconductor, it gave a strong positive effect. As soon as the machine was set in motion, the light was restored from a faint red or blue, which was the best extinction, to an intense and sensibly pure white; and the effect was strengthened by tension parallel to lines of force, and weakened to good extinction by compression. This result agrees with my former observations on solid resin, where also the effects were positive, though very much fainter.

Paraffin fuses into a purely transparent liquid at about 50° C. Tried in the fusion-cell as a nonconductor, it gave a strong regular effect. A quarter turn of the plate restored the light well from extinction; and the effect was strengthened by tension parallel to lines of force, and neutralized by compression. The sluggishness of rise and fall of optical effect was very manifest. There was also some disturbance, produced by movements in the liquid, when the machine was in motion.

Naphthaline, $C_{10}H_8$, fuses at a moderate temperature into a transparent and very volatile oil. Tried in the same way as paraffin, it acted very similarly. There was a greater disturbance by gross movements in the liquid; but the optical effect was regular and perfectly distinct, conspiring with tension parallel to lines of force, and neutralizing compression.

Alcohols, $C_n H_{2n+1} HO$.

5. Of this series, the eight members corresponding to the following values of n were examined in order:—

16, 8, 5, 4, 3, 2, 1, 0.

The first six are purely negative, falling in power perceptibly and regularly from first to sixth; the seventh and eighth are purely but feebly positive.

Cetyl alcohol ($n=16$) fuses into a transparent and nearly colourless plate, at or near 50° C. It was tried in the fusion-cell as a nonconductor, after the initial extinction had been

perfected by careful regulation of temperature and by the use of a neutralizing plate. One turn of the machine gave a strong restoration in the polariscope; and the effect was purely negative, being neutralized by tension of the hand-compensator in a direction parallel to the lines of force, and regularly strengthened by compression. Good results of the same kind were obtained with half a turn of the plate, and even with a quarter turn. When the electric force was intense, there was some disturbance produced by movements in the liquid. When the temperature was raised very high, the regular effect disappeared, probably from failure of the insulation; and when the temperature was lowered to the point of solidification, and particles began to appear in the liquid, the optical effect became irregular and very weak, and (on the whole) apparently positive. But when these exceptional temperatures were avoided, the action of this alcohol was seen to be constantly, strongly, and purely negative. All the following members of the series were examined in the plate cell, and at ordinary temperatures.

Caprvl alcohol ($n=8$) gave no spark in the test for insulation, and was inactive accordingly when tried as a nonconductor. Tried as a conductor, with the jar and two or three turns of the plate, it gave a brilliant effect, which was found to be purely negative, being strengthened by compression of the hand-compensator in a direction parallel to the lines of force, and neutralized by tension, weakened in fact from a high intensity down to perfect extinction. Tried then without the jar, by discharge at every turn or half turn of the plate, it gave a clear and pretty strong effect, always purely negative by both tests.

Amyl alcohol ($n=5$) is weaker than the last, both in insulating and in electro-optic power; for when tried as a conductor, without the jar, it gave a mere suspicion of effect. Tried then with the jar, and two or three turns of the plate, it gave a pretty strong effect, which was found to be purely negative by one test, being strengthened by compression parallel to lines of force, but not sensibly affected by tension. The failure of the second test, weakening by tension, was evidently due to the great abruptness of the electro-optic action, the momentary darkening of a steady flame being less perceptible than a momentary brightening. If any proof of the truth of this explanation were required, it could be supplied by a simple form of experiment, which I may describe further on.

Butyl alcohol ($n=4$) is weaker than the last. Tried with the jar, it gave a very good restoration by discharge at every tenth turn of the plate. Five turns, powerful formerly with

amyl alcohol, gave now no sensible effect. The electro-optic action was found to be purely negative, conspiring regularly with compression parallel to lines of force, and giving no sensible change (except an apparently instantaneous tremor of the flame) with tension.

Propyl alcohol ($n=3$) shows an additional fall of power. Tried with the Leyden jar, it gave a good restoration, though not strong, with fifteen turns of the plate. Ten turns, powerful with butyl alcohol, gave now no sensible restoration from extinction. The effect was purely negative, strengthened always by compression parallel to lines of force, and not sensibly affected by tension.

Ethyl alcohol ($n=2$), specific gravity ·795, showed a large fall of power. With plate machine and strongest charges of the jar, it gave no sure trace of effect. With the large Ruhmkorff's coil, working in the usual way with oscillating rheotome, it gave a strong heat-effect almost at once. The rheotome was therefore worked by hand, separate discharges being taken at intervals of 1 or 2 seconds. At each discharge, there was a perfectly regular, but very faint, restoration from pure extinction. Tested by the hand-compensator with great care, the electro-optic action was found to be purely negative, regularly neutralized or much weakened by tension parallel to lines of force, and regularly strengthened by compression.

Methyl alcohol ($n=1$), tried with strongest charges of the jar, gave an effect which was clear and regular, but too feeble to be characterized. Tried with separate discharges of the coil, it gave a faint restoration regularly from pure extinction. Tested by the hand-compensator with great care, the effect was strengthened regularly by tension parallel to lines of force, and neutralized or much weakened regularly by compression; so that this alcohol, contrarily to the preceding six, is distinctly and purely positive. As far as I could judge, the action was somewhat stronger than the contrary action of ethyl alcohol.

Distilled water ($n=0$), tried with strongest charges of the jar, gave a clear effect, apparently better than that in methyl alcohol, but still too faint to be characterized. Tried with separate discharges of the coil, it gave a faint restoration regularly from pure extinction. The effect was found to be purely positive, strengthened by tension parallel to lines of force, and neutralized or much weakened regularly by compression. I thought the action rather stronger than that of methyl alcohol, and certainly not weaker. The experiment with one charge of water could not be continued well for any length of time, as the electro-optic field was obscured very soon by an ascending cloud of gases.

Acids, $C_n H_{2n} O_2$.

6. Of the series of fatty acids, the seven members corresponding to the following values of n were examined in the plate cell at ordinary temperatures:—

8, 6, 5, 4, 3, 2, 1.

The first six are purely positive, the seventh feebly but purely negative ; and they fall regularly in power from the first to the last.

Caprylic acid ($n=8$), tried as a nonconductor, gave a faint but good restoration regularly from extinction. As a conductor, its action was much stronger ; and when the jar was used, the effect of discharge at every second or third turn of the plate was very brilliant. The effect from weak to strongest was purely positive, strengthened by tension parallel to lines of force, and extinguished perfectly by compression.

I never obtained this liquid, nor indeed any of the following acids, in a state of perfect (optical) purity. After any amount of filtering, there were always some visible specks left, solid particles apparently of the acid itself, or of some chemically homologous matter.

Caproic acid ($n=6$) is distinctly weaker than the last, but otherwise hardly distinguishable from it in experiment. As a nonconductor, it gave no satisfactory effect in the polariscope. Tried as a conductor, with or without the jar, it gave a fine restoration, which was very brilliant with strong discharges ; and the effect, weak or strong, was always purely positive, strengthened by tension, and extinguished by compression.

Valeric acid ($n=5$) acts as a weak photogyre, barely separating the red and blue at extinction. Tried as a conductor, without the jar, it gave a regular effect, which was merely strong enough to be characterized as purely positive. With the jar it gave a series of brilliant effects, always purely positive by both tests.

With either of these three acids, it is easy to obtain a large range of optical effects, from very intense to evanescent, in one experiment. The jar is well charged, say by twenty turns of the plate, and the discharging balls are brought at short intervals into instantaneous contact, so as to discharge the jar gradually and discontinuously through the cell. Working in this way with valeric acid, and with one moderate charge of the jar, I have counted up to about fifty good restorations in the polariscope.

Butyric acid ($n=4$), tried as a conductor, but without the jar, gave an effect which was too faint to be characterized, showing a distinct fall of power in comparison with the last

acid. With the jar and two turns of the plate, the restoration was good, though still faint; with ten or fifteen turns it was brilliant. The effect was always strengthened by tension parallel to lines of force, and always extinguished perfectly by compression.

Isobutyric acid, examined immediately afterwards in the same way, acted very similarly, but was apparently stronger.

Propionic acid ($n=3$) showed an additional fall of power—gave no clear effect without the jar and five or more turns of the plate. The effect of discharge at every tenth turn was moderately strong, and was found to be purely positive by both tests.

Temperature was observed to have a great influence on the action of this acid. In one experiment, the initial temperature happened to be about 0° C., and there was no effect obtained with less than twenty turns of the plate; but when the cell was now gently heated by exposure for a minute in front of the fire, the effect of discharge was certain at every third turn, and good at every fifth.

Glacial Acetic acid ($n=2$) showed an additional fall of power. With the jar and ten turns of the plate, it gave an effect so faint as to be often uncertain. With fifteen turns, the restoration was clear and regular, though not yet strong. Tested by the hand-compensator at this or higher power, the effect was strengthened regularly by tension parallel to lines of force, but hardly or not at all affected (and certainly not strengthened) by compression. Acetic acid is seen thus to be purely positive, and a very weak insulator. The second test by the hand-compensator (neutralization by the proper strain), fails here, as it failed earlier (with Leyden discharges) in the series of alcohols.

Crystallizable Formic acid ($n=1$) showed a large additional fall of power, with undoubted reversal of sign. Tried with plate machine and strongest charges of the jar, it gave an effect which was clear and perfectly regular, though very faint and abrupt. The light was now restored steadily and very faintly from extinction, by permanent strain of the hand-compensator; and the observations were repeated. In the case of compression parallel to lines of force, the effect was regular and very distinct; the light brightened sensibly for a moment at the instant of discharge. In the case of tension parallel to lines of force, the effect was not so regular: at the instant of discharge the light was sensibly and very abruptly darkened in a few instances, never certainly strengthened, and generally affected merely as by an instantaneous shock, without seizable change of intensity. Like results, though hardly so good, were obtained

afterwards with the Ruhmkorff's coil. Formic acid is therefore purely negative, but a very weak insulator. It gives no good effect at temperatures very near the freezing-point; and the same remark applies to acetic acid.

7. Two of the so-called solid acids of the same series were examined in the fusion-cell, and were found to be negative.

Palmitic acid ($n=16$), a transparent oil at or above 62° C., acted well as a nonconductor. One turn of the plate gave an intense restoration, whose purely negative character was very little disturbed by movements in the liquid. When the temperature was little above the point of fusion, and carefully regulated, the initial extinction was very good without the neutralizing plate, and there was no trace of disturbance by currents. One eighth of a turn gave then a good effect which was purely negative, strengthened by compression parallel to lines of force, and extinguished by tension.

Stearic acid ($n=18$), a transparent oil at or above 70° C., acted on the whole as well as the last, though there was rather more disturbance by currents. One turn of the plate gave always a strong effect which was purely negative, conspiring with compression parallel to lines of force, and neutralizing tension.

These results were very unexpected. I therefore tested them at some length, working with new samples of the acids, the purest that could be obtained, and comparing them successively with paraffin and the fats. The facts never varied: the two fused acids were always distinctly and purely negative.

Other Alcohols and Acids.

8. *Allyl alcohol*, $C_3 H_5 HO$, is distinctly though not quite purely negative, and a good deal stronger than ethyl alcohol. With good charges of the jar it gave a clear effect, which was strengthened by compression parallel to lines of force, and weakened almost to extinction by tension. There was always some disturbance produced, partly by a feeble photogyric action of the liquid, and partly by a special effect of electric discharge. The flame was not simply restored in the polariscope by discharge, but was deformed as by an undulatory movement in the liquid, and rendered also fibrous or thready in appearance. This effect was probably due to heat; for it came out much more strongly afterwards with the Ruhmkorff's coil, in which case three separate discharges were sufficient to mask the electro-optic action completely by a permanent heat-effect.

Benzyl alcohol, $C_7 H_7 HO$, is purely negative. Tried in the plate cell as a conductor, it gave a faint but good restoration

with five turns of the plate, strong with ten turns; and the effect was purely negative.

Cinnyl alcohol, C_9H_9HO, is purely negative. Tried in the fusion-cell at a temperature a little above its melting-point, 33° C., it acted well as a conductor. With ten turns of the plate it gave a clear and pretty strong effect, which was found to be purely negative by both tests.

Glycol, $(C_2H_4)''H_2O_2$, is very feebly and impurely negative. This is a very inconvenient liquid to work with, viscous and heterogeneous, giving permanent deformations of the flame, and impure initial extinction in the polariscope. With strongest charges of the jar, it gave a mere trace of effect, which appeared to be negative. With the strongest Ruhmkorff-discharges, the restorations were clearer, and almost perfectly regular, but still faint, and obscured by disturbance. The effect was pretty surely negative, strengthened sensibly by compression parallel to lines of force, but not extinguished by tension.

Glycerin, $(C_3H_5)'''H_3O_3$, is certainly, but feebly and impurely, negative. As far as I can judge from my samples, this liquid is more unsuitable even than glycol for delicate optical work. With the Ruhmkorff's coil, it acted better than glycol, giving very good retorations, notwithstanding great disturbance produced by the syrupy structure of the plate. The effect was found to be certainly negative, though the neutralizing action of tension was not always seized.

Phenol, C_6H_5HO, is purely positive. In the fusion-cell, at or above 35° C., it acted well with the plate machine and without the jar, but required several turns of the plate. There was generally some optical disturbance by heterogeneous structure, and also by movements in the liquid, when the electric force was intense; but the optical effect was regular and very distinct, purely positive by both tests.

Oleic acid, $C_{18}H_{34}O_2$, is purely and strongly positive. Tried in the plate cell as a non-conductor, it acted more intensely than any of the fatty acids—gave a regular and pretty strong effect, which was strengthened by tension parallel to lines of force, and extinguished perfectly by compression.

Lactic acid, $C_3H_6O_3$, is apparently positive. Tried with strongest charges of the jar, and afterwards with the coil, it gave a regular effect, undoubtedly pure, and pretty surely positive, but too faint and abrupt to be characterized with perfect certainty. The liquid was evidently a very weak insulator.

Oxides, $(C_nH_{2n+1})_2O$.

9. The only two of these that have been examined are purely negative, and much stronger than the corresponding alcohols.

Ethyl oxide (purus, P.B.), tried as a conductor with machine and Leyden jar, gave a good restoration with eight turns of the plate, pretty strong with ten turns; and the effect was strengthened by compression parallel to lines of force, and regularly neutralized by tension. With the Ruhmkorff's coil afterwards, there was a good effect obtained even when the length of the safety spark was reduced to zero; and the action of the compensator was as clear as formerly, extinction by the proper tension being regularly perfect. This liquid is therefore purely negative, and a great deal stronger than ethyl alcohol.

Amyl oxide, tested for insulation in the plate cell, gave a strong spark ($\frac{3}{4}$ inch) from the prime conductor. Tried optically as a nonconductor, it acted like the best of the fixed oils. As soon as the machine was set in motion, there was a strong effect from extinction, which was neutralized perfectly by tension parallel to lines of force. Among chemically definite bodies, I regard this amylic ether as the best negative dielectric yet discovered.

Iodides, $C_nH_{2n+1}I$.

10. Four of these were examined in the plate cell, first as nonconductors, but without effect, then as conductors. The regular effects were disturbed in some degree by photogyric action, but were all clearly positive.

Methyl iodide, without the jar, gave a good restoration, which was too quick to be characterized. With the jar and five or four turns of the plate, the effect was distinctly positive, strengthened by tension parallel to lines of force, but not sensibly affected by compression.

Ethyl iodide was of a deep red colour, much darker than the methyl compound, but otherwise not distinguishable from it in experiment.

Butyl iodide, still deeper in colour, and almost opaque, required the jar and fifteen or more turns of the plate. The effect was regularly strengthened by tension parallel to lines of force, but not sensibly affected by compression.

Amyl iodide was the strongest of the four. With the jar and three or even two turns of the plate, it gave a clear effect, which was strengthened by tension parallel to lines of force, and weakened to extinction by compression.

Bromides, $C_nH_{2n+1}Br$.

11. These are purely positive, the powers rising as n increases; and they are distinctly stronger than the corresponding iodides.

Ethyl bromide, tried as a conductor with the jar, gave a clear

but faint restoration with five turns of the plate, strong with ten turns; and the effect was purely positive by both tests.

Propyl bromide, as a conductor, but without the jar, gave a good effect which was positive by one test, clearly strengthened by tension parallel to lines of force, but not sensibly affected by compression. With the jar, one turn of the plate gave a strong effect, which was purely positive by both tests.

Isopropyl bromide acted more strongly, giving a clear effect as a nonconductor. Tried as a conductor, with the jar, it gave increasingly brilliant restorations with two, five, ten, or more turns; and the effect from weak to strongest was purely positive, strengthened by tension, and extinguished by compression.

Butyl bromide, not a very clean sample, was hardly so strong as the last, giving a mere trace of effect as a nonconductor. Tried as a conductor, with the jar, it gave a good restoration with one turn, strong with three turns; and the effect was purely positive by both tests.

Amyl bromide, tried as a nonconductor, was quite inactive at first, giving no trace of effect in the polariscope, no sensible spark from the prime conductor; but when the machine had worked for a little time, the optical effect came out clearly, and was found to be purely positive. Tried then as a conductor, with the jar, it gave a splendid restoration with one turn of the plate. When the jar was detached, well charged, and again connected for discharge through the cell, the light flamed up in the polariscope with great brilliancy, and faded away gradually, remaining visible for sixty or more seconds. The increase of insulating and electro-optic powers of the dielectric in the course of this experiment, and in a few minutes of time, was very remarkable.

Chlorides, $C_nH_{2n+1}Cl$.

12. These are purely positive, and stronger than the bromides; and they show a great increase of power as the experiment proceeds.

Propyl chloride, tried in the plate cell as a nonconductor, gave at first no trace of effect in the polariscope, no sensible spark from the prime conductor. Tried then as a conductor, it gave a clear and strong restoration at each discharge. When the jar was added, one, two, three, or more turns gave increasingly brilliant effects, which were found to be purely positive by both tests. When the observations had been carried on for some time, the liquid was tried again as a nonconductor. The prime conductor gave now a spark fully a quarter of an inch long; and the polariscope gave a

fine effect, purely positive. The final action was at least as strong as that of amylene. My sample of this liquid was not very clean; and the initial extinction was far from pure.

Butyl chloride was rather weaker than the last, and showed no great increase of power with time. As a conductor, without the jar, it gave a good restoration which was too abrupt to be characterized; with the jar and four or five turns of the plate, it gave a very strong effect, purely positive by both tests. My sample was not clean; and the initial extinction was quite impure.

Amyl chloride, a very fine optical medium, giving perfect initial extinction. As a nonconductor, it gave at first no trace of effect in the polariscope, no spark from the prime conductor. Tried then as a conductor without the jar, it gave a clear restoration, too abrupt at first to be characterized; but as the experiment went on, the machine working at a moderate rate, and discharges passing regularly through the cell, the restorations in the polariscope rose very gradually in intensity, till at last the effect was one of the finest that I had ever seen. The liquid being now tried again as a non-conductor, the prime conductor gave a spark more than half an inch long, and the electro-optic action was magnificent, as fine apparently as that of CS_2. The effect was always strengthened by tension parallel to lines of force, and always weakened to extinction by the proper compression.

[To be continued.]

XIX. *Researches on Chemical Equivalence.*—I. *Manganous and Nickelous Sulphates. By* Edmund J. Mills, *D.Sc., F.R.S., and* J. H. Bicket*.

THE following experiments were conducted with the view of determining on what terms manganous and nickelous sulphates might prove to be mutually equivalent; and the particular equivalence we have examined has been equivalent precipitability of the sulphates, by sodic carbonate, from an aqueous solution.

I. *Preparation of the Salts.*

The pure manganous sulphate was prepared from the ordinary commercial dioxide. The oxide was dissolved in hydric chloride, the solution filtered and evaporated to dryness, the residue dissolved in water, and excess of baric carbonate added

* Communicated by the Authors.

to precipitate the iron; the barium was separated by hydric sulphate; and the manganese was precipitated from the solution by means of sodic carbonate. The manganous carbonate thus obtained was well washed with water, dissolved in hydric chloride, and baric carbonate again added in order to precipitate any remaining traces of iron. After the barium had been removed with hydric sulphate, the manganese was again thrown down as carbonate with sodic carbonate. The manganous carbonate, after being well washed and dried, was roasted; and the oxide obtained was washed with dilute hydric nitrate to remove any sodic salt. As the oxide obtained was very slightly acted on by hydric sulphate at any temperature or of any strength, we could not obtain the sulphate by the direct action of hydric sulphate on this oxide. The oxide was therefore dissolved in hydric chloride, the solution evaporated to dryness, and ignited with excess of strong pure hydric sulphate. This ignition with hydric sulphate having been repeated, the salt was found to be free from all impurities, and quantitative analysis showed it to be pure anhydrous manganous sulphate.

The pure nickelous salt was prepared from a sample of nickelous chloride which contained copper, lime, and iron, but no cobalt. The copper present was precipitated with hydric sulphide; and the nickel in the filtrate was precipitated by hydric oxalate in an acid solution. The nickelous oxalate was washed thoroughly with dilute hydric nitrate, ignited, and the oxide so obtained heated with pure hydric sulphate and so converted into sulphate.

The sodic carbonate was prepared by igniting hydrosodic carbonate in a double platinum crucible, dissolving in water, and reprecipitating with carbonic dioxide, igniting this as before, and repeating the operation. The final precipitate was dissolved in boiling water to the necessary strength.

II. *Method of separating Nickel from Manganese.*

The method for the separation of manganese from nickel that we found to give the most accurate results was that of Beilstein and Jawein* for the separation of manganese from iron. It consists in evaporating the mixed solutions to dryness, adding strong pure hydric nitrate, heating to boiling, and adding potassic chlorate in small quantities at a time. The manganese is thus precipitated as pure black oxide; which, after filtering and washing free from the nickel that is in solution, is dissolved in hydric chloride, precipitated with sodic phosphate, and ignited and weighed as manganous pyrophos-

* *Deut. chem. Ges. Ber.* 1879, p. 1528.

phate. The nickel solution is evaporated to dryness (to get rid of the large excess of acid), dissolved in water, and precipitated as sulphide with ammonic sulphide. The sulphide is then ignited with excess of pure hydric sulphate in an open porcelain crucible, and weighed as nickelous sulphate.

In experiments that were tried to ascertain the accuracy of this method, the following results were obtained:—Two solutions, each containing 0·2500 grm. manganous sulphate and 0·2500 grm. nickelous sulphate, were taken; the manganese was separated by the method given above, with the following results:—

(*a*)	Manganous sulphate obtained .	=0·2506
	Nickelous sulphate	0·2503
(*b*)	Manganous sulphate obtained .	0·2497
	Nickelous sulphate	0·2517
	Mean	0·2506

Having now fixed on the method for separation that was to be used, we made up solutions of the salts required.

For the manganous and nickelous sulphates, 1-per-cent. solutions were made. The sodic carbonate solution was of 5·7151 per cent. strength. It was kept in a stoppered bottle that had been well coated internally with paraffin.

III. A. *Conjoint Precipitability.*

A series of nine experiments was made, in which the relative weights of nickelous or manganous sulphate present varied from ·1 to ·9 grm.,—the total weight of nickelous and manganous salt and the volumes of the solution being, however, always the same, viz. 1 grm. and 100 cub. centim. The experiments were conducted as follows:—The bottles containing the solutions of the sulphates and the sodic carbonate were immersed in a tank, through which there was a constant flow of water from the main to bring them to a constant temperature. The necessary quantities of nickelous and manganous solutions were then carefully measured out, mixed, and the temperature observed. 10 cub. centim. of sodic carbonate solution were then added, the mixture stirred, and the temperature again observed. The precipitate was then filtered off as quickly as possible* (an aspirator being used to facilitate the filtration) and washed, first with cold and then with hot water.

* Continued experience with precipitation experiments has convinced us that our probable error would have been less if we had allowed the precipitate to stand some time.

The washing of the precipitate took a long time (a maximum of two days), especially when the nickelous salt was in excess of the manganous. After the precipitate had been washed, it was dissolved in dilute hydric chloride, and the manganese and nickel separated and estimated by the method given above.

In the course of these experiments we noticed that, (1) if we kept the clear filtrate in a glass-stoppered bottle for some time, or boiled it, a further precipitate appeared; and (2) that the clear filtrate was alkaline to test-paper, turning red litmus blue.

The results obtained in these experiments are comprised in the following table ("temperature" meaning temperature of the reagents *after* mixture):—

TABLE I.

Nickelous salt taken. *n.*	Manganous salt taken. *m.*	Nickelous salt precipitated. *ν.*	Manganous salt precipitated. *μ.*	Temperature.
grm.	grm.	grm.	grm.	
·1000	·9000	·0953	·5850	12·9 C.
·2000	·8000	·1852	·4616	13·6
·3000	·7000	·2799	·3766	12·5
·4000	·6000	·3588	·2976	13·0
·5000	·5000	·4305	·2450	13·6
·6000	·4000	·4788	·1536	12·8
·7000	·3000	·4991	·1089	17·0
·8000	·2000	·5584	·0722	17·0
·9000	·1000	·5841	·0363	15·2

III. B. *Separate Precipitability.*

We next tried the following experiments to determine the precipitability of manganous and nickelous sulphates when separate. Four series were undertaken, viz. (1) when the nickelous salt varied and the sodic carbonate was constant, (2) nickelous salt constant and sodic carbonate varying, (3) manganous salt varying and sodic carbonate constant, (4) manganous salt constant and sodic carbonate varying.

(1) *Nickelous Salt varies, Sodic Carbonate constant.*—The bottles containing the solutions were brought to a constant temperature by immersion in a tank of running water. After the solution of nickelous sulphate had been measured out, it was made up to 100 cub. cent. with distilled water that had been cooled to the same temperature, and then 10 cub. cent. of the sodic carbonate solution was added, the mixture stirred,

and the temperature observed. The precipitate was then filtered (with the help of an aspirator), and washed with cold and afterwards with hot water until the washings showed no trace of nickel; the precipitate was then dissolved in pure hydric sulphate, evaporated to dryness, ignited, and weighed as nickelous sulphate. The results obtained in these experiments are given in the following table:—

TABLE II.

Sodic carbonate taken.	Nickelous salt taken. x.	Nickelous salt (obtained) precipitated. y.	Nickelous salt (calculated) precipitated.	Temperature.
grm.	grm.	grm.	grm.	°
0·57151	0·1050	0·1015	·1192	5·6 C.
,,	0·2100	0·1912	·2139	4·8
,,	0·5000	0·4084	·3967	7·3
,,	0·7100	0·5103	·4856	4·8
,,	1·0000	0·5550	·5745	5·6

(2) *Nickelous Salt constant, Sodic Carbonate varies.*—This series was conducted in the manner of (1); only there was always the same volume of nickelous sulphate measured out, viz. 100 cub. cent. To this was added enough distilled water to make, along with the volume of sodic carbonate to be added, 10 cub. cent. The results thus obtained are given in the following table:—

TABLE III.

Sodic carbonate taken. x.	Nickelous salt taken.	Nickelous salt (obtained) precipitated. y.	Nickelous salt (calculated) precipitated.	Temperature.
grm.	grm.	grm.	grm.	°
0·057151	1·000	0·0300	0·0393	7·8 C.
0·114302	1·000	0·0798	0·0810	8·1
0·285755	1·000	0·2311	0·2242	7·9
0·571510	1·000	0·5438	0·5450	7·9

(3) *Manganous Salt varies, Sodic Carbonate constant.*—The manganous sulphate was measured out, the solution made up to 100 cub. cent. with distilled water, and 10 cub. cent. of sodic carbonate solution added; the mixture was then stirred, and the precipitate filtered off, washed, and dissolved in pure hydric chloride. The manganese was estimated as pyrophosphate. The results were as follows:—

Table IV.

Sodic carbonate taken.	Manganous salt taken. x.	Manganous salt (obtained) precipitated. y.	Temperature.
grm.	grm.	grm.	°
0·57151	0·1050	0·1039	9·3 C.
,,	0·2000	0·1969	9·3
,,	0·5000	0·4894	10·1
,,	0·7100	0·6748	10·1
,,	1·0000	0·7500	9·7

(4) *Manganous Salt constant, Sodic Carbonate varies.*—This series was conducted like series (2). The washed precipitate was dissolved in hydric chloride and the manganese estimated as pyrophosphate. The results are given in the following table :—

Table V.

Sodic carbonate taken. x.	Manganous salt taken.	Manganous salt (obtained) precipitated. y.	Manganous salt (calculated) precipitated.	Temperature.
grm.	grm.	grm.	grm.	°
0·057151	1·0000	0·0400	·0577	7·6 C.
0·114302	1·0000	0·1145	·1188	7·6
0·285755	1·0000	0·3558	·3261	7·8
0·57151	1·0000	0·7592	·7797	9·2

Discussion.

Conjoint Precipitability.—If n represent nickelous sulphate taken, and ν the carbonate (calculated to sulphate) obtained from it by precipitation, then ϕ in the expression $n=\phi\nu$ is the precipitability. Our experiments (Table I.) satisfy the equation

$$\phi = 98570 + \cdot 61438\, n. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

(which is deduced from all of them), with a probable error of ·055284 for a single determination, or ·018428 for nine determinations.

Similarly, in the case of manganous sulphate (Table II.),

$$\phi = 3\cdot 0881 - 1\cdot 7203\, m\,; \quad . \quad . \quad . \quad . \quad . \quad (2)$$

with probable error amounting to ·088893 and ·029631 respectively. In both cases, therefore, the precipitability is a linear function of the mass. The second constants in equations (1) and (2) present an interesting relation, closely resembling that

which we have elsewhere* shown to hold good for nickelous with cobaltous sulphate. Within the limits of error of experiment, the second constant in (1) is the reciprocal of the second constant in (2). The attraction of nickelous sulphate for the reagent is thus the inverse of that of manganous sulphate.

In order to ascertain in what proportions the commixed sulphates are equally precipitable, we have

$$\cdot 98570 + \cdot 61438\,n = 3\cdot 0881 - 1\cdot 7203\,m,$$

whence

$$m = 1\cdot 2221 - \cdot 35714\,n.$$

This equation does not admit of solution except on some further supposition with respect to m or n. Now Table I. shows that the required values of m or n are very nearly equal to 0·9, and consequently that equal weights of nickelous and manganous sulphates are equally precipitable. If, in fact, in the equation just given we put $n = 0\cdot 9$, we shall find the substantially identical value 0·9007 for m.

To calculate in what proportions the two sulphates must be mixed so that they may give equal weights of precipitate, we have

$$\left.\begin{aligned} \frac{n}{m} &= \frac{(\cdot 98570 + \cdot 61438\,n)\nu}{(3\cdot 0881 - 1\cdot 7203\,m)\mu} \\ n &= 1 - m. \end{aligned}\right\}$$

Putting $\frac{\nu}{\mu} = 1$, and combining these equations, we obtain a quadratic, one of whose solutions is $m = \cdot 6215$, and consequently $\nu = \cdot 3785$. Substituting these values in the primitive equations, we find $\mu = \cdot 3086$, $\nu = \cdot 3107$—very nearly equal weights.

Separate Precipitation.—In cases of precipitation of a single salt, if x be the weight of reagent taken, y the weight of precipitate obtained, and α, β constants of condition, some form of the relation

$$y = \frac{\beta x}{1 + \alpha x} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is ordinarily valid. The sign of β is necessarily positive; but the sign of α depends upon the order of constancy in precipitation. The equation can obviously apply only as long as $\phi = \frac{x}{y}$ is $= $ or > 1. There are very few instances in which, as when sodic chloride acts on argentic nitrate in a weak solution, the

* Proc. Roy. Soc. xxix. 181.

value of α vanishes and β represents an undisturbed combining proportion.

In Table II. we have stated the result of acting upon a constant quantity of sodic carbonate with a varied amount of nickelous sulphate. The equation employed in the calculation, all experiments being taken into account, is

$$y = \frac{1{\cdot}2816x}{1+1{\cdot}2308x}. \quad \ldots\ldots \quad (4)$$

Probable error of a single experiment, ·014915. Applying the criterion as to ϕ, we notice that the equation cannot actually hold good until $x = {\cdot}2288$, though the difference between theory and experiment is not great before that stage.

On the other hand, when (Table III.) nickelous salt is kept constant and sodic carbonate is the varied precipitant, we have

$$y = \frac{{\cdot}66641x}{1-{\cdot}52700x}, \quad \ldots\ldots \quad (5)$$

with a probable error ·0046138 for a single experiment. Similarly, when carbonate is constant and manganous salt varies (Table IV.),

$$y = \frac{2{\cdot}7486x}{1+2{\cdot}6648x}, \quad \ldots\ldots \quad (6)$$

an equation which is not rational until $x = {\cdot}6562$, and has therefore been necessarily calculated from the last two experiments only.

Again, when manganous salt is constant and carbonate varies (Table V.),

$$y = \frac{{\cdot}98103x}{1-{\cdot}49146x}; \quad \ldots\ldots \quad (7)$$

the probable error of a single experiment being ·015742.

If in equation (3) we put $\phi = \frac{x}{y}$, we shall find that

$$\phi = \frac{1+\alpha x}{\beta},$$

a relation which, when we adopt other constants, may be expressed as follows:—

$$\phi = a + bx.$$

Hence the precipitability equation is, as we have already seen, a linear one.

The equation to ϕ, as thus deduced from the separate precipitation experiments $[\phi_s]$, differs in its constants from that to which the "conjoint" experiments $[\phi_c]$ have led us. The

two equations are apparently related thus

$$\phi_c = a + bx,$$
$$\phi_s = (a+\rho) + (b-\rho)x,$$

where ρ is some constant to be deduced from the determinations. We find, in the case of manganous sulphate, that these relations do in fact hold accurately; but the results with nickelous sulphate are not in definite accordance with them. We trust on some future occasion to resume the consideration of this subject, which is of primary importance in the theory of precipitation.

The equations of separate precipitability (as deduced from the separate precipitation experiments) are

$$\phi_m = \cdot 36383 + \cdot 96951 m, \quad . \quad . \quad . \quad . \quad (8)$$
$$\phi_n = \cdot 78027 + \cdot 95815 n. \quad . \quad . \quad . \quad . \quad (9)$$

Here there is no reciprocal relation between the second constants, which are indeed identical within the limits of experimental error. We infer from these numbers that manganous and nickelous sulphates, when separate, are equally precipitable.

The following are the chief conclusions which we have drawn from our work.

1. Precipitability is a linear function of mass.

2. When commixed manganous and nickelous sulphates are precipitated by sodic carbonate, equal weights of them are equally precipitable, the attraction of one of them for the reagent being the inverse of that of the other.

3. When the above sulphates are separately precipitated by the same reagent, they are equally precipitable, and do not exhibit the inverse function.

4. There is some evidence that the precipitabilities of the commixed and separate sulphates are mathematically related in a simple manner.

5. Within moderate limits, precipitation is not traceably affected by temperature.

XX. *Researches on Chemical Equivalence.*—II. *Nickelous and Cadmic Sulphates. By* Edmund J. Mills, *D.Sc., F.R.S., and* Bertram Hunt*.

WE propose to consider a case in which a metallic sulphate belonging to some other group is compared with nickelous sulphate (the previously employed standard), sodic carbonate being the precipitant. For this purpose we have selected cadmic sulphate—partly because it admittedly

* Communicated by the Authors.

fulfils the required condition, and partly because it nevertheless has some strong analogies with the nickel group.

I. *Preparation of the Salts.*

Pure cadmic sulphate was prepared from an excellent commercial sample, the only impurity in which was a small quantity of zinc sulphate. From an acidified aqueous solution of the sample, cadmic sulphide was thrown down by hydric sulphide. The washed sulphide was then dissolved in pure hydric sulphate, and rendered neutral by careful ignition.

1-per-cent. solutions of each of these salts were accurately prepared. The sodic carbonate solution was prepared and treated as in Part I.*

II. *Separation of Nickel from Cadmium.*

From a solution of the mixed chlorides, cadmic sulphide was precipitated as sulphide; this was washed, dissolved in strong hydric chloride, evaporated with hydric sulphate, ignited and weighed as sulphate. The filtrate and washings were also evaporated with hydric sulphate; the ignited residue was weighed as nickelous sulphate.

III. *Precipitability Experiments.*

The solution of pure sodic carbonate was of such a strength that 1 cub. cent. of it exactly corresponded (Na_2O_3 : $CdSO_4$) to 1 cub. cent. of the cadmic solution. The usual series of nine experiments was made, under the same general conditions as in Part I.—the constant weight of mixed metallic sulphates being 1 grm., and volume of their solution 100 cub. cent., the constant volume of carbonate solution being 100 cub. cent. The precipitates were in all cases allowed one hour's repose; they were then filtered as rapidly as possible with the aid of an aspirator, and washed with cold water. The results are given in the following table, the precipitate being returned as sulphate:—

Nickelous sulphate taken. *n.*	Cadmic sulphate taken. *d.*	Nickelous sulphate precipitated. *v.*	Cadmic sulphate precipitated. *c.*	Temperature.
·9	·1	·5705	·0810	7·0 C.
·8	·2	·5351	·1823	7·0
·7	·3	·4477	·2763	10·4
·6	·4	·3875	·3546	12·0
·5	·5	·3295	·4807	9·6
·4	·6	·2563	·5937	10·0
·3	·7	·1876	·6990	10·5
·2	·8	·1358	·8010	10·7
·1	·9	·0692	·9037	11·3

* For Part I. see *antè*, p. 169.

IV. *Discussion.*

The precipitability of nickelous sulphate being, as heretofore, represented by $\phi=\frac{n}{\nu}$, we find our experiments in accordance with the equation

$$\phi=1{\cdot}53106. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Probable error of a single experiment, ·034802: of nine experiments, ·011601.

In a similar manner, the precipitability of cadmic sulphate, $\phi=\frac{d}{\delta}$, proves to be

$$\phi=1{\cdot}19462-{\cdot}25763d. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Probable error of a single experiment, ·025793: of nine experiments, ·008598.

In order to ascertain under what conditions the two sulphates are equally precipitable, we must have

$$1{\cdot}19462-{\cdot}25763d=1{\cdot}53106,$$

which leads to $-1{\cdot}3059$ as the value of d. To this negative result no chemical interpretation can be assigned. We consequently infer that, in these experiments, *nickelous and cadmic sulphates are not comparable in point of* precipitability.

To calculate in what proportions the sulphates should be mixed in order to yield equal weights of precipitate, we must solve the quadratic

$$\left.\begin{aligned}\frac{n}{d}&=\frac{1{\cdot}53106\,\nu}{(1{\cdot}19462-{\cdot}25763\,d)\delta},\\ n&=1-\delta.\end{aligned}\right\}$$

[The condition gives $\frac{\nu}{\delta}=1$]. The deduced values are $d={\cdot}4153$ and $n={\cdot}5847$. Inserting these quantities in the primitive equations, we obtain $\nu=\delta={\cdot}3819$, a result evidently in accordance with experiment.

Our present inference with regard to the precipitability of nickelous-cadmic salt will lead (if confirmed by the action of reagents other than sodic carbonate) to the following important criterion:—*Two elements belong to the same group when*, in saline solutions of identical genus, *they may be equally precipitable.*

XXI. *Chemical Symmetry, or the Influence of Atomic Arrangement on the Physical Properties of Compounds.* *By* THOMAS CARNELLEY, *D.Sc., Professor of Chemistry in Firth College, Sheffield.*

[Concluded from p. 130.]

II. INFLUENCE OF ATOMIC ARRANGEMENT ON THE SOLUBILITY.

WE have seen that those compounds whose atoms are symmetrically arranged melt higher than those in which the arrangement is asymmetrical. Now let us see if there are not other physical properties which vary in this respect in the same way as the melting-point.

All bodies are acted on by at least two forces; one of which is the attraction between the molecules, and the other (viz. heat) tends to drive them apart. When the first force is in excess of the second, the body is solid; when the second is in excess, the body is a gas; when the two are about equally balanced, the body is liquid. Therefore the greater the attraction between the molecules the greater the temperature required to melt the body, and consequently the higher the melting-point. Having come to this conclusion, let us see if there are not other physical properties which depend on the attraction between the molecules, and which hence ought to be affected by symmetry of arrangement of the atoms in the molecule in the same way as the melting-point.

The first of such properties which suggests itself is solubility. In order that a solid may dissolve in any liquid, it is necessary that its molecules should undergo a sort of unloosening process; and we should therefore conclude that, of two isomeric compounds, that would dissolve the most easily in which the atttraction between the molecules was the least, and which would therefore, as shown above, have the lowest melting-point. This argument shows, then, *that of two isomeric bodies that dissolves the most easily which has the lower melting-point, and in which therefore the atomic arrangement is less symmetrical.*

Let us now apply this rule in a few cases, and see whether symmetrical compounds are less soluble than their asymmetrical isomers. The following instances were taken quite promiscuously from Watts's 'Dictionary,' and are all that were tried, whether agreeing or not with the rule. The numbers in the third column represent the *order* of solubility; whilst the melting-point is taken as a measure of the symmetry:—

		Melting-point.	Order of solubility.	Remarks.
Diphenyl benzene, $C_6H_4Ph_2$	(1.2 or 1.3)	85	1	In alcohol, ether, CS_2 or C_6H_6.
	(1.4)	205	2	
Phthalic acid. $C_6H_4(COOH)_2$	1.2	175	?	In alcohol or water.
	1.3	*a* 300	1	
	1.4	does not melt.	2	
Dinitrobenzene, $C_6H_4(NO_2)_2$	1.3	89	1	In alcohol, benzene, or chloroform.
	1.2	118	2	
	1.4	171	3 (?)	
Dichlorbenzoic acid, $C_6H_3.Cl_2.COOH$	1.3.4	126	1	The melting-points are for the free acids, the solubilities for the barium salts in water.
	1.2.3 or 1.2.5	156	2	
	1.2.4	201	3	
Chlorbenzoic acid, $C_6H_4.Cl.COOH$ ...	1.2	137	1	In water.
	1.3	152	2	
	1.4	234	3	
Brombenzoic acid, $C_6H_4.Br.COOH$ ...	1.2	147	1	In water.
	1.3	155	2	
	1.4	250	3	
Nitrobenzoic acid, $C_6H_4.NO_2.COOH$ ...	1.2	143*	1	In water.
	1.3	142	2	
	1.4	240	3	
Nitrobrombenzoic acid, $C_6H_3.NO_2.Br.COOH$	1.2.5	140	1	In water.
	1.2.3	250	2	
Amidobenzoic acid, $C_6H_4.NH_2.COOH$	1.2	144	1	In water.
	1.3	173	2	
	1.4	186	3	
*Oxybenzoic acid, $C_6H_4.OH.COOH$ ...	1.2	156	2	In water.
	1.3	200	?	
	1.4	210	1	
Methoxybenzoic acid, $C_6H_4.OCH_3.COOH$	1.2	99	1	In water.
	1.3	106	1	
	1.4	175	3	
*Oxybrombenzoic acid, $C_6H_4.Br.OH.COOH$	1.2.5	164	2	In water.
	1.2.3	219	1	
Pyridine monocarboxylic acids:—				
Picolinic acid		135	1	In water.
Nicotinic acid		228	2	
Pyridine dicarboxylic acids	γ	237	1	In water.
	β	243	2	
	α	245	3	

Table (*continued*).

	Melting-point.	Order of solubility.	Remarks.		
	Amide.		Ba salt.	Pb salt.	
†Toluene sulphonic acids, $C_6H_4.CH_3.SO_3H$ 1.3	108	1	1	1	In water.
1.4	136	2	2	2	
1.2	154	3	3	3	
Isostilbene, $CH_2=C(C_6H_5)_2$ ..	Liquid.	1	In alcohol.		
Stilbene, $C_6H_5.CH=CH.C_6H_5$	120	2			
Ditolyl, $CH_3.C_6H_4.C_6H_4.CH_3$ 1.2; 1.4	Liquid.	1	In alcohol.		
1.4; 1.4	121	2			
Dinaphthyl ..... ($\alpha-\beta-$)	75	1	In alcohol.		
($\alpha-\alpha-$)	154	2			
($\beta-\beta-$)	187	3			
Phenanthrene	100	1	In alcohol.		
Anthracene	213	2			
Dinitrophenyl ... (?)	93	1	In alcohol.		
1.4; 1.4	213	2			

III. Influence of Atomic arrangement on the Heats of Formation and Combustion.

We may compare the union of two or more atoms in the formation of a compound to the falling of a stone to the earth. The further the stone falls, the greater the amount of heat evolved on its being suddenly stopped; and the nearer the stone gets to the centre of the earth, the greater the attraction between the stone and the earth, and therefore the greater the force required to raise the stone—*i. e.* to separate it from the earth. In an analogous manner, the greater the heat produced in the formation of a compound from its elements, the greater its stability, and the greater therefore the force required to separate the atoms.

Again, if a stone falls halfway towards the earth's centre and afterwards falls the remaining half, then the heat given out at the second half of its journey would be less than in the

† The solubilities of the amides, Ba salts, and Pb salts of the three toluene-sulphonic acids obey the rule as regards the melting-point of the amides, the 1.4 amide, however, melts lower than the 1.2. It therefore seems probable that the orientation of the toluene sulphonic acids is incorrect.

second part of the journey of a stone which had first fallen only one quarter of the way towards the earth and afterwards fell the remaining three quarters. For a stone falling from a given height to the centre of the earth can only produce one given quantity of heat; and if it divides its fall into two parts, then the greater the amount of heat produced at the end of the first portion of its fall the less will be the amount produced in the second part of the fall.

And so it is in the formation of chemical compounds. Suppose we have a compound of carbon and hydrogen; this, on complete combustion, gives out a fixed and definite quantity of heat, say $3x$. If now, instead of being completely burnt to carbonic acid and water, the process stops at an intervening point with the formation of two isomeric compounds A and B, and if x units have been evolved in the formation of A and $2x$ units in the formation of B, then the heat evolved by the further complete combustion of A and B will be $2x$ and x respectively; *i. e.* of two isomeric compounds, the one which has the greatest heat of formation will have the least heat of combustion, and *vice versâ*. We have also seen, that the greater the heat evolved in the formation of a compound from its elements the greater is the attraction between its atoms, and therefore the higher the melting-point. So that, of two isomeric compounds, that which has the highest melting-point will also have the greatest heat of formation and least heat of combustion.

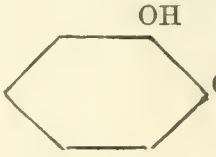
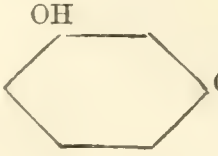
Now though isomeric compounds, as a rule, have *nearly* the same heat of formation, and therefore of combustion, yet they are not exactly the same in this respect. This being so, what is it that causes the difference, small though it may be? The cause can be nothing but the difference in the arrangement of the atoms in the two compounds. And from a comparison of all available data, which in this respect are very meagre, I venture to conclude

(1) *That the stability, and therefore the heat of formation, of symmetrical compounds (and therefore those with highest melting-points) is greater than that of asymmetrical compounds isomeric with them.*

If this be true, it will follow

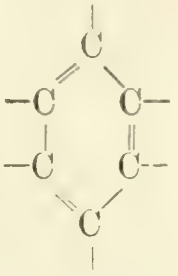
(2) *That the heats of combustion of the former compounds are less than those of the latter.*

The following table contains all the examples to which the above hypothesis can at present be applied:—

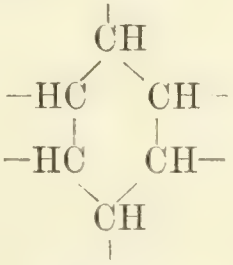
	Formula.	Melting-point.	Heat of formation.	Heat of combustion.
Salicylic acid ..... (1.2)		155	106	759
Metoxybenzoic acid (1.3)		200	111	754
Paraoxybenzoic acid (1.4)		210	115	750
Isopropylenic glycol	$CH_3.CH(OH).CH_2.OH$	?	.....	436
Methylal	$CH_3.O.CH_2.O.CH_3$	?	.....	434
Normal propylenic glycol ..	$OH.CH_2.CH_2.CH_2.OH$	?	.....	431
Allyl alcohol	$CH_2 : CH.CH_2.OH$	?	?	443
Propyl aldehyde	$CH_3.CH_2.COH$	?	69*	426
Acetone	$CH_3.CO.CH_3$	?	65	424
Mannite	$CH_2(OH).(CH.OH)_4.CH_2(OH)$	166	287	760
Dulcite	?	185	294	753
Methyl formate	$H.COOCH_3$	Liquid.	.....	252
Acetic acid	$CH_3.CO.OH$	17	.....	210
Ethyl acetate	$CH_3.CO.OCH_2.CH_3$	Liquid.	.....	554
Butyric acid	$\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!CH—COOH$	0	.....	497
*Methyl acetate	$CH_3.CO.OCH_3$	.....	.....	395
*Ethyl formate	$H.COOCH_2.CH_3$	.....	.....	391
Methyl valerate	$CH_3.CH_2.CH_2.CH_2.COOCH_3$	.....	.....	856
Ethyl butyrate	$CH_3.CH_2.CH_2.COOCH_2.CH_3$	.....	.....	823
Amyl acetate	$CH_3.COOCH_2.CH_2.CH_2.CH_2.CH_3$	.....	.....	1036
Ethyl valerate	$CH_3.CH_2.CH_2.CH_2.COO.CH_2.CH_3$	.....	.....	1019
Normal propyl alcohol ...	$CH_3.CH_2.CH_2.OH$	.....	.....	480
Isopropyl alcohol	$CH_3.CH(OH).CH_3$	.....	.....	478
Isoamyl alcohol	$\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!CH.CH_2.CH_2.OH$	Liquid.	.....	794
Dimethylethyl carbinol ...	$\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!C(OH).CH_2.CH_3$	12	.....	788
Ethylvinyl carbinol	$CH_3.CH_2.CH(OH).CH=CH_2$	.....	.....	753
Valeraldehyde	$\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!CH—CH_2—COH$	.....	.....	742

These relations as regards the heats of formation and combustion of isomeric compounds are thrown out merely as suggestions; for the data are so extremely few in number that I should not be justified in doing more.

I may here mention that Julius Thomsen has shown that the determination of the heat of combustion will in many cases enable us to say whether two or more carbon atoms are united by single, double, or more linkings. Thus, he has shown that ethane is $CH_3—CH_3$, and propane $CH_3.CH_2.CH_3$, *i. e.* that their carbon atoms are united by single links only; whilst in ethylene and propylene two carbon atoms are connected by double linkings, thus, $CH_2=CH_2$, and $CH_2=CH.CH_3$; whereas in acetylene the two carbon atoms are bound by treble linkings, $CH\equiv CH$. Thomsen also concluded, from the heat of combustion, that the six carbon atoms in benzene are not bound by three double and three single linkings, thus,



but by nine single linkings, thus:—



Bruhl, however, as we shall see presently, concludes from the specific refraction of benzene that the old view is the correct one.

IV. Influence of Atomic Arrangement on Optical Rotation.

According to Pasteur (*Researches*, p. 27), molecules are divisible into two classes:—(1) those in which the atoms are symmetrically arranged, (2) those in which the arrangement is asymmetrical. The property of optical activity belongs to the latter class only; and Le Bel has proposed the hypothesis that when a carbon atom is united directly with four different

elements or radicals, an asymmetrical form of molecule is produced, which must therefore be optically active. This view is supported by Van t'Hoff (*Bull. Soc. Chim.* [2] xxiii. p. 295), who finds, from the comparison of the chemical composition of all known active and many inactive bodies:—(1) *That every organic combination which rotates the plane of polarized light contains an atom of asymmetrical carbon.* The converse of this law, however, does not hold good. This may be attributed either to the presence of two isomerides of opposite rotatory power, or to the fact that asymmetrical carbon is not of itself sufficient to produce optical activity, which may also depend on the nature, as well as on the mutual diversity, of the atoms attached to the asymmetrical carbon atom. The following compounds are all optically active, and all contain an asymmetrical carbon atom, which for distinction is printed in black type:—

Lactic acid .....$CH_3.\mathbf{C}H(OH).COOH.$
Malic acid.....$COOH.CH_2.\mathbf{C}H(OH).COOH.$
Malamide .....$(CONH_2).CH_2.\mathbf{C}H(OH).COOH.$
Aspartic acid .....$COOH.CH_2.\mathbf{C}H(NH_2).COOH.$
Asparagine .....$(CONH_2).CH_2.\mathbf{C}H(NH_2).COOH.$
Tartaric acid.....$COOH.\mathbf{C}H(OH).\mathbf{C}H(OH).COOH.$
Tartaramide .....$(CONH_2).\mathbf{C}H(OH).\mathbf{C}H(OH).(CONH_2).$
Secondary butyl carbinol...$CH_3.CH_2.\mathbf{C}H(CH_3).CH_2(OH).$
Methylethylacetic acid ...$CH_3.CH_2.\mathbf{C}H(CH_3).COOH.$
Hydroxyglutanic acid .....$COOH.CH_2.CH_2.\mathbf{C}H(OH).COOH.$
Glutamic acid .....$COOH.CH_2.CH_2.\mathbf{C}H(NH_2).COOH.$
Secondary butyl acetic acid.$CH_3.CH_2.\mathbf{C}H(CH_3).CH_2.COOH.$
Mannitol .....$CH_2(OH).(\mathbf{C}H.OH)_4.CH_2(OH).$
Glucose.....$CH_2(OH).(\mathbf{C}H.OH)_4.COH.$
Saccharic acid .....$COOH.(\mathbf{C}H.OH)_4.COOH.$
Phenylhydroxyacetic acid..$C_6H_5.\mathbf{C}H(OH).COOH.$

(2) *Derivatives of optically active combinations lose their rotatory power when the asymmetry of their carbon atoms disappears. When the contrary happens they do not generally lose it.*

These facts will in the future, no doubt, render great service in determining the constitution of optically active chemical compounds.

V. Influence of Atomic Arrangement on Molecular Refraction.

The molecular refraction of an element $=\frac{A-1}{d}\times M$; where A = the index of refraction, d = specific gravity, and M = molecular weight.

Now Brühl has recently shown, in a most important series of papers, that the molecular refraction of a compound containing polyatomic atoms is dependent on the manner in which these atoms are combined in the compound, whilst that of monatomic elements is quite independent of this.

The atom of a given element has a definite refractive equivalent, which it retains in all those compounds in which there are no double linkings; so that the refraction-equivalent of such a compound is equal to the sum of the refraction-equivalents of its constituent atoms; consequently, if we know the refraction-equivalents of each of the constituent atoms, we can readily calculate that of the compound. Thus, the refraction-equivalents of the following elements are:—

$$H=1{\cdot}29;\quad O=2{\cdot}71;\quad C=4{\cdot}86;$$

and therefore the refraction-equivalent of alcohol, $CH_3.CH_2.OH$, which contains only single linkings, is

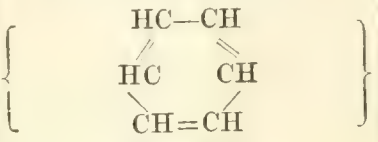
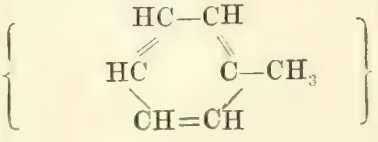
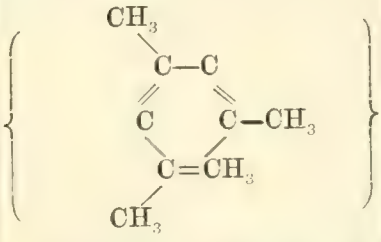
$$(2\times 4{\cdot}86)+(6\times 1{\cdot}29)+2{\cdot}71=20{\cdot}17.$$

But we find that, in the case of those compounds containing double linkings between carbon atoms, the observed refraction-equivalent is always in excess of the calculated quantity, as shown in the table, p. 188.

Here we see that, for every double linking of carbon atoms, the refraction-equivalent is raised by two units.

Brühl has also shown that a similar thing holds good in the case of other polyatomic elements besides carbon. Thus, oxygen, when combined with carbon by a double linking, as in acetic acid, $CH_3.C(=O)-OH$, has a higher refraction-equivalent (3·29) than when combined with only a single link, as in alcohol, $CH_3.CH_2.OH$, viz. 2·71.

Monatomic elements, on the other hand, possess refractive equivalents which are constant in all their compounds. The above will show at once what an important influence the double or single linking of atoms has on the refractive equiva-

	Structure.	Formula.	Molecular refraction-equivalent.		Difference.	No. of double linkings × 2 = difference.
			Found.	Calculated.		
Propyl alcohol .	$CH_3.CH_2.CH_2.OH$	C_3H_8O	28·00	27·61	0·39	No double linkings.
Amylene	$CH_3.CH_2.CH_2.CH=CH_2$	C_5H_{10}	39·29	37·20	2·09	=1×2=2
Diallyl	$CH_2=CH.CH_2.CH_2.CH=CH_2$	C_6H_{10}	45·99	42·06	3·93	=2×2=4
Valerylene.....	$CH_2=CH.CH_2.CH=CH_2$	C_5H_8	38·65	34·62	4·03	=2×2=4
Benzene		C_6H_6	42·16	36·90	5·26	=3×2=6
Toluene.....		C_7H_8	50·06	44·34	5·72	=3×2=6
Mesitylene.....		C_9H_{12}	65·22	59·22	6·00	=3×2=6

lents of compounds, and also what great service the knowledge of refraction-equivalents is capable of rendering in the determination of the constitution of compounds.

VI. Influence of Atomic Arrangement on Fluorescence.

Till within a few years ago, the number of bodies known which possessed fluorescing properties was very small. And it is only with the advance of the manufacture of coal-tar colours, and the investigations to which this advance has given rise, that a large number of fluorescing compounds has become known to us; so that the phenomenon of fluorescence has nowadays ceased to be a rarity. Up to the present, however, but little has been done in tracing any connexion between this property and the chemical constitution of the bodies exhibiting it. There can nevertheless be no doubt that it is dependent on a certain grouping of atoms—just as the tinctorial properties are dependent on special atomic groupings, as pointed out by O. N. Witt, to which I shall shortly refer.

Quite recently, Liebermann (*Ber.* xiii. p. 913) has made

an attempt to refer the fluorescent phenomena of the anthracene series to the chemical constitution of the members of this series. According to him, *those derivatives fluoresce in which the two carbon groups binding together the two benzene rings possess the constitution* $C_6H_4\begin{smallmatrix}CM\\|\\CM\end{smallmatrix}C_6H_4$ (where M = a monatomic element or group); *whilst those derivatives which contain the double ketone group* $C_6H_4\begin{smallmatrix}CO\\|\\CO\end{smallmatrix}C_6H_4$, *or the group corresponding to it in phenylanthracene,* $C_6H_4\begin{smallmatrix}C(OH)(C_6H_5)\\|\\CO\end{smallmatrix}C_6H_4$, *do not fluoresce.* This rule he finds to hold good in the case of all the 60 to 70 compounds to which it can at present be applied; thus:—

Fluorescent.

Anthracene	$C_6H_4\begin{smallmatrix}CH\\|\\CH\end{smallmatrix}C_6H_4$.
Dichloranthracene	$C_6H_3Cl\begin{smallmatrix}CH\\|\\CH\end{smallmatrix}C_6H_3Cl$.
Trichloranthracene	$X_1\begin{smallmatrix}CH\\|\\CH\end{smallmatrix}X^*$.
Dibromanthracene	,,
Tetrachloranthracene	,,
Anthracene sulphonic acid ...	$C_6H_4\begin{smallmatrix}CH\\|\\CH\end{smallmatrix}C_6H_3.SO_3H$.
α-anthracene disulphonic acid	$X_1\begin{smallmatrix}CH\\|\\CH\end{smallmatrix}X^*$.
β-anthracene disulphonic acid	,,
&c.	&c.

Non-fluorescent.

Anthraquinone	$C_6H_4\begin{smallmatrix}CO\\|\\CO\end{smallmatrix}C_6H_4$.
Anthraquinone sulphonic acid ...	$C_6H_4\begin{smallmatrix}CO\\|\\CO\end{smallmatrix}C_6H_3.SO_3H$.
Oxyanthraquinone	$C_6H_4\begin{smallmatrix}CO\\|\\CO\end{smallmatrix}C_6H_3.OH$.
Alizarine.....	$C_6H_3.OH\begin{smallmatrix}CO\\|\\CO\end{smallmatrix}C_6H_3.OH$.

&c.

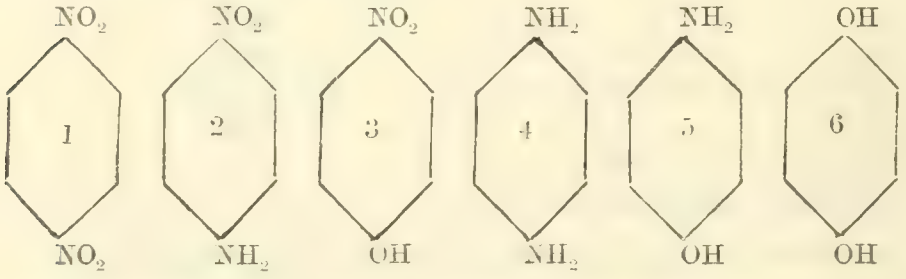
* Constitution of X and X_1 unknown.

The strength of the fluorescence is very different for different members of the fluorescing series. Thus, β-anthracene disulphonic acid is strongly fluorescent, whilst the α-acid is but slightly so.

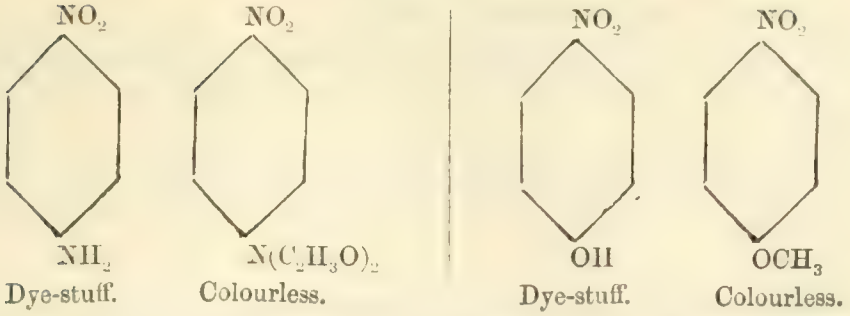
VII. Influence of Atomic Arrangement on the Tinctorial Properties.

The influence of chemical constitution on the tinctorial properties of compounds has been studied more especially by Otto Witt (*Ber.* ix. p. 522), whose more important conclusions may be briefly stated as follows :—

Since benzene is not a dye-stuff, it follows that colouring-properties are conditioned by the side chains. Of the three side chains, NO_2, NH_2, OH, not one of them alone is capable of producing a compound possessing tinctorial properties; for $C_6H_5.NO_2$, $C_6H_5.NH_2$, $C_6H_5.OH$ are all colourless bodies. It is possible, however, that certain combinations of two or more of these groups will produce the necessary result; thus, of the six possible compounds,

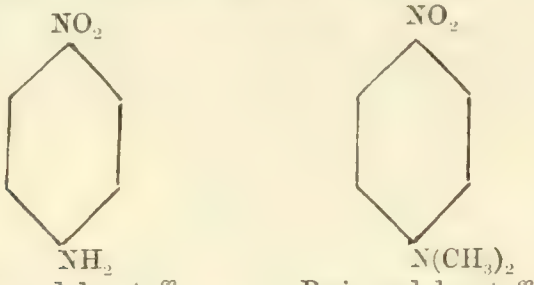


only 2 and 3 are dye-stuffs. We therefore see that the combination of the nitro-group with the amido-, or hydroxyl groups, produces a colouring compound; or, in other words, the presence of a NO_2 and a salt-forming group conditions the dyeing properties. The nitro-derivatives of benzene are to a certain extent coloured; but it is only on the introduction of a salt-forming radical that we get a true dye-stuff. That to obtain a dye-stuff the presence of a NO_2 group and a salt-forming radical is necessary, is shown by the fact that if we destroy the basicity of an amido-group by the introduction of an acetyl group, or if we destroy the acidity of a hydroxyl group by introducing a methyl group, the compound entirely loses its colouring-properties, though by these changes no alteration has been produced in the general constitution of the compound. Thus



Dye-stuff. Colourless. Dye-stuff. Colourless.

If, however, we methylate the NH_2 group of nitro-aniline, we do not destroy its tinctorial properties, because we do not at the same time destroy its basicity. Thus



Basic, and dye-stuff. Basic, and dye-stuff.

From such considerations as these Witt has enunciated the following laws:—

(1) *The tinctorial properties of aromatic compounds are conditioned by the simultaneous presence of a colour-imparting group* (such as a nitro-group) *and of a salt-forming group* (such as NH_2 or OH group).

The colouring group he terms the *chromophor*; and a compound containing a chromophor, and in which a salt-forming radical is still wanting for the formation of a dye-stuff, he calls a *chromogen*: thus NO_2 is the chromophor of $C_6H_4.NO_2.NH_2$ and $C_6H_4.NO_2.OH$, whilst $C_6H_5.NO_2$ is the chromogen of both compounds.

Further, the tinctorial properties increase with the number of NO_2 groups present; thus, the nitrodiphenylamines are all acids and dye-stuffs, and their dyeing-power increases with the number of NO_2 groups.

Again, azobenzene, $C_6H_5.N=N.CH_5$, though of a dark yellow colour, is not a dye-stuff; nor is benzidine,

$$NH_2.C_6H_4.C_6C_4.NH_2,$$

though it is a strong base; but as soon as we combine the properties of the two, by introducing into the latter compound the chromophor $-N=N-$, or a salt-forming group into the former, we get a series of beautiful dye-stuffs.

Another point observed by Witt is

(2) *The chromophor exerts its tinctorial influence much more in the* SALTS *of a dye-stuff than when the latter is in the free state.*

Thus, nitraniline, nitrophenol, and picric acid, when pure, are pale yellow, whilst their salts are dark orange and sometimes red.

Again,

(3) *Of two dye-stuffs, both possessing in other respects a similar constitution, that has the highest tinctorial power whose salts are the most stable.*

Since most dye-stuffs owe their basicity or acidity to the presence of the NH_2 or OH group, each chromogen may give rise to two colouring compounds. All dye-stuffs, therefore, may be arranged in pairs; and it is worthy of notice that corresponding dye-stuffs always have a similar colour. Thus,

Nitrophenol	$C_6H_4.NO_2.OH$	bright yellow.
Nitroaniline	$C_6H_4.NO_2.NH_2$	
Picric acid	$C_6H_2.(NO_2)_3.OH$	dark yellow.
Trinitraniline ...	$C_6H_2.(NO_2)_3.NH_2$	
Amidoazobenzene	$NH_2.C_6H_4.N{=}N.C_6H_5$	yellow.
Oxyazobenzene...	$OH.C_6H_4.N{=}N.C_6H_5$	
Tetramidotetrazotriphenyl		brown.
Tetroxytetrazotriphenyl		
Rosaniline		red.
Rosolic acid.....		
Dioxyanthraquinone or alizarin		red or violet.
Diamidoanthraquinone		

All the compounds referred to above (except the two anthraquinone derivatives) contain a nitrogenous chromophor. The dye-stuffs of anthraquinone, however, show that a carbon group may be a chromophor. Neither anthraquinone nor dioxyanthracene are dye-stuffs; whilst a combination of both, viz. alizarin or dioxyanthraquinone, is a powerful colouring agent. The double (—CO—) group is therefore the chromophor of alizarin and the related dye-stuffs. These compounds also lose their colouring-properties on the introduction of methyl into the hydroxyl groups. If we increase the salt-forming power of alizarin by introducing more hydroxyl groups, or by nitration or amidation, we also increase the tinctorial properties. Further, in alizarin and the related

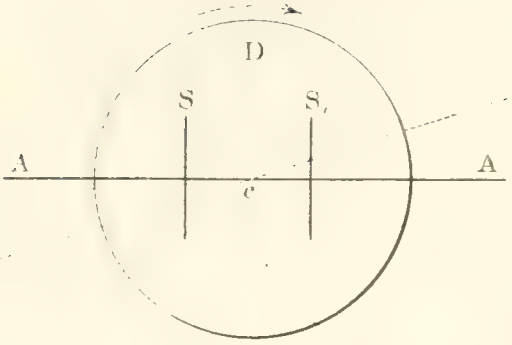


Fig. 1.

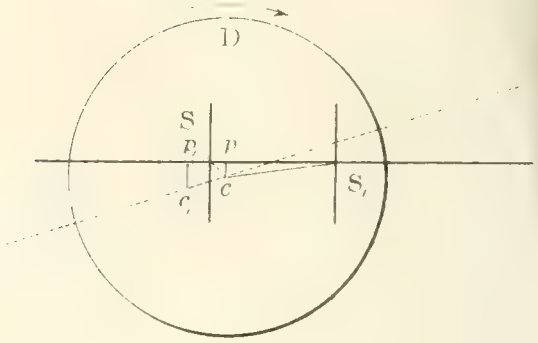


Fig. 2.

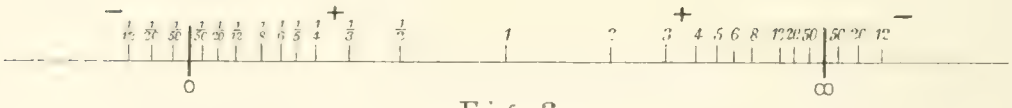


Fig. 3.

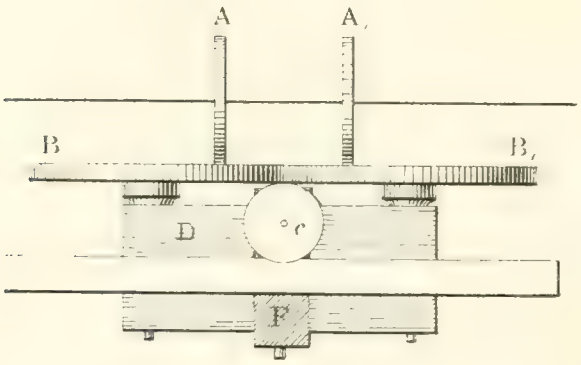


Fig. 4.

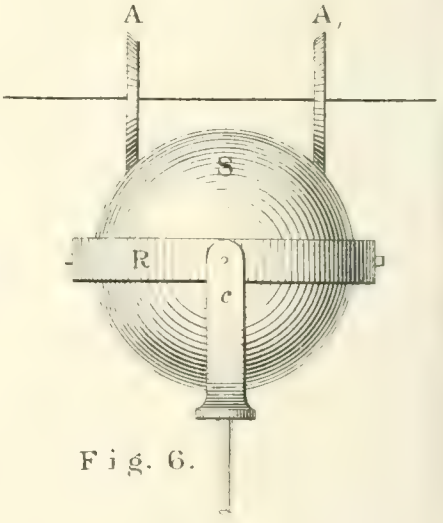


Fig. 6.

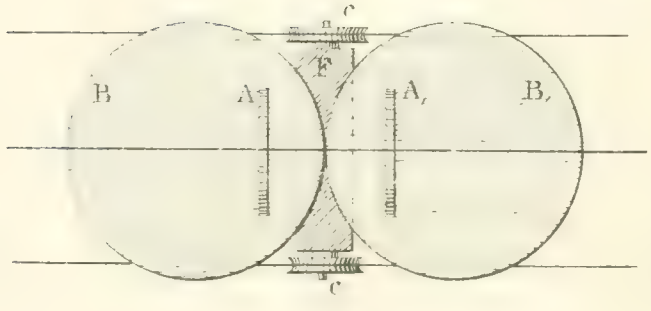


Fig. 5.

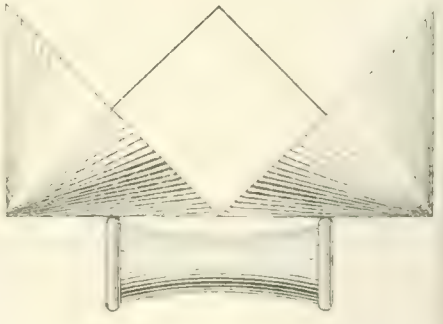
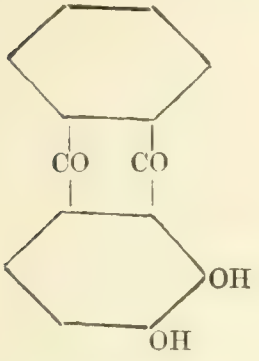


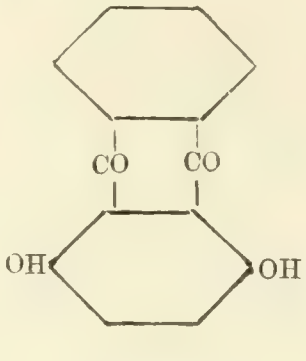
Fig. 7.

dye-stuffs, one of the hydroxyl groups must be in the ortho-position as regards the (—CO—) group in order that the compound may be a dye-stuff. For there are numerous isomers of alizarin, but most of them are colourless, only quinizarin and xanthopurpurin possessing tinctorial properties; and in these, as well as in alizarin, one of the HO groups is in the ortho-position, thus :—



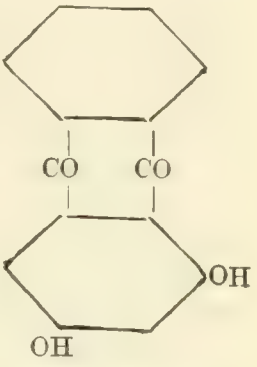
Alizarin.

m.p. = 160.



Quinizarin.

m.p. = 200.



Xanthopurpurin.

m.p. = 184.

In the above sketch I have endeavoured to give a brief account of the more important facts which have come to light during late years in reference to the influence of chemical constitution on the physical properties of organic compounds.

I had intended saying something of this influence of atomic arrangement on the specific volume of compounds and also on the chemical activity, or the susceptibility of certain elements or groups of elements in a compound being replaced by other elements or groups of elements; but my remarks have already attained more than sufficient length, and to do these latter subjects full justice would require the space of another paper.

XXII. *Apparatus for calculating Efficiency.* By C. Vernon Boys, *A.R.S.M., Demonstrator of Physics at the Normal School of Science, South Kensington**.

[Plate V.].

IN a previous paper I have shown how work done in an engine or transmitted by shafting or belting, or expended by an electric current, or how the quantity of electricity which has passed in a conductor during any time, may be automatically measured and integrated or recorded. The present paper refers to apparatus for dividing rates of growth

* Communicated by the Physical Society, having been read at the Meeting on January 28, 1882.

of two integrals so found one by the other, and continuously recording the quotient. Before describing any of these machines, it may be well to give an example showing an application of a divider to some useful purpose. Let there be a steam-engine driving a dynamo-electric machine, which is employed to produce an electric light. Steam does work on the piston of the engine, which may be integrated as already described. This is the work put in. The electricity does work in the electric arc and in the conducting wires, which may be integrated. This is the work taken out. If after any time, say one hour, the work taken out is divided by the work put in, the quotient will represent the average efficiency of the engine and machine combined during the hour. In like manner, if the readings are taken after a minute, the quotient will give the average efficiency during the minute. If instead of a minute an indefinitely short period of time is occupied, then the quotient obtained will give the true efficiency *at* that time. Now, if by mechanism or otherwise a curve can be drawn in which the ordinates represent the true efficiency, while the abscissæ are time, then an inspection of the curve will show exactly how well the machines have done their work at every moment, and the highest points will indicate the time at which the best results have been obtained. What is wanted in practice is not a curve giving the true efficiency as above described, because work is not put into an engine uniformly, but intermittently, but a curve showing the average efficiency for the last few seconds or minutes as the case may be ; and it is this that the mechanism I am going to describe accomplishes. If one of the integrals represents time, and the other work done in an engine, then the curve gives the continuous value of the horse-power per hour ; or if one integral represents turns of a dynamo-machine, while the other represents electric current or electric energy, then the curve gives current-quantity or current-energy per turn. Or, generally, if two things are turning, either or both at a variable rate, a dividing machine will give the ever-varying value of the quotient of one by the other.

I have made use of two principles in the construction of dividing machines, which may therefore be classed under two heads. In the first class a pointer, if at a wrong position on the scale of quotients, moves towards its right place with a speed proportional to its distance from it : its motion is therefore of a logarithmic nature. In machines of the second class a pointer, if at a wrong position on the scale of quotients, changes its speed of moving towards its right position with a speed proportional to its distance from it : its motion therefore is of an harmonic nature. In either case the movement of the

pointer may be made to trace a diagram on a travelling band of paper. I shall describe one logarithmic, and three harmonic dividers.

Logarithmic Divider.

For this a pair of wheels incapable of steering are required—that is, wheels which when turned while their edges are in contact with a surface are compelled to move forward, but which at the same time are perfectly free to move laterally. Disks with smoothly milled edges very imperfectly fulfil these conditions; but an outside smoke-ring, such as is described in the smoke-ring integrator on p. 79, Phil. Mag. Feb. 1882, should answer this purpose well. Let two such smoke-rings be mounted on a common axis, but so that each may revolve independently of the other; let there be a disk so supported that its plane is parallel to the common axis, and so that its own axis would if continued meet the other at a point midway between the two smoke-rings; moreover, let the axis of the disk be carried by an arm in such a manner that it is capable of moving in a direction parallel to the plane of the disk, but inclined at a small angle to the common axis of the smoke-rings. Fig. 1 shows the arrangement in its central position: A A is the common axis, S S_1 are the smoke-rings, and D the disk; the dotted line shows the line of travel of the axis of the disk. If while the disk is in its central position the two rings S S_1 are caused to revolve at equal speeds in opposite directions, then the disk merely turns, and there is no further result. Let the direction of motion be that shown by the arrow.

Now let the ring S_1 begin to revolve faster than S, then the centre of the disk D would, if free, begin to move downwards with a speed equal to half the excess; but it is incapable of moving vertically downwards; yet it may move down the slope indicated by the dotted line. Now, as the rings S S_1 in no way interfere with the lateral movement of the disk, its centre will move down the slope till it reaches such a point that the ratio of the distances of the point from the two rings is equal to the ratio of the speeds. It will be necessary to show that what is true when the produced axis of D intersects A A, is equally true when it has so far moved up or down the slope as to cause a considerable displacement. Let the centre have moved down to *c*, fig. 2, and let S S_1 be the points on the disk touched by the two smoke-rings, and let *c p* be the perpendicular on S S_1 from *c*. Now, if S S_1 are turning with speeds proportional to the lengths S*p* and $S_1 p$, then the centre will have no tendency to move up or down. The motion of S about *c* as a centre may be resolved into two:—one, an upward motion, proportional to S*p* (and this motion

must, from the mechanical construction, be the same in the disk and in the ring); and the other, a lateral motion, proportional to $p\,c$, with which the ring in no way interferes. In a similar way, the motion of S_1 may be resolved into two—a downward motion proportional to $S_1 p$, and a lateral motion proportional to $p\,c$. The downward motion must be common to the disk and ring, while the lateral motion is free. It is clear then, if the rings move with speeds in the ratio $S\,p : S_1 p$, that the centre c will have no up or down tendency; and if the rings are not moving with speeds in the ratio $S\,p : S_1 p$, that the centre c will move up or down the slope with a vertical speed proportional to the distance of the point p from a point which does divide $S\,S_1$ in the ratio of the speeds of the smoke-rings. If θ is the inclination of the dotted line, then c will move along this line with a speed equal to cosec θ times its vertical speed, and p will travel along $S\,S_1$ with a speed equal to cot θ times the vertical speed of c. Should one disk ever stop and change the direction of its motion, then c must move along the slope till it is immediately under the ring, and move beyond till it arrives at such a position c_1 that $S\,p_1 : S_1 p_1$ is the ratio of the speeds.

Let the two smoke-rings be turned by two integrating machines as already described, then either or both may be going at a variable speed. There must at every moment be some point x in the line $S\,S_1$ such that $S\,x : S_1\,x$ is the ratio of the speeds. This point will sometimes coincide with P, at which times C will be stationary; it will generally, however, be distant more or less from P, in which case P will pursue it with a speed proportional to its distance from it. If the arm which carries C carries also a pencil bearing against a uniformly travelling band of paper, then the curved line drawn will show what has been the efficiency, horse-power per hour, or whatever it was set to find during any period of time. The travelling band would have to be ruled across with lines showing time, and longitudinally with lines showing ratios, the scale being of the kind shown in fig. 3.

If the slope or the direction of motion had been in the opposite direction to that shown, then p, instead of approaching its places, would have fled from it with a speed proportional to its distance from it.

I think it possible that the logarithmic divider might be applied to solve some difficult problems; for while in action the inclination of the path of the centre c to the line $S\,S_1$, or the position of either ring on their common axis, may be changed in any way without interfering with the freedom of the motion of c.

Harmonic Dividers.

All harmonic dividers depend on the steering-power of wheels, an action which renders wheels of ordinary construction useless in the case of the logarithmic divider. The steering action, however, is not determined by any direct effect, as in the case of my cart integrating-machine, or in the more familiar case of a common bicycle, but depends on an intermediate action, and is in this respect exactly similar to the steering arrangements in that most beautiful and ingenious machine the "Otto" bicycle. In this the rider produces a difference in the speeds of two wheels, one on each side of him; the angular deviation therefore is the integral of the difference in speed between the two wheels, while the linear deviation from the original straight course is the integral with respect to the distance run of the sine of the angular deviation. The method of adapting this principle is exactly analogous to the case of the disk-cylinder integrator. That machine was developed in this way:—Take a cart integrator running on fixed ground, remove the whole of the cart except its steering-wheel, which fix in position, then give it movable and cylindrical ground to run on; a disk-cylinder integrator will be the result. In this case do away with the whole of Otto's machine except the two wheels, fix them in position, and give them each a movable floor to run on. Fig. 4 is a side view, and fig. 5 is a plan, of a machine of this kind. $A\,A_1$ are the two steering wheels mounted independently on a common axis; $B\,B_1$ are a pair of disks geared together by their edges, and they form the movable floors for the disks $A\,A$ to rest upon. $c\,c_1$ are the supporting wheels of a frame F, and are capable of running on rails parallel to the common axis of $A\,A_1$. The frame F carries a block D swivelled on a vertical axis passing through the centre of both F and D. The axes of $B\,B_1$ are supported by D. If $A\,A_1$ are caused to turn with speeds proportional to their distances from the centres of $B\,B_1$ and in the same direction, $B\,B_1$ will revolve at equal speeds in opposite directions, and neither D nor F will be affected; but if either A or A_1 is made to revolve a little faster than is due to its central distance, D will begin to turn in its swivel-frame. But no sooner does D begin to twist, than the disks $A\,A_1$, which previously were describing circles on $B\,B_1$, tend to move in spiral paths, the faster one receding from, and the slower one approaching, the centres of the disks $B\,B_1$ on which it is moving. But as $A\,A_1$ are incapable of any movement but rotation, while the disks $B\,B_1$ can, from the nature of their support, accommodate themselves to every kind of movement

except one of lateral translation, therefore the combination c, F, D, B, and B_1 will move longitudinally till the central distances of $A A_1$ are proportional to their speeds; but, unfortunately, by this time the obliquity of the block D has become a maximum, and therefore its rate of longitudinal travel is a maximum also. It therefore travels on and introduces an error of position on the opposite side, which is corrected as before; and so the frame F oscillates on either side of its correct position. The motion may be considered more exactly in this way:—The rate at which D twists is proportional to the error of F, while the rate at which F corrects its position is proportional to the amount of twist of D: this expressed mathematically gives the equation $\frac{d^2y}{dx^2} = -y$, the solution of which is $y = \sin x$ or $y = \cos x$. The motion of both D and F therefore is harmonic. An exactly analogous case is that of a heavy body moving under the influence of a force which varies with the displacement, the movements of F being equivalent to that of the heavy body, while the rate at which D twists represents the force. Of course in the dividing-machine described we have nothing to do with inertia or with a resisting force; but the movements are the result of an action of pure rolling, and it so happens that they follow exactly the same law as the movements of a vibrating heavy body. This view of the subject removes all difficulty in discovering what will be the action during either slow or very rapid variations in the ratio of the velocities. Let the dynamical equivalent be a heavy magnetic needle balanced in a magnetic field. Then the direction of the field corresponds to ratio of velocities, and the motions of the needle to the motion of the frame F. As has been shown in the case of a constant ratio, an error of position of F at starting causes F to oscillate on either side of its correct place, just as a displacement of a magnetic needle causes it to oscillate. Now suppose F to be in its correct position and then the ratio to change slowly (that is, slowly compared with the time of an oscillation); then the frame F will move slowly also, always being in the correct position, because during a slow change in the direction of the magnetic field a balanced needle follows the change without oscillation. Next suppose the ratio to alternate rapidly—that is, rapidly compared with the time of an oscillation, as, for instance, is the case when a steam-engine is made to do work uniformly, for work is put in intermittently and taken out gradually; then the dividing-machine will steadily show the mean value, and will take no account of the rapid variations

in ratio, because a heavy magnetic needle subjected to rapidly alternating currents, which produce rapid variations in the direction of the magnetic field, is unaffected by these currents, but steadily maintains its position in the mean direction of the field. The most complex possible case is a combination of rapidly alternating and slowly changing ratios with occasional sudden changes. The first two are properly considered by the machine, while the sudden changes merely cause the frame F to oscillate on either side of its correct position. As in the case of the logarithmic divider, so here; a curve may be drawn on a travelling band of paper by a pencil attached to the frame.

A more simple arrangement is shown in fig. 6, where the disks $B\,B_1$ and the frame and block are replaced by a sphere S with its axis horizontal mounted in a horizontal ring R, and where this ring is supported in a crutch c which is capable of moving round on a vertical axis. Thus, when R is horizontal, the sphere can turn independently round three axes at right angles to one another. Then, if the disks $A\,A_1$ touch the sphere in points 90° apart, the ratio of their speeds will be measured by the tangent of the inclination of R, while the deviation of c will correspond to the obliquity of the block D in the last machine.

The most simple harmonic divider that I can imagine is made by mounting, as shown in fig. 7, two iron cones with their bases adjacent, and with the lowest generating line in each horizontal and in one straight line. Then, if a magnetized steel reel is hung on and the two cones are turned, the reel will travel about and find the ever-varying value of the ratio of their speeds. The method of attaching a recording-pencil is too obvious to need description. If the two cones were placed so that their bases were turned away from one another instead of being adjacent, then the reel would change its speed of moving *away* from its correct position with a speed proportional to its distance from it. This kind of action could only exist, of course, for a very short time.

Whether dividing-machines are likely to be of general value for practical or experimental purposes, it is difficult to say; but there can be little doubt that cases might arise in which some machine such as I have described might be used with advantage.

XXIII. *On the Electrical Resistance of Gases.* *By* E. Edlund, *Professor of Physics at the Swedish Royal Academy of Sciences**.

§ 1.

THE resistance opposed by gases to the propagation of electricity has shown itself to be different in several respects from that produced by solid or liquid conductors. I shall in this memoir take these differences into consideration, and endeavour to show how they are to be explained.

1st. In order that the current may traverse a solid or liquid conductor, there is no need of a certain amount of electromotive force. However slight the electromotive force may be, the current will nevertheless pass, although of course it becomes less intense as the force is diminished or the resistance increased. Only when the force is equal to *nil* will the current become equal to *nil*. With gases, on the contrary, the circumstances change. For the current to be able to traverse a gas the source of electricity must have a certain amount of electromotive force, or be capable of producing a certain electrical tension, the amount of which depends moreover on the chemical nature, the density, and the temperature of the gas, but never falls below a certain limit in given circumstances. For any electromotive force below that limit the gas is a perfect insulator.

2ndly. The quantity of heat produced by an electric current in its passage through a solid or liquid conductor is, as is known, proportional to the square of the intensity of the current. In gases, on the contrary, the amount of heat is proportional to the first power of the intensity of the current, and not to its square. This remarkable property of gases was first observed by G. Wiedemann†; and later it was completely demonstrated by Naccari and Bellati that the quantity of heat evolved is really proportional to the quantity of electricity which has passed ‡.

3rdly. For solid and liquid conductors the quantity of heat developed, under conditions in all other respects equal, by a given current, is inversely proportional to the cross section of the conductor; while the quantity of heat developed in a column of gas is independent of the section of the latter. This proposition has in like manner been demonstrated experimentally by G. Wiedemann, as well as by Naccari and Bellati.

4thly. In a solid or liquid conductor the resistance is in-

* Translated from a copy, communicated by the Author, of the memoir forming No. 7, vol. vi. of the *Bihang till K. Svenska Vet.-Akad. Handlingar*.

† Pogg. *Ann.* cxlv. p. 237.

‡ *Beiblätter zu den Ann. der Ph. und Ch.* ii. p. 720 (1878).

versely proportional to the section of the conductor. G. Wiedemann has proved, by experiments *, that the tension which is necessary upon the electrodes in order to force the electricity of a Holtz machine to traverse a cylindrical tube filled with rarefied gas is independent of the radius of the tube—which means, in other terms, that the electrical resistance of the gas is independent of the section of the tube. In two experiments, one with a tube 16 millim. and the other with a tube 0·5 millim. in diameter, Schulz† found only an insignificant difference in the electrical tension requisite to compel the electricity of a Holtz machine to pass through the tube.

5thly. With solid and liquid conductors, the difference between the electroscopic tensions on two points situated at a certain distance the one from the other is proportional to the product of the resistance between those points multiplied by the intensity of the current. Warren De la Rue and Hugo Müller‡, have, on the contrary, proved experimentally that the difference between the electroscopic tensions at two points situated at a distance from one another in a column of gas is totally independent of the intensity of the current. These physicists caused the current of a battery composed of a great number of elements to vary between very wide limits; and yet it was impossible to perceive any variation in the above-mentioned difference. With the aid of a galvanic battery of great electromotive force, Hittorf has also proved, although in another way, that the difference between the electric tensions of the electrodes conducting to the column of gas is independent of the intensity of the current §. When for the column of gas between the electrodes a liquid conductor was substituted, the difference became, as might have been expected, proportional to the intensity of the current. From this Hittorf draws the conclusion, somewhat prematurely, that the resistance of the column of gas must be in the inverse proportion of the intensity of the current, to which circumstance he attributes a fundamental importance for the conductivity of gases.

6thly. Several years since, Edm. Becquerel proved‖, by experiments, that gases begin to be conductors when heated to the temperature of redness, after which their conductivity increases in proportion as the temperature rises above that point. If the temperature is sufficiently elevated, they even let the feeble current of a single element pass. The conducti-

* Pogg. *Ann.* clviii. p. 53 (1876). † Ibid. cxxxv. p. 254 (1868).
‡ *Comptes Rendus*, lxxxvi. p. 1072 (1878).
§ Wied. *Ann.* vii. p. 573 (1879).
‖ *Ann. de Ch. et de Ph.* [3] xxxix. p. 377 (1853).

vity moreover increases as the density of the gas diminishes. The fact that gases heated to redness become conductors was at first questioned, although erroneously, by Beetz* and G. Wiedemann†, but has since been confirmed in the most complete manner by Hittorf‡.

But Becquerel's experiment has furnished another result, to which I will call special attention. When, keeping the same battery, the intensity of the current was modified by introducing a rheostat of greater or less resistance, the resistance of the gas was shown to be dependent on the intensity of the current. For example, i and i_1 designating the current-intensities, let E be the electromotive force, r and r_1 the resistances of the solid or liquid conductor, z and z_1 the gas resistances, corresponding to i and i_1, we get, according to Ohm's formula, $i=\frac{E}{r+z}$ and $i_1=\frac{E}{r_1+z_1}$. When the current does not pass through the gas, and M and M_1 designate the resistances necessary in order that the current-intensities may become the same as before, we shall have $i=\frac{E}{M}$ and $i_1=\frac{E}{M_1}$, whence we get $M-r=z$ and $M_1-r_1=z_1$. Putting the calculation thus, it will be seen that the resistance of the gas was nearly proportional inversely to the intensity of the current which had passed.

The observation numbers obtained also appear to indicate that the resistance of the gas increases with the number of the elements of the battery employed, although the intensity of the current be maintained constant by introducing a suitable rheostatic resistance. We will not now dwell upon this almost incomprehensible result (as Becquerel himself remarks), seeing that W. De La Rue and H. W. Müller§ have quite recently found, in their investigations, which, however, were executed by other processes than those of Becquerel, that the resistance of gases at one and the same current-intensity is independent of the number of the elements.

The above-mentioned differences between solid and liquid conductors, on the one hand, and gases on the other, are explained without difficulty if the unitarian theory of the nature of electricity explained by me‖ be taken as the starting-point. In order, however, to furnish the proof of what I advance, it is necessary first to cite some propositions of that theory.

* *Fortschritte der Physik*, ix. p. 479 (1853).

† *Die Lehre vom Galvanismus*, 2nd ed. vol. i. p. 339.

‡ Pogg. *Ann.* Jubelband, p. 234 (1874).

§ Phil. Trans. clxix. p. 236 (1878).

‖ *Théorie des Phénomènes Electriques*, Mem. Roy. Ac. Sciences, Sweden, vol. xii. no. 8 (1874).

§ 2.

(*a*) According to the unitarian theory, the galvanic current in a closed circuit consists in the free æther present in the circuit being set in translatory motion. The intensity of the current is determined by the quantity of æther passing, in the unit of time, through any section of the conductor; and the velocity of the æther, for equal intensity of current, is inversely proportional to the magnitude of the section. The galvanic current may therefore be compared to the current of an ordinary gas in a system of tubes; and the properties belonging to a current of this latter kind are therefore to be met with, *mutatis mutandis*, in the æther-current. Thus, let us imagine a tube, of which one half has the section 1, and the section of the other half is n times as great; let us suppose, secondly, that this tube is filled with a fluid (liquid or gaseous) having a translatory motion impressed by forces acting at one of the extremities of the tube. Now, if we wish to stop or diminish at any point the motion of the fluid by a counterpressure (for instance, by means of a piston or otherwise), in order to attain *the same effect* we must cause to act in the widest part of the tube n times as much pressure as in the narrowest. The diminution of the velocity of the motion by means of the counterpressure does not depend on the absolute value of the latter, but on its value in comparison with the unit of section. If the counterpressure upon the unit of section is as strong in the widest part of the tube as in the narrowest, the diminution of the intensity of the current is equal in the two cases. It will always be so, whatever the resistance, provided that the fluid employed possess sufficient fluidity to produce equal pressure in all directions.

What has just been said finds its direct application in the galvanic current. Whatever opinion may be held upon the nature of electricity, all agree that it is a fluid which the extreme mobility of its particles permits to communicate pressure in all directions. Galvanic resistance impedes the motion of electricity; it therefore acts as a pressure in the opposite direction distributed uniformly over all points of the section of the conductor. Now, if two resistances (two wires, for instance, each of a different metal, and with different sections) produce an equal diminution in the intensity of a given current, their resistance is said to be equal. We know, likewise, in conformity with the foregoing, that the counterpressure opposed by each of them, on the unit of section, to the propagation of the current is in like manner equal. It is therefore exclusively the counterpressure on the unit of section that can serve for the determination of the galvanic resistance. This

is a consequence of the laws of hydrodynamics; and it is impossible to give any other interpretation to the fact if we will continue to admit that the electric material is a fluid.

Now let us suppose a single conducting wire or a liquid column f, of which the section is 1, and also a number n of other conductors, f_0, f_1, f_2, &c., of the same material, section, and length as the preceding, placed side by side. Let us then suppose that a current i passes through the conductor f and afterwards simultaneously through the conductors f_0, f_1, f_2, &c. placed side by side. Each of these latter will therefore have to be traversed by a current $= \frac{1}{n} i$. Now we know by experiment that the resistance to be overcome by the current in order to pass simultaneously through the n conductors f_0, f_1, f_2, &c. constitutes $\frac{1}{n}$ of the resistance to be overcome when the current passes through f. According to the above account, the counterpressure upon the unit of section of the n conductors will be likewise $\frac{1}{n}$ of the counterpressure in the single conductor f, the resistance being determined exclusively by the amount of the counterpressure upon the unit of section. It hence follows, therefore, that in each of the conductors f_0, f_1, f_2, &c. the resistance will in the present case be $\frac{1}{n}$ of what it is in f. We therefore find ourselves confronted by the unexpected result that *galvanic resistance is proportional to the intensity of the current.* It must, however, be observed that this demonstration refers only to solid and liquid, and not at all to gaseous bodies, to which the above experiment is not applicable.

The above-mentioned result is in direct opposition to the opinion hitherto generally admitted, namely that the resistance is independent of the intensity of the current. But if that opinion is still to be maintained, it will be necessary, in consequence of the foregoing, to assume that the fluid which we call electricity is governed by quite different laws of motion from those to which the other fluids known to us are subject. It has moreover been demonstrated in the above-mentioned memoir, that although the formulated proposition may contradict the common opinion, it is by no means contrary to the experimental results upon which that opinion has been believed to be founded.

In consequence of the results of experiment, as also from the theoretical consideration above given, we have, then, as

the expression of the resistance r_0 in a conductor of length 1 and section a, traversed by the current i,

$$r_0 = \frac{ki}{a} = ri,$$

where k is a constant dependent on the chemical and physical nature of the conductor, as well as on the temperature. The constant k is evidently the resistance in a conductor of section 1 and length 1, passed through by the current 1; $\frac{i}{a}$ is the intensity of the current on the unit of section; r, or, in other terms, what we have hitherto named the galvanic resistance, is nothing else but the resistance per unit of intensity of the current. In order to distinguish this resistance from the others, in the following we call it the principal resistance.

(*b*) Electromotive force, like all other motive forces, is measured by the acceleration which it can impart, in the unit of time, to the unit of mass. Taking this for granted, which we are warranted in doing under all the circumstances, Ohm's law can be without difficulty deduced from ordinary mechanical principles. We will nevertheless previously show that the electromotive force is independent of the intensity of the current.

Electromotive force acts with equal intensity upon every point of the electromotive surface of contact; consequently the total value of this force increases proportionally to the extent of that surface. Besides, it is evident that the force not only acts upon the æther molecules which are at the contact surface itself, but also extends to those situated at a very little distance from that surface. Now let us designate by E the quantity of motion which the electromotive force can communicate to the mass of æther on each unit of surface in the unit of time. Let us, in the first place, imagine a current sufficiently powerful for the unit of mass to pass, in the unit of time, through each unit of the surface of contact; then each unit of mass will have received the acceleration E. If the extent of the surface of contact be called n, nE will in this case constitute the total value of the electromotive force. Let us suppose, secondly, the surface of contact passed through, in unit of time, by a mass of æther p times as great as before, and which can then be expressed by pn. The æther possessing the same density in a feeble as in an intense current, the velocity will in this case be p times as great. Each particle of the mass of æther therefore undergoes the action of the electromotive force during a space of time constituting $\frac{1}{p}$ of the time

of action in the former case. The acceleration acquired amounts to only $\frac{E}{p}$. On multiplying by the mass pn, we shall have the total electromotive force equal to nE. Thus the electromotive force can be expressed by nE, whether the current be strong or weak.

If r signifies the totality of the principal resistance, and if i designates the intensity of the current, the total resistance will be ri—which in this case signifies nothing else but the counterpressure, upon the unit of section, opposed by the resistance to the propagation of the current. We shall therefore have nri for the total value of the counterpressure upon a surface of contact of n units magnitude. Designating by L the total length of the circuit, we thus obtain the equation of motion,

$$L\frac{di}{dt}=nE-nri^{*},$$

whence

$$i=\frac{E}{r}\left(1-e^{\frac{nrt}{L}}\right).$$

As soon as the current has become constant we have

$$i=\frac{E}{r}.$$

It follows from this that the electromotive force represented in Ohm's formula is independent of the extent of surface of the electromotor—which, we know, is conformable to experience.

(c) Let us imagine a closed galvanic conductor, of which the length is l and the section everywhere equal to a, composed of the same material throughout its length, and passed through by a constant current of intensity i. If δ is the mass of æther in motion per unit of volume, and h the velocity of the motion, we shall shall have $i=a\delta h$. To calculate the mechanical work done by the current during the unit of time, we shall first consider a current-element comprised between two

* The total length L of the circuit being equal to the sum of all its parts l_1, l_2, l_3, l_4, &c., and these having respectively the sections a_1, a_2, a_3, a_4, &c., the total volume of the conductor will be $a_1l_1+a_2l_2+a_3l_3+$&c. By multiplying this sum by the mass of æther δ in the unit of volume, we obtain the entire mass of the æther in motion. Now, if the augmentation of the velocity during the time dt is respectively dh_1, dh_2, dh_3, the total mass of æther will receive, during the time in question, an augmentation of the quantity of motion which will be expressed by $(a_1l_1dh_1+a_2l_2dh_2+a_3l_3dh_3+\ldots)\delta$. Now $\delta a_1\,\delta h_1=\delta a_2\,\delta h_2=\delta a_3\,\delta h_3=\delta i$, whence, consequently, the total augmentation of the quantity of motion of the æther will be Ldi.

planes situated at distance 1 from each other. The resistance upon the unit of section being r_0, and the magnitude of the section a, the resistance upon the entire section will be $r_0 a = ki$. In the time-unit this element is repelled the path-length h, whence the work done will be kih. Now $h = \frac{i}{\delta a}$, in which expression δ is a constant, as we have seen above. The mechanical work of this element will therefore be $\frac{ki^2}{\delta a}$. If this quantity be multiplied by l, the product $\frac{kli^2}{\delta a}$ will be equal to the work of the entire current. If, lastly, this expression be multiplied by the thermal equivalent of the unit of work, and if the constant δ be made to enter into k, the quantity of heat produced by the current during the time-unit will be equal to $\frac{\mathrm{A}kli^2}{a}$, which is known to be in accordance with experiment.

The calculation can be effected with equal facility, on the same principles, in the case in which the section and the nature of the conductor vary from one part to another.

(*d*) With respect to the production and distribution of free æther at the surface of a galvanic conductor, these can best be explained in the following manner:—

Let us imagine a tube in which a mass of gas is set in motion by a force acting at one extremity, while the gas can issue freely at the other. Let us assume, further, the resistance of the tube to the motion of the gas to be, as is in reality the case, proportional to the length of the tube. If x be the distance between a certain section-plane and the open extremity of the tube, the resistance undergone in that plane by the motion can be put proportional to x. We neglect altogether the influence which may be exerted upon the resistance by the difference of density and velocity of the gas. Let D′ denote the density of the gas at the above-mentioned plane, and D its density at the open extremity of the tube. Every one knows that, from the moment the motion in the tube has become constant, the excess $\mathrm{D}' - \mathrm{D}$ is proportional to x. The density of the gas therefore goes on increasing from the open end of the tube to that at which the force acts. Suppose now the two extremities of the tube joined so as completely to enclose the moving mass of gas. The gas will evidently undergo in one portion of the tube an expansion equal to its increase of density in the other; and at the transition-point between the two portions it will have the same density as if it were at rest. If the tube is everywhere equal, this transition-plane (neutral plane) will divide the tube into two equal parts. At equal

distances from this plane the condensation on one side will be equal to the expansion on the other. If the resistance is greater in one part of the tube than in the other, the neutral plane will be displaced towards the part presenting the greater resistance: and the amount of the displacement will be that which is requisite in order that the resistance of the whole of this part (from the above-mentioned plane to the place where the force acts) may become equal to the resistance of the other part. If D is the density of the gas at the neutral plane, D′ the density in a plane situated on the side where the gas is condensed, D′ − D will be equal to am', in which a is a constant, and m' is the resistance from the neutral plane to the plane in question. If D″ represents the density of the gas in a plane situated on the other side of the neutral plane, D − D″ will in like manner be equal to am'', if m'' is the resistance between these two planes.

These well-known propositions have a direct application to the circulating æther. It possesses the properties of ordinary gases, in that the mobility of its molecules is considerable, so as to permit it to exert equal pressure in all directions. The fact that an electrified body is endowed with the same optical properties as in its normal state shows, as I have remarked in the above-mentioned memoir, that the elasticity of free æther is proportional to its density. Therefore what has been said in this investigation with regard to ordinary gases may also be applied to the æther. The only difference will be that the æther, whether compressed or expanded, will place itself at the surface of the galvanic conductor, since the æther molecules repel each other.

Let us suppose a galvanic circuit in which an electromotive force E excites the motion of the æther in one direction. The æther will therefore become denser on the side of the electromotive force towards which the current goes, and be rarified on the other side.

The neutral plane will have a position such that the galvanic resistance from this plane to the seat of the electromotive force will be equal in amount on both sides. Now let i denote the intensity of the current, D the density of the æther at the neutral plane, or, what amounts to the same, the density of the æther when it is at rest; let D′, D″ be its density at any two planes on the condensation side, and D'_0, D''_0 its density on the expansion side; and, lastly, let m', m'', m'_0, m''_0 be the respective principal resistances from the neutral plane to the planes above mentioned. As the resistance is proportional to the intensity of the current, we shall evidently have

$$D'-D=m'i,\quad D''-D=m''i,\quad -(D'_0-D)=m'_0 i,$$
$$-(D''_0-D)=m''_0 i.$$

Now $D'-D$, $D''-D$, D'_0-D, and D''_0-D are nothing else but the differences between the electroscopic tensions in the respective planes, the first two being positive, and the last two negative.

We obtain, then, the conclusion that *the difference between the electroscopic tensions of two planes is proportional to the intensity of the current, multiplied by the principal resistance between those planes.*

These deductions of Ohm's law, of the development of heat, and of the distribution of electroscopic tension at the surface of the conductor, are applicable only to the case in which there are solid and liquid bodies in the circuit, since only for those bodies is it proved that the total resistance is proportional to the intensity of the current.

§ 3.

The fact that electromotive force, or electric tension, need not exceed a certain limit in order that the current may be able to pass through a solid or liquid conductor, depends, then, according to the unitarian theory, on this—that the effective resistance opposed by a conductor of that sort is proportional to the intensity of the current. If the electromotive force is little, the intensity of the current will also be little, and consequently the resistance will become so feeble that the electromotive force will be able to overcome it. Experiment teaches us that it is quite otherwise with gases. To compel the current to pass through a column of gas a determined electric tension upon the electrodes between which that column is situated is necessary; if the tension is below the above-mentioned limit, the current will not pass. It is true that the insulating property exhibited by the gas must not be attributed to its effective resistance alone; in fact experiments have led to the admission of the rise, at the electrodes, of electromotive forces obstructing the propagation of the electricity by the gas. If the gas is nevertheless sufficiently dense, experiment has shown that the tension necessary for bringing about a discharge is proportional to the distance between the electrodes. The electromotive forces which have their seat upon the electrodes having no connexion with the distance which separates the latter, it follows that, when the gas has sufficient density, it is chiefly its resistance that constitutes the true cause of the discharge not taking place if the electric tension remains below a certain limit*. We arrive, therefore,

* The resistance of the gas diminishes when it is highly rarefied, while the electromotive forces which originate upon the electrodes are seen to increase; but it is not necessary to take that circumstance into consideration here.

at the result that the resistance opposed by a gas to the propagation of electricity cannot be proportional to the intensity of the current, as is the case with solid and liquid conductors. It is quite as impossible to admit, with Hittorf, that the resistance of gases is in inverse proportion to the intensity of the current; for in that case the resistance in a column of gas through which an infinitesimal current passes would be infinite. Now, at the discharge of a condenser, or the closing of a galvanic pile, the current is at first excessively slight. If Hittorf's hypothesis were true, the resistance of the gas would at first be excessively great, and the current could not begin to circulate. Consequently the resistance of a gas cannot in any case be inversely proportional to the intensity of the current. It is in the nature of things that the effective resistance of a column of gas should be proportional to the length of the latter. Therefore, if l denotes the length of the column, and r the resistance in the unit of length, the resistance will be proportional to rl; and from what has just been said, r is neither directly nor inversely proportional to the intensity of the current or to the tension of the electrodes. Quite the contrary, experiments show that the tension necessary for the discharge is proportional to l, whence it follows that r is independent of the tension.

If we admit, in accordance with what has been said above, that the resistance of gases is independent of the intensity of the current, all the differences above stated, between gases, on the one hand, and both solid and liquid bodies, on the other, can be accounted for by the unitarian theory.

If the resistance or counterpressure opposed to the propagation of the current by a column of the gas of the length 1 and section 1 be called k, the total counterpressure in a similar column having the section a will be equal to ka, not to ki as is the case with both solid and liquid conductors. On multiplying this expression by the velocity h of the electricity, the product will be proportional to the mechanical work which is accomplished in the column during the unit of time. Now $i=\delta ah$, an expression in which, as has been said above, δ is a constant. We obtain, then, for the work done in a column of length 1 and section a the expression $\frac{ki}{\delta}$, to which the quantity of heat developed in the same column must be proportional. The quantity of heat developed in a column of gas will therefore be proportional to the intensity of the current, but independent of the section of the column.

The resistance being determined by the counterpressure which the conductor opposes, on the unit of section, to the

propagation of the electricity, and this counterpressure being, in gases, independent of the intensity of the current, it is self-evident that the resistance of a column of gas must be independent of its section.

The difference of electroscopic tension between two points in the conductor situated at a certain distance from each other is, according to the foregoing, proportional to the resistance between the same points. In solid and liquid conductors, therefore, the difference of tension will be proportional to ri, if r denotes the principal resistance between the points, and i the intensity of the current. For gases, on the contrary, the same difference will be proportional to kl, in which k has the signification which has just been mentioned, and l denotes the distance between those points. The difference of electroscopic tension between two points in a column of gas must, then, as has already been proved by experiment, be independent of the intensity of the current; and it can be predicted that it will also be independent of the section of the column, which, however, has not yet been confirmed by experiment.

If r designates the principal resistance in the part of a closed circuit composed of solid and liquid conductors, R the resistance of a column of gas introduced into the circuit, E the electromotive force, i the intensity of the current, and L the total length of the conductor, the differential equation of motion of the æther will be

$$L\frac{di}{dt} = nE - nR - nri.$$

From this equation we get, for the case in which the current has had time to become constant,

$$i = \frac{E-R}{r}.$$

The resistance R of the gas has therefore in reality its place in the numerator, although, according to the ordinary formula of Ohm, it ought to be in the denominator. Of course E must be greater than R for a current to be possible.

If, when gas is introduced into the circuit, i and i_1 denote two different current-intensities answering to the resistances r and r_1 of the solid and liquid conductors introduced into the circuit, and if M and M_1 are the resistances necessary in order to produce the same current-intensities when the gas is excluded from the circuit, we get

$$i = \frac{E-R}{r} = \frac{E}{M}$$

and

$$i_1 = \frac{E - R}{r_1} = \frac{E}{M_1},$$

whence

$$\frac{i_1}{i} = \frac{r}{r_1} = \frac{M}{M_1} = \frac{M - r}{M_1 - r_1}.$$

If, as was done in the calculation of Becquerel's experiments, $M - r$ and $M_1 - r_1$ were made to signify the resistance of the gas at two different current-intensities, the resistance of the gas would be found inversely proportional to the intensity of the current, although in reality it is independent of that intensity.

XXIV. *On the Violet Phosphorescence in Calcium Sulphide.* *By* Captain W. de W. Abney, *F.R.S.**

IN some investigations in photography it became necessary that I should study the phenomenon of phosphorescence exhibited in calcium sulphide, such as is employed in Balmain's paint. And as one or two points of interest arose which have not (as far as I am aware) been described before, I have thought it might be of interest to lay the subject before the Society.

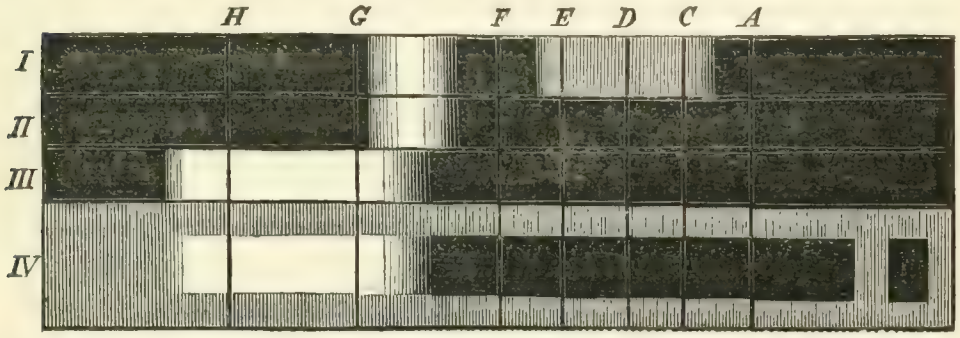
The phosphorescent light, which is a peculiar violet, can be generated, if I may use the term, by day-light or candle-light —by the former fairly brightly, and by the latter only feebly, for reasons which will appear. In order to gain strong phosphorescence, the light from a magnesium-ribbon or the electric light should be employed. Mr. Warnerke has shown that $\frac{1}{8}$ inch of ribbon of the former is sufficient to excite phosphorescence to the maximum point that can be obtained from light of that brilliancy. Light of greater brightness, however, seems to excite it even more strongly. In a communication made to the Society by Lieut. Darwin, R.E., he gave the formula for the decrease of phosphorescence after excitation, from which it will be seen that it very rapidly diminishes in brilliancy.

My first experiment was to determine the spectrum of the emitted light; and this I observed with a small spectroscope; and the emission-spectrum is shown in I, figure opposite. It will be seen that to the eye the greatest luminosity is between G and F, and a feebler one extending from between E and F

* Communicated by the Physical Society, having been read at the Meeting on January 28, 1882.

as far as the eye can recognize colour of low intensity towards the red. It became a matter of curiosity to know if any rays lay beyond the violet; and for this purpose an Iceland-spar prism and quartz lenses to the collimator and camera were brought into requisition, together with extremely sensitive photographic plates. Glass was spread with a layer of this sulphide and held together by paraffin as a substratum; the phosphorescence was excited by the electric light, and the tablet held in front of the slit. The exposure lasted one minute, when the phosphorescent tablet was again excited, and used as a source of light as before. After forty such excitations the photographic plate was developed; and a band (shown in II.) was registered, which absolutely coincided with the band already registered in the visible spectrum. There was no trace of any radiation having wave-lengths in the ultra-violet. Whether there is any radiation below the red is a moot point; but from the gradually increasing brightness of the spectrum in the yellow, it seems probable that there is such.

The next point to ascertain was the part of the spectrum which excited phosphorescence. A tablet similarly prepared was exposed to the spectrum of the electric light, which showed carbon-bands strongly just above H, and also in the violet near G. A sensitive photographic plate was placed in contact with the tablet, and allowed to remain in contact 40 seconds, after which it was developed. III shows the



locality of the spectrum by which phosphorescence was excited. This agreed absolutely with the visual observations. The exact locality was fixed by the carbon-bands above alluded to, and also by comparing it with a sensitive photographic plate exposed in the ordinary manner. It will be noticed that the ultra-violet rays do not seem to cause phosphorescence in this case.

It now remained to register those rays which destroy the phosphorescence. This was effected in the following manner:— A tablet was first excited, and after half a minute exposed to

the spectrum: those rays which destroyed phosphorescence were distinctly visible, as were also those which excited it. The tablet was placed in contact with a sensitive plate and developed. The wave-lengths were determined by placing a liquid in front of the slit and noting the place where the known absorption-bands in the infra-red region occurred, which is here shown by these localities remaining of the same luminosity on the tablet surrounding the impressed spectrum. The rays which destroy phosphorescence of this description are shown in IV. It will be noted that in the infra-red region is a portion which does not destroy it. When the wave-lengths of this are compared with the wave-lengths of the exciting portion about G and H, it is found that they are octaves one to another. (In the figure the infra-red region is much compressed, owing to the spectrum used being a prismatic spectrum.)

This fact appears remarkable and worthy of note. My own impression is that another band below this may also be traced; but as it was not shown upon the photographic plate, I have not mapped it. Such a band would probably be another octave below the second band. It should be noted that the infra-red band apparently is of the same luminosity as the general luminosity of the plate, and that these rays only feebly excite the plate.

I have endeavoured to make out any spectral difference in the light excited about H and about G, and have failed to obtain evidence as to any alteration in colour. It seems indifferent whether phosphorescence be excited by the indigo or by the violet rays.

I am at present engaged upon other phosphorescent material.

XXV. *Notices respecting New Books.*

Leçons sur l'Électricité et le Magnétisme. Par E. Mascart *et* J. Joubert. Tome I. Paris: G. Masson. 1882.

THIS is a work whose value is not to be judged on its absolute merits, though these are in many places of a high order. Like the former treatise by M. Mascart, it is to be looked on as intended to fill an almost absolute gap in modern French scientific literature. The nation that once could boast of Fourier, Lagrange, Laplace, and Poisson, has never since been without worthy representatives in the front ranks of mathematicians. Galois, Cauchy, Leverrier, and Chasles are names never to be forgotten: and the veteran Liouville, with De Saint-Venant, Hermite, and others, still uphold in that department the prestige of the Academy of Sciences. But the successors of Ampère and Sadi Carnot—masters of the still higher art of applying mathematical reasoning to new fields of

physics—must now be sought in Britain and in Germany rather than in France; the newer great branches of Applied Mathematics have been, till recently, almost ignored there. The Theory of Heat has remained, so far as France is concerned, almost confined to the region so thoroughly and so charmingly explored by Fourier; the real value of Carnot's wonderful treatise was first pointed out by Thomson. That treatise has supplied the Second Law of Thermodynamics, and some of the grandest scientific generalizations ever made. But this was not done in France. Ohm's little adaptation (to electricity) of one of the merest elements of Fourier's great work has been developed in Britain and in Germany into one of the most extensive and most rapidly growing branches of physics —profoundly valuable alike in theory and in practice—which, till quite recently, was practically known only in its beggarly elements to the most illustrious of French scientific men. Some detached portions of Faraday's splendid discoveries were no doubt developed in France, but only as isolated facts; the grand chain of deductive reasoning by which he was led to them, and by which he coordinated them, was almost completely ignored. Where shall we seek for a modern French work containing any thing new on Vortex-motion, on Discontinuous Fluid-motion, on the Kinetic Theory of Gases, or on the theories of Anomalous Dispersion or of Fluorescence? Where for a contribution to the more modern treatment of the size of the grained structure of matter? The best of recent French scientific men seem to have been content to work only on *some* of the lines of *some* of their more illustrious predecessors. Fizeau, Foucault, and Cornu are worthy successors of Fresnel; but Ampère has had no successor in France. All countries appear to pass through these peculiar phases of arrested development. Newton's grand achievements effectually paralyzed, for a century at least, the progress alike of pure mathematics and of the true theory of light, so far as his own countrymen were concerned. Young's brilliant career made ample amends for this in the matter of the Theory of Light; but we had to take our mathematics from France and Germany in the first quarter of the present century. It would seem that it is next to be Germany's turn for a partial eclipse of a similar kind. Gauss and Riemann have made possible the existence there of names as great as their own—Helmholtz and Kirchhoff, for instance; but the men of the rising generation seem to be satisfied, like the French for the last fifty years, or like the British for a century after Newton, merely to swear by (and comment upon), while infinitesimally extending, the words of their *own* present Masters. It is this self-containedness, so to speak, that arrests the growth of science in a nation. The human family, in its every branch, needs all the help it can get from the other branches; and the real reason for France's present deficiency in the theories of Heat and Electricity is its fatal neglect of *every thing not written in French*.

The recovery in France, first seriously set afoot by the lamented

Verdet, seems now really likely to progress ; and all men of science will hail with satisfaction the well-conducted efforts which M. Mascart has made to promote it. What an enviable position he occupies ! To be the first, or practically the first, to instruct his countrymen in the great life-work of Clerk-Maxwell ! to open to them the storehouses filled to overflowing with marvels by Helmholtz and Thomson ! to show them, once for all, that great as may be the services of an individual, or even of a race, science progresses with almost infinitely greater rapidity by the mutual action between individuals and between races—the more widely different they are the better !

After what we have said, it would be improper to formally criticise the work before us. Almost all of its contents have been long known to those whom they concern, alike here, in Germany, and even in Italy. When Babbage, Herschel, and Peacock introduced the more modern mathematics into England, they contented themselves with modestly translating Lacroix's work ; and we might be tempted to think that a translation of Clerk-Maxwell's 'Treatise on Electricity' would have been a greater boon to France than the present work. But a large amount of the *matter* of that work is here ; and that, after all, is the chief question. It is too much overlaid, however, with details of small contributions to the science, whose place might advantageously have been supplied by theorems of a higher value. There is therefore, here and there, rather a superfluity of sack ! But the authors seem to have mastered fully the leading features of the most modern treatment of the subject; and they have put these in forms (always clear and neat as it is the peculiar privilege of their countrymen to be able to do, while) sometimes considerably improved from those in ordinary use either here or in Germany. One great defect, however, we cannot pass over. This is the absolute want of references to the various original sources of information, though of course the authors are named. This is totally indefensible. Without occasional, and often frequent, study of original memoirs, little good progress can be made ; and a few additional pages would have added infinitesimally to the cost of so large a work, while enormously increasing its value.

We do not suppose that the work is likely to have much circulation out of France ; but that is of little moment. Let us hope that M. Mascart's countrymen will avail themselves of this excellent opportunity of attaining the higher level of this fascinating subject, which is at present temporarily occupied by some of their neighbours. Once raised to that level, they are very unlikely (if the past furnishes any criterion) soon to fall again below it. There seems to be little doubt that it is from the point of view furnished by electrical phenomena, properly coordinated and comprehended, that the next really great advances in physical science are to be looked for. He who discovers the true *mechanism* of friction, of thermo-electric currents, and of electric inertia, will take a step of quite as important a character as that which Newton took with regard to Gravitation or Huyghens with regard to Light.

An Old Chapter of the Geological Record with a New Interpretation, or Rock Metamorphism (especially the Methylosed kind), and its resultant Imitations of Organisms: with an Introduction, giving an Annotated History of the Controversy on the so-called Eozoon canadense, *and an Appendix. By* Professors W. King, *Sc.D. &c., and* T. H. Rowney, *Ph.D. &c.* London: Van Voorst.

The present volume is the latest outcome of the *Eozoon* controversy. It commences with an introduction of fifty-seven pages, more than one fourth of the whole book, giving an epitome of the literature (already formidable) which the discovery, rather less than a quarter of a century since, of this supposed fossil has already evoked. The different publications are arranged in chronological order, with abstracts of their principal contents. This is a valuable part of the authors' work, and will form a useful compendium of the literature of the subject. It would, however, have been still more valuable had it not been annotated, or if Messrs. King and Rowney had been able to refrain from passing sarcasms, which are often, to say the least, in bad taste, and occasionally, from their obvious unfairness, arouse in the reader's mind a feeling of scepticism as to whether the authors could possibly do justice to the views of an adversary. They may no doubt plead in extenuation that they are not the only sinners in this respect, perhaps that they were not the first to import bad temper into a scientific discussion; at the same time, they would certainly have improved their position, if they had preserved their equanimity in the historical part of their work, and reserved their sneers and retorts for the more controversial portion.

The work itself discusses, after two or three preliminary chapters on "ophites" in general, some wider questions concerning mineralized and methylosed metamorphic rocks. By the former of these terms is meant a rock which has had "the original substance of its members crystallized into minerals of various kinds;" by the latter, one which has had "the same minerals altered or replaced by chemical reactions." The distinction—which, however, is perfectly well known to all petrologists, though the authors seem to treat it as if it were a discovery—is undoubtedly an important one, though in practice it is often by no means easily maintained; and we may consequently doubt whether it was worth while cumbering the literature of science with another word of foreign origin. The work next passes on to discuss the origin of ophites. Here the authors point out that serpentinization is effected in certain deposits without the intervention of mineralization; that many ophites were originally sediments, others igneous rocks; and discuss the origin of hemithrene and other related calcitic rocks. Then comes a chapter on the minerals characteristic of ophites and related rocks, especially of peridote, followed by some on the origin of crystalline limestones, dolomites, &c., and on the age of ophites.

A lengthy appendix follows, treating of various questions, some of them rather remotely connected with the subject-matter of the

book. One of these also exhibits the authors' fondness for "comfortable words" from tongues not generally understood by the vulgar, as it is entitled *Thalassa* and *Xera* in the Permian Period. We are doubtless glad to learn that Messrs. King and Rowney understand Greek, or at any rate know enough of it to use an English-Greek lexicon; but we fail to see why, in this matter, plain English would not have equally well expressed their meaning.

The work is illustrated by nine plates and some woodcuts. The latter are only diagrammatic; the former are far from well executed. Whether this is the fault of the authors or of their lithographers we are, of course, unable to decide; we may, however, say that in other cases where we have found our leaning (to which we confess) towards the organic origin of *Eozoon* somewhat shaken by their assertions and arguments, we have felt our faith is refreshed by turning to their illustrations, which represent structures, like those upon which Dr. Hahn* has lately expended so much pains, which to our eyes have only a rough general resemblance to those unquestionably organic, or claimed as such, in the best specimens of *Eozoon*.

The book, we may add, is not well edited. A long list of *corrigenda* is given; and there are several other mistakes which have altogether escaped the authors' notice. Some are no doubt merely printer's errors; but others are sufficiently grave to show a want of due care in passing the work through the press.

We do not purpose in our present notice to enter into the vexed question of the nature of *Eozoon canadense*, further than to say that the present work contains a very full statement of the authors' reasons for believing it to be only a mineral structure; and a large amount of curious and instructive matter is brought together which deserves to be well considered by those who incline to the other view.

We pass on, then, to some questions of a more general nature raised by the present work. First, as to the classification of ophites in the opening chapter, which is "offered indeed," as they state, "more for practical purposes than as being a natural one, though it is not altogether devoid of the last character." It is as follows:—

A. Silacid Ophites.

Serpentinytes.
Talc schist, Rensselaeryte (potstone).
Sepiolytes, Magneso-argillyte ("Argille magnésienne," *Delesse*).

* In reference to this we may, in passing, note a paragraph in the American journal 'Science,' which states (vol. ii. p. 410) that Dr. Hahn exhibited his slides to Mr. C. Darwin, who had no sooner "peered through the microscope on one of the finest specimens, when he started up from his seat and exclaimed 'Almighty God! what a wonderful discovery! wonderful!' and after a pause of silent reflection he added, 'Now reaches life down!'" A story so circumstantial one would think must needs be true; but we have the best authority for characterizing it as simply fabulous.

a. *Sub-silacid Ophites.*

Chlorargillytes.
Agalmatolytes.
Chlorite-schists, Hydro-phlogopite schists.
Talc gneisses (Protogines).
Peridolytes* (Lherzolyte, Dunyte, Picryte, Ossipyte, &c.)
Ophi-euphotides.

B. Silo-carbacid Ophites.

Ophi-calcites.
Ophi-magnesites.
Ophi-dolomites.

b. *Sub-silo-carbacid Ophites.*

Malacolophytes.
Hemithrenes.
Calci-micaschists.
&c. &c.

Putting aside all questions as to the nature of *Eozoon*, we confess ourselves entirely at a loss to understand the principle of this classification. Clearly it is neither petrological nor mineralogical. In a work purely chemical, an arrangement which depends solely on chemical composition might be allowable. In certain cases also it might be of advantage to pay regard solely to mineral constituents; but inasmuch as the scope of the authors' work is, to say the least, distinctly petrological, we must protest against one which groups together rocks which not only differ markedly in structure, but also, by the authors' own admission, are sometimes of igneous, sometimes of sedimentary origin. Further, we are at a loss to determine what principle underlies the above classification. At first we supposed it to be chemical, and that Ophite, in the authors' minds, was equivalent to "a rock characterized by the presence of minerals containing silicate of magnesia," *i. e.* in which this is an important constituent; but we are baffled by finding among the members (and that, too, next the "Peridolytes,"—*i. e.* rocks consisting mainly of silicate of magnesia), talc-gneisses (Protogines). In the latter, if the authors use the word Protogine in the ordinary sense (*i. e.* to designate such a rock as we find in the heart of the Mont Blanc *massif*), even the presence of talc is disputed by some authorities, and in any case it is certainly subsidiary; for the percentage of magnesia, in all the analyses which we have seen, is extremely small. Such a classification, then, rests on no principle whatever, and is only calculated to throw the student into hopeless confusion, traces, indeed, of which are to be detected in the book itself. The authors do not appear to have clearly made up their

* We are at a loss to understand why the authors persistently write this word as above. The recognized textbooks always have it Peridotite, or (to follow another spelling of the authors in which they are not always consistent) Peridotyte; and as it is named from the mineral Peridote, this seems the only possible orthography. Is this an example of *methylosis*?

minds as to what is meant by the word serpentine. For this there is, no doubt, excuse in the fact that few names have been used more vaguely: the unfortunate accident that the same name has been applied to a mineral and a rock (an accident which the authors themselves attempt to remedy in the case of dolomite, and should in very consistency have done in this also) has tended much to cause this confusion. It has been augmented by the general neglect of petrology by English geologists; we had almost written the contempt, all but expressed for it, by not a few of the more eminent among them. It has been further augmented by the fact that rocks which chemically, mineralogically, and sometimes petrologically, widely differ from true serpentines, to the unaided eye resemble the latter; so that a rock which is only serpentinous may often, at first sight, be mistaken for a serpentine,—this being a rock consisting chemically of about 39 per cent. of silica, 36 per cent. of magnesia, with probably about 12 per cent. of water, and 7 per cent. of iron oxides, and rather variable quantities (usually not exceeding, and, except in the case of the first, almost always less than 2 per cent.) of alumina, lime, nickel, &c.—a rock, in short, which, chemically, lithologically, and petrologically may be represented by the serpentine of the Lizard. There being then such a type in not a few regions, a nomenclature which neglects it, and a classification like the above, appear to us as objectionable and as fatal to all progress in petrology as it would be to put a basalt with a clay slate, because examples might be found containing about the same proportion of alumina: indeed we are much perplexed at not finding the former rock inserted in the above classification, seeing that it certainly contains a considerable percentage of magnesia.

An instance of the confusion which the above hazy definition of ophites has caused in the minds of the authors themselves (or possibly a proof of their inability to view a question judicially) may be found in their introduction (p. xliv), where they state they are unable to understand how a writer can "believe in the organic nature of *Eozoon*" and yet assert "that he has never seen a serpentine which was not intrusive." To this they append the remark, it "is a puzzle to us, as it must be to Eozoonists, considering that their doctrine is based on the sedimentary or aqueous deposition of eozoonal serpentines. But is not eozoonism full of inconsistencies?" Any careful reader will see at a glance that the guarded statement in the last clause is not equivalent to saying "there is no serpentine which is not intrusive." The context also of the letter quoted, and every paper which we can remember published by the author cited, always uses the term serpentine in the strict sense mentioned above. Further, there is no more inconsistency in believing that serpentine (mineral) may occur sometimes in a rock of igneous, sometimes of "aqueous" origin (assuming that the mineral in the eozoonal "ophicalcites" is serpentine) than in stating that quartz-crystals are found both in trachytes and in the interior of organisms in Cretaceous deposits. Indeed, if we remember aright, the above author has been at some pains to show that, even in the true ser-

pentine rock, the mineral is a secondary product. This confusion is continued in the authors' speculations as to the origin of such serpentines as that of the Lizard. It has been shown during the last few years by several independent observers that, chemically and microscopically, there are the strongest reasons for believing that these serpentines are in all cases altered peridotites. The process of transition from the wholly unaltered olivine rock to that in which every characteristic structure of the olivine has been obliterated has been laboriously traced and described: yet Messrs. King and Rowney simply reiterate their opinion, that it has been derived by methylosis from a "dolerite (wacke or melaphyre) like that of Bufaure in the Tyrol"*. No suggestions are made as to the means by which the large percentage of silicate of alumina (necessarily present in the felspar of a dolerite, and usually a very obstinate constituent, declining to yield to this kind of methylotic action) has been removed; and but little confirmatory evidence is adduced beyond the vague statements of authors who lived before the microscope was applied to petrology. In the same hasty way, the bronzite crystals in the Lizard serpentine are held to be pseudomorphs after black augite; and a chemical and microscopical demonstration that they cannot be distinguished (allowing for partial hydration) from varieties of enstatite is cited as confirmatory of their idea that the "crystals are pseudomorphs of chlorite after augite."

We do not deny the possibility of the alteration of augite into a mineral which might be mistaken for enstatite or bastite; but after a considerable experience in the microscopic study of augite and its changes, we could not accept the statement without a clear demonstration.

The above case, however, is simply illustrative of a tendency which runs through and vitiates the whole work, viz. to postulate the very points which are of vital importance to the argument, and which are denied by not a few authorities at least as experienced as the authors themselves. The following are a few instances of begging the question:—Augite and hypersthene are placed among mineral species "common to ordinary metamorphics." The authors are surely aware that at any rate normal augite cannot be called common in metamorphic rocks, and that the metamorphic origin of any important mass of hypersthene rock is a matter of dispute. The "green marble of Connemara" is assumed to be post-Archæan, and the Upper Gneiss series of Scotland Lower Silurian; but the

* The authors' remark about the opinion that "ophite, or its essential component serpentine," has originated from peridotic rock, is in itself a proof of their hasty way of looking at an adverse opinion. "It cannot be denied that the common occurrence at Snarum of pseudomorphs of serpentine after peridote, and the frequent association of the two minerals in other places, may be taken as good evidence in favour of this view; but it would be just as reasonable to assume that basalt, because it usually contains a large proportion of peridote, was generated out of masses of this mineral." *Just as reasonable*! Is there any evidence of the replacement of olivine by a lime-felspar, accompanied, too, as it would here be, by an entire change of the intimate structure of the rock?

age of all the metamorphic rocks of Scotland and Ireland must be regarded, after the conflicting statements of the last few years, as still an open question; and, though the authors are more cautious here, we may extend the remark to their comments as to the Alpine gneisses and schists. While on that topic, we may note that they appear to be ignorant that the metamorphism of the great dolomitic masses of the Tyrol differs greatly from that of the Alpine gneisses and schists. Indeed, unless it be proved that the magnesian constituent is the result of methylosis, these masses are no more metamorphic than most of the British Carboniferous limestone. In fact they are just as much and just as little metamorphosed as the Permian dolomites (on which, by the way, the authors make some valuable remarks which are a real contribution to the perplexing history of this rock). A similar want of caution characterizes the following remark:—"Studer long ago discovered ammonites and belemnites in a similar but less crystalline deposit (that is, a crystallized limestone associated with gneiss on the Jungfrau), lying between gneisses at Mettenberg, near Grindelwald" (p. 32). Those who know the region will hesitate to accept the apparent induction without clearer evidence than is generally produced in this and, we may add, other cases. Some also of the citations of opinions, apparently favourable, are only true when the word "ophite" is used in the vague sense in which the authors employ it.

We had purposed to comment upon two other subjects mentioned by the authors—the "striping of triclinic felspars," and the secondary origin of peridote; but space will not allow us to say more than that the former structure, although evidently a puzzle to them, is generally held to be the result of twinning, and that their arguments on the latter question appear to us as ingenious and as unconvincing as those by which Daubuisson proves that basalt is of "aqueous origin."

Journal and Proceedings of the Royal Society of New South Wales. 1880. Vol. XIV. *Edited by* Prof. A. Liversedge. 8vo, 392 pages. With ten Plates. Sydney, 1881.

In this volume, astronomy, meteorology, botany, organic and inorganic chemistry, lithology, mineralogy, palæontology, and the study of local water-supply are all and each made sources of information to the world at large, as well as to the Society in particular, by some twelve of its energetic members. There is also an elaborate report on morbid psychology, treating of the causation and prevention of insanity, by one of its medical members.

XXVI. *Intelligence and Miscellaneous Articles.*

ON THE FORMATION OF PEROXIDE OF HYDROGEN DURING COMBUSTION. BY ALOIS SCHULLER.

WHEN determining the heat of combustion of hydrogen*, I remarked during the combustion of oxygen in hydrogen, besides the usual very small flame, a second, much larger, blue so-called

* Schuller, Wied. *Ann.* ii. p. 373 (1877).

flame, the closer investigation of which led to the conclusion that during the combustion of oxygen and hydrogen, under certain circumstances, peroxide of hydrogen is also produced. As this subject has not, to my knowledge, yet been thoroughly discussed, I will briefly communicate the experimental results which up to the present time I have obtained.

As previous experiments had already indicated that the blue flame is conditioned by accidental circumstances, I sought before all things to ascertain these. It then appeared that the flame is not visible when hydrogen burns in oxygen, but only on the combustion of oxygen in hydrogen, provided that both are free from atmospheric air,—and, further, that it comes out better when to one of the gases a little sulphurous acid is added, while it disappears if the gases are passed over potassium hydrate. In consequence of this I removed the sulphuric acid (from which the gases appear to have taken up a trace of sulphurous acid) from the gasholder. The blue flame was now no longer visible; but it immediately became so after the addition of sulphurous acid. The different experiments all led to concordant results, whether a smaller or a larger combustion-space (from 5 to 500 cub. centim.), or gases produced by electrolysis or otherwise were employed. All this shows that the blue flame stands in connexion with the sulphurous acid contained in the gases.

During these experiments the water formed each time was also examined. A trace of sulphuric acid was found in it as often as the blue flame was visible, which was absent whenever the latter did not appear. Besides this, the water usually took up some peroxide of hydrogen, which was proved by iodide-of-zinc starch and sulphate of iron, by permanganate of potassium, and by the so-called perchromic-acid reaction.

These in themselves interesting facts formed the subject of further experiments. First of all I convinced myself that, under certain conditions, H_2O_2 is produced both when H burns in O and when O burns in H, but that the amount differs very much, according to the conditions of the experiments. Of essential importance is the velocity with which the burning gas penetrates that which surrounds the flame: if this velocity is very inconsiderable, traces only of H_2O_2 appear, while with considerable excess of pressure striking reactions are obtained. Thus, in one case, during the combustion of oxygen in hydrogen at the temperature of the room, I obtained water containing 0·0008 gram H_2O_2 in 1 gram, therefore nearly 0·001 of the whole; and it appears to me very probable that under more favourable conditions considerably more might be produced. The presence of SO_2, while, on the one hand, it conditions the blue flame, diminishes the amount or prevents the occurrence of peroxide of hydrogen; at the same time the corresponding amount of sulphurous is converted into sulphuric acid. These, as well as other experimental results, indicate that the blue light has its origin in the combination of H_2O_2 and SO_2 to form H_2SO_4.

These circumstances explain, on the one hand, why in the above-mentioned calorimetric measurements no peroxide of hydrogen could be demonstrated (for in the slow combustion no more appears to have been formed than was necessitated by the minute quantity of sulphurous acid contained in the gases), and, on the other, why the amount of sulphuric acid contained in the water was so small; it appears to have arrived in the combustion-space not as sulphuric, but as sulphurous acid.

The same circumstances seem also to explain the green edge shown by the hydrogen-flame when iodine is present. The simultaneous formation of iodic acid, which has been pointed out by G. Salet*, may probably be connected with the occurrence of peroxide of hydrogen.

After these facts it appeared probable that in the third case of the combustion also, viz. during the explosion of oxyhydrogen, besides water, peroxide of hydrogen is produced, which supposition was confirmed by experiment. It could be demonstrated in all the experiments made in relation thereto: a scarcely perceptible quantity of the water formed in the explosion gave the iodine reaction; and a drop was sufficient to show strikingly the perchromic-acid reaction.

During explosion the peroxide of hydrogen appears to be formed in still larger quantity than in ordinary combustion; but I have not yet succeeded in carrying out a reliable quantitative determination, because all the tubes employed, of the length of about 1 metre, were shattered. These unsuccessful experiments, however, led to an in other respects interesting result:—All the tubes were very strong, so that one of the weakest required a *continued* pressure of 100 atmospheres to burst it. To this must be added that it was always only the part most distant from the place of ignition that was burst, even when, with 3 millim. thickness of wall, its bore was only 3–4 millim. in diameter—whence it follows that very pure oxyhydrogen, such as I employed, occasions in long tubes, in the part furthest from the place of ignition, a pressure of hitherto unsuspected magnitude. This holds good even when the tubes are narrowed by one or two coils of cord to 2 millim. internal diameter; but a slight content of air modifies the result considerably. I believe that this can be accounted for as follows:—In consequence of the considerable pressure arising at the place of ignition, the still unburnt gas is driven with great velocity along the tube; hence the slowly spreading ignition meets continually with more and more powerfully compressed gas; wherefore the pressure in the corresponding part of the tube increases more and more, till finally, at the end of the tube, the most violent action takes place. In my experiments the pressure there was much greater than it can have been, according to Bunsen†, in the most favourable case of simultaneous ignition of the entire mixture.

* *Comptes Rendus de l'Académie des Sciences*, lxxx. p. 884 (1875).

† Bunsen, *Gasometrische Methoden*, (1875) p. 256; cf. (1877) p. 331.

The driving forward of the oxyhydrogen gas here supposed has also been assumed by Mallard and Le Chatelier*, in order to explain the increase of the velocity of propagation of the ignition observed by them.

From what has been adduced, it follows, *inter alia*, that in precise determinations of the heat of formation of water the H_2O_2 formed must also be taken into account, and, further, that, in the case of explosion, the loss of heat occasioned by the breaking must be taken into consideration.—Wiedemann's *Annalen*, 1882, no. 2, vol. xv. pp. 289–292.

ON THE LAW OF RADIATION. BY J. VIOLLE.

The intensity of a simple radiation emitted by incandescent platinum is very accurately represented by the formula

$$I = mT^3(1+\epsilon\alpha^{-T})^T,$$

as I have previously pointed out, and as is proved by the following Table, which contains the values calculated by means of this formula for a portion of the measurements before mentioned :—

t.	$\lambda=656$ $\epsilon=0{\cdot}011495$ $\alpha=1{\cdot}00045$.		$\lambda=589{\cdot}2$ $\epsilon=0{\cdot}04295$ $\alpha=1{\cdot}00044$.		$\lambda=535$ $\epsilon=0{\cdot}04467$ $\alpha=1{\cdot}00043$.	
	Calculated intensities.	Differences from the observed.	Calculated intensities.	Differences from the observed.	Calculated intensities.	Differences from the observed.
775°	...	...	0·05	0		...
954	1	0	1	0	1	0
1045	3·1	−0·2	3·4	−0·2	3·6	− 0·1
1500	153	0	218	−1	324	+17
1775	501	−6	812	+3	1365	0

But if the attempt be made to represent by the same formula the numbers of Dulong and Petit relative to the radiation of their thermometer between 80° and 240°, good results are not obtained. Nor does the celebrated formula of those two physicists, $I=m\alpha^t$, agree with the measurements obtained with incandescent platinum; and the expression $I=mT^4$, recently proposed by M. Stéphan, giving for the relative intensities at 954°, 1045°, 1500°, and 1775° the numbers 1, 1·33, 4·36, and 7·73, cannot be adopted.

On the other hand, a very satisfactory representation of the whole of the measurements is obtained by means of the formula

$$I=mTb^{T^2}\alpha^T,$$

in which T represents the absolute temperature, m a constant coefficient, b the number 0·9999938, $\alpha=1{\cdot}03550-13\lambda$, λ being the wave-length in millimetres.

In fact, if we apply this formula, first, to the platinum radiation, we shall have :—

* *Comptes Rendus*, xciii. pp. 146, 147 (1881).

t.	λ=656.		λ=589·2.	
	Calculated intensities.	Differences from the observed.	Calculated intensities.	Differences from the observed.
775°	...	...	0·04	−0·01
954	1	0	1	0
1045	3·0	−0·3	3·2	−0·4
1500	161	+7	219	0
1775	507	0	807	−2

t.	λ=535.		λ=482.	
	Calculated intensities.	Differences from the observed.	Calculated intensities.	Differences from the observed.
954°	1	0		
1045	3·4	−0·3		
1500	311	+4	142	1
1775	1371	+6	807	5·7

The calculations were made with the values of α which appeared to accord best with the observations. These values,

1·02713, 1·02772, 1·02838, 1·02935,

satisfy the formula $\alpha = 1{\cdot}03550 - 13\lambda$, with the respective differences

−0·00016, +0·00012, +0·00015, −0·00012.

The experiments of Dulong and Petit are equally well represented by the same formula, assuming $\alpha = 1{\cdot}01161$,—that is to say, supposing $\lambda = 1838$, which is perfectly admissible according to the measurements of M. Mouton, who has determined the wave-lengths up to 2140. We have, in fact:—

Excess.	Velocity of cooling.		Δ.
	Determined by Dulong and Petit.	Calculated by my formula.	
80°	1·74	1·71	−0·03
100	2·30	2·35	+0·05
120	3·02	3·10	+0·08
140	3·88	3·97	+0·09
160	4·89	4·96	+0·07
180	6·10	6·10	0
200	7·40	7·39	−0·01
220	8·81	8·84	+0·03
240	10·69	10·46	−0·23

The differences are not, in general, greater than those existing between the numbers calculated by Dulong and Petit from their formula and the observed numbers.

From the whole of these facts it appears to me to follow that, between 0° and 1775°, the law of radiation can be represented by the formula

$$I = mTb^{T^2}\alpha^{T}.$$

Comptes Rendus de l'Académie des Sciences, t. xcii. p. 1204.

ON WHEATSTONE'S BRIDGE. BY K. F. SLOTTE.

The length of the platinum wire belonging to this apparatus, which cannot be exactly determined by measurement, can, it is well known, be ascertained indirectly by comparing and exchanging resistances*. The following procedure is a modification of this method which may not be without advantages.

Let s be the length of the wire, a and b that of its two divisions when two resistances w_1 and w_2 are inserted and the galvanometer shows no current. Then is

$$\frac{w_1}{w_2} = \frac{a}{b} = \frac{s+a-b}{s-(a-b)} = \frac{s+d_1}{s-d_1}, \quad \ldots\ldots\ldots \quad (1)$$

in which $a-b$ is put $= d_1$. If now the resistances w_1 and w_2 be exchanged and the movable contact shifted till again no current passes through the galvanometer, this displacement, taken as positive or negative according to whether a is more or less than b, is equal to d_1, which quantity can be directly determined by reading it off upon the scale of the apparatus.

If in the same manner w_2 be compared with a third resistance w_3, and, again, w_3 with w_1, and if the displacements corresponding to d_1 be denoted respectively d_2 and d_3, then we have

$$\frac{w_2}{w_3} = \frac{s+d_2}{s-d_2}, \quad \ldots\ldots\ldots\ldots\ldots \quad (2)$$

$$\frac{w_3}{w_1} = \frac{s+d_3}{s-d_3}. \quad \ldots\ldots\ldots\ldots\ldots \quad (3)$$

From (1), (2), and (3) we get

$$1 = \frac{(s+d_1)(s+d_2)(s+d_3)}{(s-d_1)(s-d_2)(s-d_2)},$$

and finally, by solving the last equation,

$$s = \sqrt{-\frac{d_1 d_2 d_3}{d_1+d_2+d_3}}. \quad \ldots\ldots\ldots\ldots \quad (4)$$

Three determinations effected by this method, in which w_2 was chosen approximately equal to $\sqrt{w_1 w_3}$, gave for s the values 1118·11, 1118·75, 1118·6 millim.—Wiedemann's *Annalen*, 1882, no. 1, vol. xv. p. 176.

* See G. Wiedemann, *Galvanismus*, [2] i. p. 254.

ON THE COMBINATION OF CARBONIC ACID AND WATER.

BY S. WROBLEWSKI.

If carbonic acid be sufficiently compressed in contact with water, in a space kept at zero, the portion of the acid not absorbed liquefies, and two distinctly separate liquids are obtained :—water, more or less saturated, below ; and liquid carbonic acid, lighter, above. The pressure being slowly lessened, the carbonic acid volatilizes, and the whole returns to the original state. But if we compress the carbonic acid almost to liquefaction and then allow the gas to expand a little, so as to produce a trace of solid substance either in the water or on the sides of the tube, the following phenomenon is observed :—Every time that the pressure while being augmented passes through a determined point (which I will call the *critical* pressure), the tube becomes covered with an opaque rime ; and every time the same point is passed through in diminishing the pressure, the rime disappears. The critical point, at zero temperature, is at the pressure of 12·3 atmospheres. The phenomenon can be reproduced at pleasure, if the pressure be not too much diminished, and consequently the ice does not completely disappear. The latter can be preserved as long as we please, if the pressure remains greater than that above indicated.

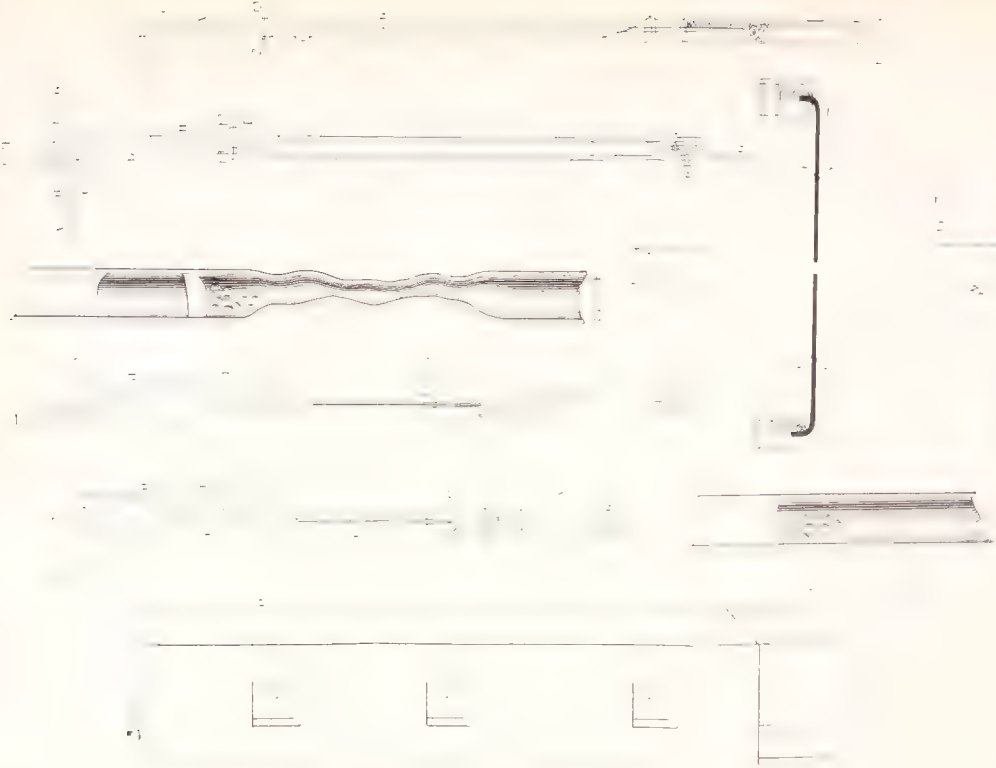
The phenomenon is likewise produced at higher temperatures than zero. The value of the critical pressure rises with the temperature. At*

°		atm.	°		atm.	°		atm.
0·48	it is	12·7	3·6	....	17·9	6·1	....	23·3
2·7	....	16·7	5·3	....	21·8	6·8	....	26·1

No such result is obtainable by experimenting with water and air. At zero, with the strongest expansion possible with my apparatus, a little ice is seen to form in the water, which soon melts without producing any rime.

The explanation of the above-described phenomenon appears to me necessarily to involve the existence of a hydrate of carbonic acid, readily dissociable, and capable of being formed by pressure, like M. Ogier's chlorhydrate of phosphuretted hydrogen. The critical pressure would be the dissociation-tension of the formed hydrate. The proportion of water and acid seeming to have no influence on the value of the critical tension, it is probable, from M. Debray's researches, that there is only one hydrate of this acid, containing equal volumes of carbonic acid gas and aqueous vapour. In order that it may readily crystallize, a portion of substance already crystallized must be present : the expansion of the gas, which determines a local lowering of temperature, serves to produce this effect.—*Comptes Rendus*, Jan. 30, 1882, p. 212.

* The numbers are the means of the pressures at which the rime begins to disappear and to form.



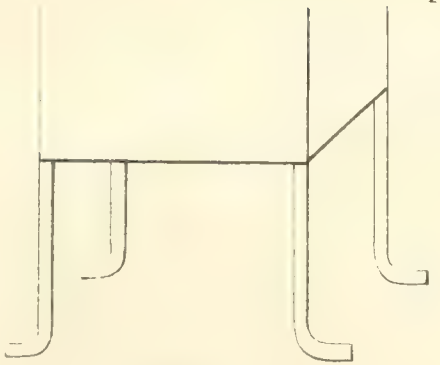


Fig. 1.

Fig. 2.

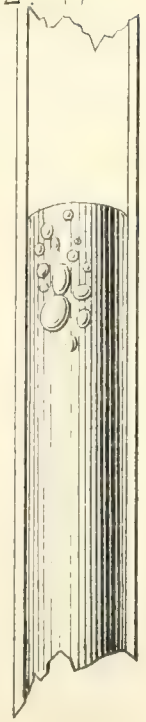


Fig. 3

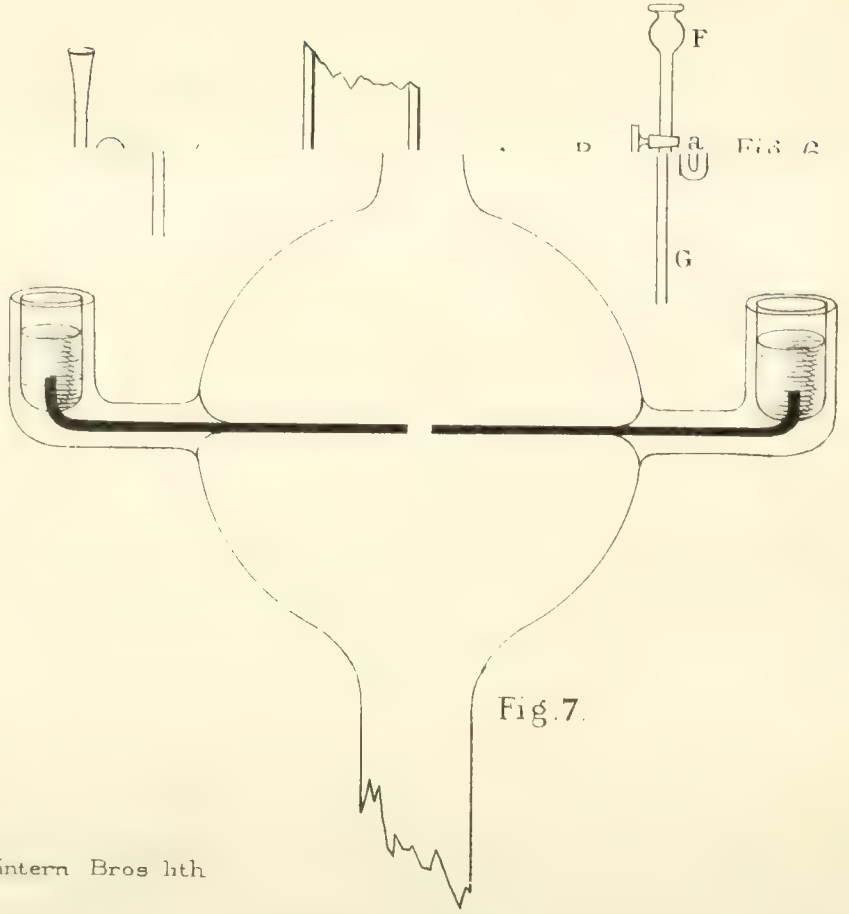


Fig. 6

Fig. 7.

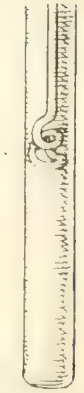


Fig. 8.

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PHILOSOPHICAL MAGAZINE

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JOURNAL OF SCIENCE.

[FIFTH SERIES.]

APRIL 1882.

XXVII. *An Examination of Vacua.*

By J. B. HANNAY, *F.R.S.E., F.C.S.**

[Plate VI.]

THE discovery detailed in a paper read before the Royal Society †, that solids are capable of absorbing and fixing large quantities of gases, led me to an examination of various surfaces in contact with gases. It soon became apparent that not only is there a closely adherent layer of gas on every surface, as shown by Crookes and Liveing and Dewar, but that the gas seems even at ordinary pressures to penetrate into, or combine with, the surface of the glass, and that it is with extreme difficulty that even a portion of this gas is driven off, the greater portion remaining fixed even to the softening-point of the glass. Experiments soon led me to the conclusion that the estimates made as to the real extent of the evacuation of spaces were fallacious; and a serious examination of the extent of evacuation was attempted. There was also in my mind the wish to test, if at all possible, the idea held by Euler, and advocated by Grove and others, that radiation may be propagated by ordinary matter, and not by the luminiferous æther. The idea was, to evacuate a space as perfectly as possible and subject it to intense cold, and then test whether or not it would pass radiant energy. As it was evident that the pressure of mercury vapour is quite appreciable at ordinary temperatures, and is in itself quite sufficient

* Communicated by the Author.

† Proc. Roy. Soc. No. 214, 1881.

for the propagation of radiation, and as the apparatus would require to be heated to a high temperature, mercury as an evacuating agent was abandoned, and fusible metal substituted. The "barometric height" of fusible metal is about 41 inches, so that all apparatus had to be constructed on that scale.

The fusible metal was composed of 50 per cent. bismuth and 25 per cent. tin and lead respectively; and its fusing-point was 94°. Increase of tin or lead raised the fusing-point; but a considerable increase of bismuth (10 per cent.) only raised the fusion-point a few degrees. A variation of 3 per cent. in any of the constituents made little difference. About 30 lb. of the metal was prepared. A portion was heated in a piece of combustion-tubing over the blowpipe, but gave off no perceptible vapours. Pure hydrogen was passed over 20 grms. of the metal in a porcelain boat in a combustion-furnace at a red heat, but after eight hours it had lost only 5 milligrammes, showing that fusible metal is quite fixed, as the empty boat might lose that weight when heated so long. A portion was placed in one end of a piece of combustion-tubing connected with a Sprengel pump, the end containing the metal being slightly elevated. The tube was then evacuated, and the portion away from the metal heated until no more gas was given off, and the McLeod gauge showed an exhaustion of a millionth of an atmosphere. The fusible metal was then gently fused and allowed to run down to the end which had been heated. The blowpipe was again applied and the metal strongly heated; but it gave off no appreciable amount of gas. The metal was therefore held to be free from dissolved gases: and being found to be suitable for the work required of it, an attempt was made to work a Sprengel pump with it. Although its melting-point was 94°, it was not thoroughly liquid till 115° or thereby; so it was determined to work at a temperature of from 120° to 150°, to ensure free flow of the metal. After several experiments a bath was constructed as in fig. I. It was made of strong sheet iron with double sides and three sets of windows. Its dimensions are, length of bath 6 ft., length of feet 10 in., inside space 10 in. by 8 in. It had a strong iron tube with four arms running down one side, set in a step at the bottom of the bath, and bolted to the top by a nut, so that it could be removed and the glass apparatus attached for experiment. It was found that, when fitted with a loosely fitting lid, the heat from one of Fletcher's heaters was sufficient when passing from below to maintain the interior at a temperature of 300°. The windows were simply pieces of glass made so as to be easily removed to gain access to the interior.

After some preliminary trials the apparatus shown in fig. II. was constructed. The bulb A, which is connected with B by a ground joint, has its lower extremity ground to fit the upper part of the bulb C. D is a tube leading to the mercury Sprengel. E are the bulbs to be evacuated. F is a narrow tube which allows the metal to run very slowly into the pump G. The stopcocks and ground joints were lubricated with Mr. Crookes's invention, burnt india-rubber, which is the only substance of use at high temperatures. I think it acts better at high than at low temperatures, because it soon hardens up on the outside to a hard varnish-like body which is quite impervious to gases. The india-rubber must be the pure variety, such as is used for toy balloons.

The *modus operandi* is as follows :—The apparatus being fixed inside the bath with the joints all made, the stopcocks *a* and *d* are closed and *b* and *c* left open. The ordinary mercury-pump is then set in motion till a good vacuum is obtained. During the evacuation the stopcock *d* is slightly opened, the point being dipped in fusible metal, and the metal allowed to rise till it is just inside *d*, and the cock then closed.

The fusible metal is poured into C through *a*, care being taken to open *a* only very slightly at first. It was found that the fusible metal adheres to the glass, and that no air is admitted with the metal, as would be the case with mercury. When all the metal is in C, the connexion with the Sprengel pump at D is sealed off. The metal is now running very slowly down through F, and acting as a Sprengel pump through G, evacuating the bulbs E. The stopcock *b* is closed, so that the metal collects in B. When B is nearly full the metal is transferred to the top by the following method:—*b* and *c* are opened, transferring the metal from B to A, and then closed, and A disconnected from B: it is then raised and adapted to C, the upper part of which beyond the stopcock *a* is filled with fusible metal. The tube below *d* also being full, no air is allowed to get in; *d* and *a* are then opened, when the metal runs into C. A is now returned to the bottom for a new charge. There is great difficulty in handling the bulbs, owing to the heat, but with flannel and proper clamps the work can be got through. During the evacuation the bulbs E are heated nearly to the softening-point, so as to free them from air ; and after an hour or so, B is allowed to fill up full and the metal to pass up into E, sweeping any residual gas into the upper tube H, where it is imprisoned by freezing the metal by a blast of cold air. B is then emptied, and the exhaustion again proceeded with, E again heated

and filled up, and the residual air again imprisoned in H by a new layer of frozen metal. After the first heating the residual gas does not show itself, as the metal goes right up into contact with that formerly frozen there. It seems that if the quantity of gas is very small, it only forms a layer or coating on the glass, and does not form a bubble. This seems to show that the McLeod gauge only measures the excess of residual gas over that required to saturate the glass surface. When sufficient exhaustion was supposed to have been attained, the bulbs E were sealed off; and here it was found that the whole work had been futile, because, on melting the tube through the metal just below the barometric height, in order to detach the bulbs, the appearances shown in figs. III. and IV. were seen. A large amount of gas was given off whenever the glass became soft enough for sealing. Fig. III. shows the sweating of the gas-bubbles when just softening. Fig. IV. shows a tube melted all round for some distance and the large amount of gas driven off. It must be remembered that this tube had been evacuated so far that it gave no residual gas on passing up the metal.

Some further attempts were made to get the bulbs evacuated again, when it was intended to heat the tube at the point of sealing till it was soft previous to sealing, allowing it in fact to be sucked in till it was nearly closed, pumping away the gases, so that none would come off on melting for the final sealing. Experiments showed that when once the glass was melted in a good vacuum no further gas would come off on remelting. It was found, however, that the stopcocks were all leaky, having warped by the long heating (the operation takes a whole day); and after fighting with this form of apparatus, it was abandoned for that shown in fig. V. Here, instead of stopcocks, the tubes were bent round like safety-funnels, and narrowed very much at the bends; and the stopping was effected by a blast of cold air kept blowing on the joint to be closed, and stopping the blast whenever it was required to be opened—the heat of the bath remelting the frozen plug. The transference of the metal from bottom to top was effected as in the other apparatus. In this the bulbs E were connected with the Sprengel pump by HD, but were afterwards separated by passing metal up into H, sweeping out residual air and freezing it in H, before the final evacuation with the fusible metal was begun. This form was adopted in order that the residual air might be subjected to no pressure, but have a free space to pass out into, thus avoiding condensation on the glass. Some progress

was made with this method; but a new difficulty arose. The fusible metal expands very much after solidification, and often bursts the tube containing it; in fact it always bursts it if allowed to cool far enough; but, as its principal expansion-point is 35°, or 60° below its freezing-point, it was hoped that by keeping the temperature between 50° and 70° a good stoppage would be obtained. I may mention that the expansion of the metal at 35° is so great that, if allowed to solidify in a common deep goblet, it invariably splits it; but it may be allowed to cool and solidify in a shallow stew-pan with safety. After working for some weeks in vain attempts to regulate the temperature of the stoppers, the difficulty was found to be so great that (some point being momentarily overlooked) the apparatus was invariably broken. When it is remembered that working at such high temperatures the glass often cracks, being extremely brittle and having little tenacity, it will be understood that failures were often due to this cause; but when something had gone wrong, and I could not get the metal out without cooling the apparatus, I have often had to stand by and see two days' glass-blowing slowly disintegrated without being able to save a tube.

It became manifest that frozen stoppers would not do, at least where many were required, and stopcocks were not reliable when in communication with the vacuum; so a new apparatus was constructed, where the stopcocks were under pressure, and therefore not liable to leakage, and where leakage could only take place to the unimportant side of the apparatus. The pumping action was also abandoned, and the apparatus shown at fig. VI. used. The tube A was attached to a mercury-pump, and the cocks *a*, *b*, and *c* closed. When evacuating, *b* was slightly opened, and dipped into a pot of metal so that it filled up to the inside of the stopcock, which was then closed. When a good vacuum had been obtained, the tubes B and C were heated and allowed to be sucked in till nearly closed. The same was done with the tubes above and below the bulbs E; and they were also heated nearly to the softening-point. B was then sealed off, and the metal poured through F into D till nearly full, and C then also sealed off from the Sprengel pump. Some metal was left in F, and new burnt india-rubber applied to *a*.

The bulbs E were then strongly heated and *c* opened, so that the metal rose and swept the residual air into H, where it was frozen by cold air. No leakage can take place by *b* or *c*, as they are under more than an atmosphere of pressure, while leakage by *a* is of little consequence, but seldom occurred. The tube at the bottom of D was frozen by the same blast as was

used for H. *c* and *b* are now opened, and the metal run out of E, which is again heated, and new metal passed round from D, which holds sufficient metal for six fillings of E. The residual air, if there is any, is always imprisoned by a new layer of frozen metal in H. The bulbs were then sealed off; but even after all this treatment, a little air was seen to come off when quite melted. The best evacuation obtained in this way was tested as follows. One bulb was placed in a freezing-mixture of calcium chloride and snow, and the other interposed between a standard candle and radiometer kindly lent me by Sir W. Thomson; but no retarding effect further than that due to the glass was observed, as on admitting air to the bulbs no acceleration took place.

It was found that, by increasing the proportion of lead in the alloy by 20 per cent., the expansion on solidification was greatly reduced, and enabled the freezing method of stoppage to be used: this raised the melting-point to 108°. The last apparatus was now modified, so as to dispense with stopcocks, and a new attempt made. The bulbs were also reconstructed for testing the conducting-power of the vacuum for electricity, one bulb being made as shown in fig. VII. In this the platinum wires were led into cups filled with fusible metal, so that no leakage along the platinum wire was possible. Wires were led into the cups from the outside, insulated with asbestos and shellac, and the spark from a strong Holtz machine made to pass. At the highest exhaustion obtained by the Sprengel pump, $\frac{1}{23,000,000}$ of an atmosphere measured by the McLeod gauge, the spark passed as a flash illuminating the glass surface inside; but on exhausting as above with fusible metal, no power I could obtain would cause a spark to pass through the vacuum. A Leyden battery of 12 jars was tried, and a coil giving a 12-inch spark, but with no effect. Mercury vapour seems, therefore, to conduct the spark. An attempt was made to detach the bulbs, but they were fractured. It was found that it was by no means necessary to keep the fusible metal in a vacuum, as it dissolves no gases; and as the great length of the apparatus rendered its construction troublesome and its fracture easy, a new apparatus was made as shown in fig. VIII.

This was constructed to obtain a simple torricellian vacuum, a movable iron bucket below being provided, so that the metal could be raised and lowered. The tube, from the bulbs downwards, was 48 inches long; and the lower end was turned into a spiral, with the termination turned inside the spiral, so that the end could nowhere touch the tube. I need not describe

the treatment, as it was similar to that before employed. The vacuum obtained by this method would not pass a spark. The metal used in this experiment had extra lead added to it, so that it could be frozen.

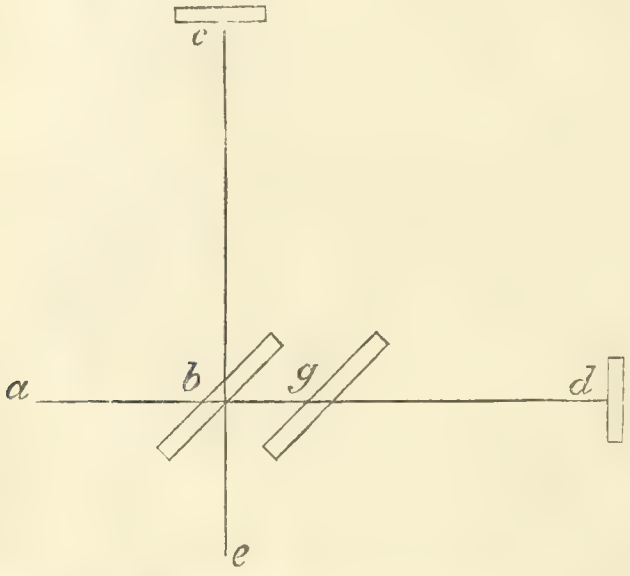
The bulbs were detached in the following way. The metal was passed up for the last time and frozen, and the tube melted through the metal at *a*, leaving a good column of frozen metal between the bulbs and the seal, so that any gas given off could not get into the former. The metal was then lowered out of the bulbs, and again frozen above *b*, where the tube was again melted through, and the air given off kept imprisoned by the solid metal above *b*. This vacuum was tested by the radiometer, but gave no retardation beyond that due to the glass; it would not, however, conduct a spark. To test whether the glass had been really freed from gas by this process, a pair of bulbs were evacuated as before, and the metal passed fully half up the upper one. The bulb was then heated to softening, so that its sides were sucked in in several places just below the surface of the metal; and it was invariably found that a sweat of gas-bubbles was formed between the glass and the metal, showing that the glass still retained air. The only method by which a real vacuum might be obtained (putting aside glass vapour), would be by having the bulbs made of hard glass outside and soft glass inside, so that the inside might be fused and its gas layer driven off, without collapsing the bulbs; but I am not sufficiently skilled in glass-blowing to make such bulbs. As I have been unable to get them made, the experiment has not been tried.

The work, as far as it goes, shows that no conclusions can be drawn as to the nature of properties of high vacua, after such vacuous space has been cut off, by sealing one of the tubes by fusion; and it is doubtful whether the measurements by the McLeod gauge represent the true density of the residual atmosphere. The object of this investigation—the testing of Euler's hypothesis—has not been fulfilled, although nearly six months' work was spent upon it, the accidental breakages making the testing of a method a matter of a month's work. I must record my hearty thanks to my two assistants, Messrs. Campell and McConechy, for the unwearied aid they have given me in this troublesome investigation.

XXVIII. *Interference Phenomena in a new Form of Refractometer.* By Albert A. Michelson*.

In an experiment undertaken with a view to detecting the relative motion of the earth and the luminiferous æther ('American Journal of Science,' No. 128, vol. xxii.), it was necessary to produce interference of two pencils of light which had traversed paths at right angles with each other. This was accomplished as follows:—The light from a lamp at *a* (fig. 1)

Fig. 1.



was separated into two pencils at right angles, *b c* and *b d*, by the plane-parallel glass *b*; and these two pencils were returned to *b* by the mirrors *c* and *d*, whence they coincide along *b e*, where they are viewed by a small telescope at *e*. It is evident that, so far as the interference is concerned, the apparatus may be replaced by a film of air whose thickness is *b c*—*b d*, and whose angle is that formed by the image of *d* in *b*, with *c*.

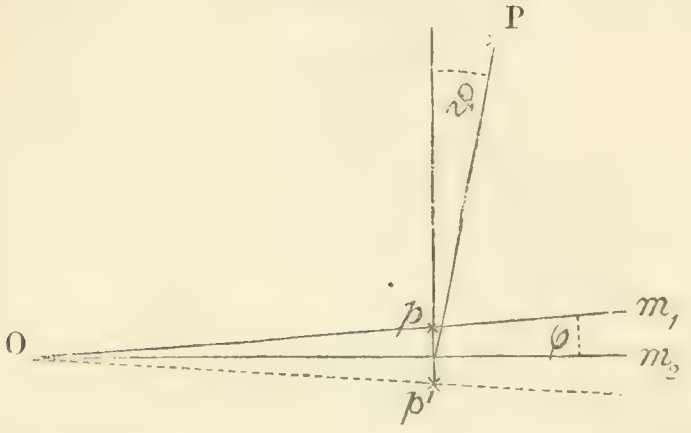
The problem of interference in thin films has been studied by Feussner; but his equations do not appear to give the explanation of the phenomena observed. In particular, in the *Annalen der Physik und Chemie*, No. 12, 1881, on page 558, the conclusion drawn from the preceding equations is that the interference-fringes are straight lines; whereas in the above apparatus they are in general curves, and there is but one case —that of the central fringe in white light—which is straight.

I have therefore thought it worth while to attempt the solution of the problem for a film of air for small angles of incidence, and neglecting successive reflections (which in the

* Communicated by the Author.

above-described instrument do not exist); and though the solution is not perhaps adapted to the general problem, it accounts satisfactorily for all the phenomena observed in the special case. Let O m_1 O m_2 (fig. 2) be two plane mirrors

Fig. 2.



whose intersection is projected at O, and whose mutual inclination is ϕ. The illumination at any point P (not necessarily in the plane of the figure) will depend on the mean difference of phase of all the pairs of rays starting from the source and reaching P, after reflection from the mirrors—a pair of rays signifying two rays which have started from the same point of the source.

If the area of the luminous surface is sufficiently large, the illumination at P will be independent of the distance, form, or position of the surface. Suppose, therefore, that the luminous surface coincides with the surface O m_1. Its image in O m_1 will also coincide with O m_1; and its image in O m_2 will be a plane surface symmetrical with O m_1 with respect to O m_2; and for every point p of the first image, there is a corresponding point p' of the second, symmetrically placed and in the same phase of vibration. Suppressing, now, the source of light and the mirrors, and replacing them by the images, the effect on any point P is unaltered. Consider now a pair of points p, p'. Let ϑ be the angle formed by the line joining P and p (or p') with the normal to the surface, ϑ and ϕ being both supposed small,

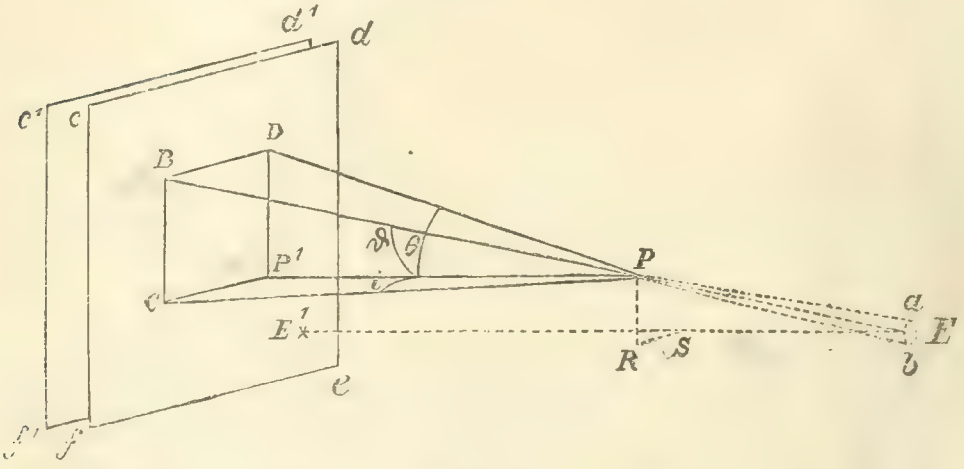
$$\Delta = \mathrm{P}p' - \mathrm{P}p = pp' . \cos\vartheta .$$

The difference between this value of Δ and the true value is $2\mathrm{P}p . \sin^2\frac{\psi}{2}$, where ψ is the angle subtended by pp' at P. If ϑ is a small quantity, ψ is a small quantity of the second

order, and $\sin^2\frac{\psi}{2}$ is a small quantity of the fourth order; consequently $2\mathrm{P}p\,.\,\sin^2\frac{\psi}{2}$ may be neglected. We have therefore, to a very close approximation, $\Delta = pp'\cos\vartheta$, or, substituting $2t$ for pp', $2t$ being the distance between the images, at the point where they are cut by the line Pp,

$$\Delta = 2t\cos\vartheta.$$

Fig. 3.



Let $c\,d\,e\,f$, $c'\,d'\,e'\,f''$ (fig. 3) represent the two images, and let their intersection be parallel with $c\,f$, and their inclination be 2ϕ. Let P be the point considered; P′ the projection of P on the surface; and P B the line forming with P P′ the angle ϑ. Draw P′ D parallel to $c\,f$, and P′ C at right angles, and complete the rectangle B D P′ C. Let P′ P C $= i$, and D P P′ $= \theta$. Let P P′ $=$ P; and call the distance between the surfaces at the point P′, $2t_0$. We have then

$$t = t_0 + \mathrm{CP'}\,.\,\tan\phi = t_0 + \mathrm{P}\tan\phi\tan i,$$

and

$$\Delta = 2(t_0 + \mathrm{P}\tan\phi\tan i)\cos\vartheta,$$

or

$$\Delta = 2\,\frac{(t_0 + \mathrm{P}\tan\phi\tan i)}{\sqrt{1+\tan^2 i+\tan^2\theta}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

We see that in general Δ has all possible values; and therefore all phenomena of interference would be obliterated.

If, however, we observe the point P through a small aperture $a\,b$ (the pupil of the eye, for instance), the light which enters the eye from the surfaces will be limited to the small cone whose angle is $b\,\mathrm{P}\,a$; and if the aperture be sufficiently small, the *differences in* Δ may be reduced to any required degree.

It is proposed to find such a distance P that, with a given aperture, these differences shall be as small as possible, which is equivalent to finding the distance from the mirrors at which the phenomena of interference are most distinct.

The change of Δ for a change in θ is

$$\frac{\partial \Delta}{\partial \theta} = - \frac{2(t_0 + \mathrm{P} \tan \phi \tan i) \frac{\tan \theta}{\cos^2 \theta}}{(1 + \tan^2 i + \tan^2 \theta)^{\frac{3}{2}}}. \quad . \quad . \quad . \quad (2)$$

The change of Δ for a change of i is

$$\frac{\partial \Delta}{\partial i} = 2 \frac{(1 + \tan^2 i + \tan^2 \theta) \frac{\mathrm{P} \tan \phi}{\cos^2 i} - (t_0 + \mathrm{P} \tan \phi \tan i) \frac{\tan i}{\cos^2 i}}{(1 + \tan^2 i + \tan^2 \theta)^{\frac{3}{2}}}. \quad (3)$$

For $\frac{\partial \Delta}{\partial \theta} = 0$ we have

$$\theta = 0 \text{ (or } \Delta = 0).$$

For $\frac{\partial \Delta}{\partial i} = 0$ we have

$$(1 + \tan^2 i + \tan^2 \theta) \mathrm{P} \tan \phi = (t_0 + \mathrm{P} \tan \phi \tan i) \tan i,$$

or

$$(1 + \tan^2 \theta) \mathrm{P} \tan \phi = t_0 \tan i,$$

whence

$$\mathrm{P} = \frac{t_0}{\tan \phi} \tan i \cos^2 \theta.$$

Hence the fringes will be most distinct when $\theta = 0$ and

$$\mathrm{P} = \frac{t_0}{\tan \phi} \tan i. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This condition coincides nearly with that found by Feussner.

If the thickness of the film is zero, or if the angle of incidence is zero, the fringes are formed at the surface of the mirrors. If the film is of uniform thickness, the fringes appear at infinity. If at the same time $\phi = 0$ and $t_0 = 0$, or $\phi = 0$ and $i = 0$, the position of the fringes is indeterminate. If i and ϕ have the same sign, the fringes appear in front of the mirrors; if i and ϕ have opposite signs, the fringes appear behind the mirrors.

To find the form of the curves as viewed by the eye at E, call T the distance between the surfaces at E′, the projection of E. From P draw PR parallel to DP′, and RS parallel to CP′, let RS = c, and let SE = D. We have then $t_0 = \mathrm{T} + c \tan \phi$, whence, since $c = \mathrm{D} \tan i$,

$$\Delta = 2 \frac{\mathrm{T} + (\mathrm{D} + \mathrm{P}) \tan \phi \tan i}{\sqrt{1 + \tan^2 i + \tan^2 \theta}}. \quad . \quad . \quad . \quad (5)$$

If, on a plane perpendicular to EE′ at distance D from E, we call x distances parallel to P′C, and y distances parallel to P′D, counted from the projection of E on this plane, then, putting $\tan\phi = K$ and $D + P = S$, we have for the equation to the curves,

$$\Delta = 2\frac{DT + SKx}{\sqrt{D^2 + x^2 + y^2}},$$

or

$$\Delta^2 y^2 = (4S^2K^2 - \Delta^2)x^2 + 8TSKDx + (4T^2 - \Delta^2)D^2. \quad . \quad (6)$$

If, numerically,

$\Delta < 2SK$,	the curve is a		hyperbola.
$\Delta = 2SK$,	,,	,,	parabola.
$\Delta > 2SK$,	,,	,,	ellipse.
$K = 0$,	,,	,,	circle.
$\Delta = 0$,	,,	,,	straight line.

All the deductions from equations (4) and (6) have been approximately verified by experiment.

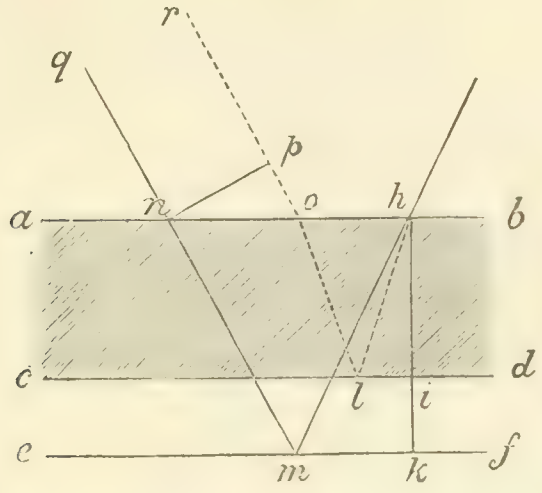
It is to be noticed that in the most important case, and that most likely to occur in practice, namely that of the central fringe in white light, we have $\Delta = 0$, and therefore also $t_0 = 0$; and in this case the central fringe is a straight line formed on the surface of the mirrors. Practically, however, it is impossible to obtain a perfectly straight line; for the surface of the mirrors is never perfect.

It is also to be noticed that the central fringe is black; for one of the pencils has experienced an external, the other an internal reflection from the surface of b (fig. 1). This will not, however, be true unless the plate g (which is employed to compensate the effect of the plate b) is of exactly the same thickness as b and placed parallel with b. When these conditions are not fulfilled, the true result is masked by the effect of "achromatism" investigated by Cornu (*Comptes Rendus*, t. xciii. Nov. 21, 1881). This remark leads naturally to the investigation of the effect of a plate of glass with plane parallel surfaces, interposed in the path of one of the pencils.

The effect is independent of the position of the glass plate, provided its surface is kept parallel with the corresponding mirror. Suppose, therefore, that it is in contact with the latter, and let cd (fig. 4) represent the common surface. Let $t = hi$ = thickness of the glass, i = angle of incidence, r = angle of refraction, n = index of refraction, λ = wave-length of light. Let ef represent the image of the other mirror, and put $n_0 = \frac{hk}{t}$.

It can readily be demonstrated that the path of the rays in the instrument is equivalent to that given in the figure, where

Fig. 4.



one of the rays follows the path $q\,n\,m\,h$, and the other the path $r\,o\,l\,h$. Suppose the mirrors $c\,d$ and $e\,f$ parallel. Then, as has been previously shown, the curves of interference are concentric circles formed at an infinite distance. Therefore the rays $q\,n$, $r\,o$, whose path is to be traced, are parallel, and from the point h they coincide. Their difference of path is

$$2nm - 2hl - op\,;$$

and their difference of phase is

$$\phi = \frac{2nm}{\lambda} - \frac{2hl}{\lambda} - \frac{op}{\lambda}$$

$$= \frac{2n_0 \cdot t}{\lambda \cos i} - \frac{2n \cdot t}{\lambda \cos r} - \frac{2t}{\lambda}(n_0 \tan i - \tan r) \sin i\,;$$

whence

$$\phi = \frac{2t}{\lambda}[n_0 \cos i - n \cos r]. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Let it be proposed to find the value of n_0 which renders any particular ring achromatic. The condition of achromatism, according to Cornu, is

$$\frac{d\phi}{d\lambda} = 0,$$

which gives

$$\phi + 2t\left(\cos r \frac{dn}{d\lambda} - n \sin r \frac{dr}{dn} \cdot \frac{dn}{d\lambda}\right) = 0.$$

We have

$$n = \frac{\sin i}{\sin r},$$

whence

$$\frac{dr}{dn} = -\frac{\sin^2 r}{\sin i \cos r},$$

whence

$$\phi + \frac{2t}{\cos r} \cdot \frac{dn}{d\lambda} = 0.$$

By Cauchy's formula we have

$$n = \alpha_1 + \frac{\alpha_2}{\lambda^2},$$

whence

$$\frac{dn}{d\lambda} = -\frac{2\alpha_2}{\lambda^3}.$$

Substituting, we have

$$n_0 \cos i - n \cos r = \frac{2}{\lambda^2} \frac{\alpha_2}{\cos r} = \frac{\frac{2\alpha_2}{\lambda^2} + n \cos^2 r}{\cos i \cos r};$$

or, finally,

$$n_0 = \frac{2(n - \alpha_1) + n \cos^2 r}{\cos i \cos r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If the angle i is small, the value of n_0 will vary very little with i; consequently there will be a large number of circles, all nearly achromatized. Under favourable circumstances as many as one hundred rings have been counted, using an ordinary lamp as source of light.

The difference of path of the two pencils which produce these rings in white light may exceed a thousand wave-lengths.

XXIX. *On the Refractive Index and Specific Inductive Capacity of Transparent Insulating Media.* *By* J. Hopkinson, *D.Sc., F.R.S.**

ONE of the deductions from Maxwell's electromagnetic theory of light is, that the specific inductive capacity of a medium is equal to the square of its refractive index. Another deduction is, that a body which is opaque to light, or, more generally, to radiant energy, should be a conductor of electricity. The first deduction appeared so clear an issue that many experimenters have put it to the test. The results

* Communicated by the Physical Society, having been read at the Meeting on February 25, 1882.

may be briefly summarized thus:—Some bodies (such, for example, as hydrocarbon oils* and paraffin-wax) agree with Maxwell's law so well that the coincidence cannot be attributed to chance, but certainly points to an element of truth in the theory: on the other hand, some bodies, such as glass† of various kinds, fluor-spar‡, Iceland spar‡, and the animal and vegetable oils§, have specific inductive capacities much greater than is indicated by their refractive indices.

How do these latter results really bear on Maxwell's theory? The facts are these. Taking the case of one substance as typical, the refractive indices of light flint-glass are very accurately known, the period of disturbance ranging from $\frac{1}{4{\cdot}0\times10^{14}}$ second to $\frac{1}{7{\cdot}6\times10^{14}}$ second; the specific inductive capacity is known to be about 6·7, the time of electrical disturbance being from $\frac{1}{170}$ second to a few seconds. If from the observed refractive indices we deduce by a formula of extrapolation the refractive index for very long waves, we find that its square is about one third of 6·7. There can be no question about the accuracy of the observed refractive indices; and I have myself no doubt about the specific inductive capacity; but formulæ of extrapolation are always dangerous when used far from the actual observations. If Maxwell's theory is true, light flint-glass should be perfectly transparent to radiations having a wave-period of, let us say, $\frac{1}{170}$ second; because this glass is sensibly a perfect electrical insulator, its refractive index for such waves should be about 2·6. Are there any facts to induce us to think such a thing possible? It is well known that in some cases strong selective absorption of light in the visible spectrum causes what is known as anomalous dispersion; that is to say, the body which presents such selective absorption of certain rays has a refractive index abnormally low for waves a little shorter than those absorbed, and an index abnormally high for waves a little longer than those absorbed‖.

Light flint-glass is very transparent through the whole visible spectrum, but it is by no means transparent in the infra-red. If the absorption in the infra-red causes in light flint-glass anomalous dispersion, we should find a diminished refrac-

* Silow, Pogg. *Ann.* 1875, p. 382; 1876, p. 306. Hopkinson, Phil. Trans. 1881, part i. p. 371.

† 'Cavendish Researches,' edited by Clark Maxwell; Schiller, Pogg. *Ann.* 1874, p. 535; Wullner, *Sitz. k. bayer. Akad.* 1877, p. 1; Hopkinson, Phil. Trans. 1878, part i., 1881, part ii.

‡ Romich and Nowak, *Wiener Sitz.* Bd. lxx. part 2, p. 380.

§ Hopkinson, Phil. Trans. 1881, part ii.

‖ 'Theory of Sound,' by Lord Rayleigh, vol. i. p. 125.

tive index in the red. We may say that we have a hint of this; for if we represent the refractive indices by the ordinates of a curve in which the squares of the reciprocals of the wave-lengths are abscissæ, this curve presents a point of inflection*. In the part corresponding to short waves it is concave upwards; in the part corresponding to long waves it is concave downwards: the curvature, however, is very slight. Does it not seem possible, looking at the matter from the purely optical point of view, that if we could examine the spectrum below the absorption in the infra-red, we should find the effect of anomalous dispersion, and that the refractive index of such long waves might even be so high as 2·6? To test this experimentally in a conclusive manner would probably not be easy. Perhaps the best chance of finding how these long waves are refracted would be to experiment on the rays from a thermopile to a freezing-mixture. Without an actual measurement of a refractive index below all strong absorption, it cannot be said that experiment is in contradiction to the Electromagnetic Theory of Light; for a strong absorption introduces a discontinuity into the spectrum which forbids us from using results on one side of that discontinuity to infer what they would be on the other side.

XXX. *Water-pipes that do not burst with Frost.* *By* C. Vernon Boys, *Demonstrator of Physics, Normal School of Science, South Kensington*†.

DURING the severe weather of last winter, Mr. L. S. Powell proposed to me a scheme for preventing the possibility of water-pipes bursting through frost; and I have since learnt that Mr. Mangnall, of Manchester, independently hit upon the same idea. As far as I can remember, there were some letters in the 'Times' describing the use of india-rubber pipes containing air inserted in the service-pipes. This would obviously prevent pipes from bursting; for the pressure is of a nature that is relieved by a comparatively small expansion; and this the india-rubber tube allows to take place in the surrounding water when it collapses. There is, however, one serious objection to this, which is the possibility of the detachment of one end of the flexible tube, in which case a rush of water might cause it to accumulate in one place and obstruct the passage.

* Proceedings of Royal Society, 1877.

† Communicated by the Physical Society, having been read at the Meeting on November 12, 1881.

Mr. Powell's plan is to make the piping elliptical—either before it is laid, in which case it may be made of that form originally or by passing round pipe through rollers, or afterwards, when suitable hand-squeezers will effect the result without the necessity of removal. As will afterwards be seen, it is not necessary that the pipe should be elliptical throughout; if left round under staples and in other inaccessible places, the adjacent elliptical portions ensure safety. The principle, of course, is obvious. As is well known, the Bourdon pressure-gauge depends on the fact that the area of an elliptical pipe is less than that of a circle of equal perimeter: therefore during increased pressure its section becomes more circular; increased circularity of section produces diminished curvature in the form of the pipe; and so the movements of the end of the pipe are used to measure pressure. Thin brass is used for this purpose, and is so elastic that it returns to its original form when the pressure is removed; and so an indefinite number of increments and decrements of pressure may be measured by it. The case of the elliptical water-piping is different. Here there is not a definite pressure to withstand, but a definite increase of volume; and, moreover, if this increase of volume is resisted, a practically infinite force arises to break down the resistance. The question then is, how best to allow of this increase of volume. The method of the indiarubber pipes I have already mentioned. The other plan is to make them of an elliptical or other-than-round section. There is, however, far more in this suggestion than one would be likely to see at first. Consider the case of a round pipe in which water is beginning to freeze. Increase of volume must take place somewhere. No pipe can be absolutely uniform in strength everywhere. So wherever a place occurs which happens to be a little weaker than the rest, no matter how little, that place will stretch, and necessarily stretch more than other places. But when a round pipe stretches, two things happen—its diameter increases and its thickness decreases; therefore, as the strength of a tube to resist bursting is inversely as the diameter and directly as the thickness, each of these effects makes the stretched portion still weaker than the neighbouring parts; therefore a round pipe under the action of frost is in a state of unstable equilibrium; the consequence is, knobs form on the pipe, and ultimately burst.

Now consider the case of an elliptical pipe, of such strength, of course, as to stand the ordinary water-pressure. As before, suppose some portions are weaker than others. When expansion takes place they will suffer most, and will begin to give way. But an elliptical pipe on giving becomes more circular,

and this the more easily as its section departs more from the circle; so the very fact of its becoming more circular makes it less ready to change its form. In a very little while, therefore, though originally weaker, it will become as strong as neighbouring portions: therefore an elliptical pipe under the action of frost is in a state of stable equilibrium, and, instead of giving way in the bulbous manner of a circular pipe, it uniformly becomes more circular. Now the expansion of water in becoming ice is known; and therefore it is easy to calculate by compound interest how many *complete* freezings (that is, freezings from one end of the pipe to the other) any given section of piping will stand before it becomes round. Of course, in practice, the whole length of a pipe does not get frozen; yet if it were originally all of it elliptical, the unfrozen portions would be effective in preventing the more exposed parts from bursting; because as soon as the exposed portions have become rounder than the rest, the latter, and not the former, will yield. If two places in the pipe become completely frozen through and then the intermediate portion freezes, it is true that parts beyond the frozen plug will have no effect.

Mr. Powell and I tried a series of experiments on the subject to see if, in practice, the pipes behaved as we expected. We obtained a quantity of $\frac{3}{4}$-inch lead pipe, about $\frac{1}{10}$ inch thick, and some thin composition pipe of the same size. The piping was cut into lengths of about three feet; half of them were squeezed into an approximately elliptical form, and the rest left circular in section. The degree of ellipticity was such that the major axis was a little more than twice the minor axis. One end of each pipe was squeezed together and soldered. Into the other ends brass plugs cut with a sharp thread were screwed while hot, having been previously smeared with a cement of rosin, beeswax, and red-ochre. After these plugs were inserted and while still hot, the lead was, as an additional precaution, squeezed over a narrower portion of the plug above the screw-thread. In each plug a hole had been drilled and tapped. Through these holes the pipes were filled with water; and then iron screws, with washers of leather boiled in beeswax and tallow, were used to make a tight joint. The pipes were then all laid together in a long box, and surrounded with a freezing-mixture. When a short test-pipe of the same diameter showed that the water was completely frozen, the pipes were removed and thawed. The round composition pipe was burst. The round lead pipe was swollen in an irregular manner. The elliptical piping had become slightly rounder, but was perfectly uniform in shape from end to end, which

was not the case when it was put in the freezing-mixture. The most noticeable thing, however, was the fact that all the unburst pipes had become good water-hammers, and this showed that leakage could not have occurred. The screws were removed, the pipes filled, again screwed up and refrozen, and this was repeated till all were burst. The round lead and the thin elliptical composition pipe burst at the third freezing, and the elliptical lead pipe at the sixth. Judging from the fact that it required three freezings to burst the round pipe, one might be led to suppose that a round pipe would last equally long under ordinary conditions, which is certainly not the case. The reason is that, under ordinary conditions, a greater length freezes at a time and more slowly; and a slight inequality arising, the expansion from a greater volume of water is concentrated on the weaker places, which therefore give way during the second, if not the first, freezing of the water.

It might be thought that, as the outer layers of ice are below the freezing-point when the pipe is being cooled, they would not act as a plastic body and accommodate themselves to the changing form of the pipe; but no doubt can remain as to their behaviour in this respect when the pipe is cooled in air; for in the freezing-mixture, where the rate of cooling must be much more rapid, such accommodation takes place perfectly, even thin composition pipes changing their form and becoming round. The apparent plasticity of the ice may depend on fracture and regelation; for if the outer layers are below the freezing-point, and a bursting-pressure is brought to bear on the compound pipe formed of lead and ice, it might yield, the lead bending and the ice cracking, and so allowing the water to penetrate the cracks and freeze in them. Whether this action takes place or not does not much matter: the result, as in the somewhat different case of glacier-motion, is much the same.

If the pipe is made of such a form that it will not become round till it has been completely frozen, say, three times, it will take a great many frosts to burst it, as those parts that do not freeze easily will protect the more exposed portions; so absolute security may be relied upon till ordinary round pipes have burst once or twice; and then the now nearly round ones may be squeezed back to their original form. The choice, then, is between two evils; either burst pipes, with the usual damage and cost of repair, or the trouble of inspection every second or third time that the neighbours find that the "thaw" has burst theirs.

We thought it possible that iron might be sufficiently elastic

to return to its original form; and so we froze water in two $\frac{3}{4}$-inch iron gas-pipes, one round and one which had been flattened when red-hot. The round pipe burst the first time; the flat one did return slightly when thawed, but not enough to prevent its bursting during the second operation.

No doubt most people will not consider this proposal of Mr. Powell's a satisfactory cure for burst pipes; they would like something which could be fixed in their houses and which would be always safe without further attention. But till such a discovery is made, I think elliptical pipes give the best solution of a problem which has troubled every householder.

As the subject of this paper is of physical as well as general interest, I hope that it may be considered not unworthy of the attention of the Physical Society.

XXXI. *Electro-optic Experiments on various Liquids.* By John Kerr, *LL.D.*, *Free Church Training College, Glasgow.*

[Concluded from p. 169.]

Sulphides, $(C_nH_{2n+1})_2S$.

13. THESE are purely negative, and rise in power as n increases; they are weaker than the corresponding oxides, but much stronger than the hydroxides.

Ethyl sulphide, not a clean liquid, required the jar and 10 to 20 turns of the plate. The effect was purely negative, strengthened by compression parallel to lines of force, and extinguished by tension.

Butyl sulphide, tried as a nonconductor, gave a very faint effect, which was barely characterizable as negative. Tried then as a conductor, with increasing charges of the jar, it gave a set of brilliant effects, always purely negative.

Amyl sulphide, a feeble photogyre, hardly separating the red and blue at extinction, was rather stronger than the last. It acted well as a nonconductor, giving a clear effect which was purely negative. As a conductor, with increasing charges, it gave increasingly brilliant effects, always negative by both tests.

Ethyl disulphide was also examined, and was found to be purely negative, like the protosulphide, but much stronger. As a nonconductor it gave a pure and moderately strong effect: and as a conductor, with increasing charges of the jar, it gave a fine series of restorations, all purely negative, extinguished perfectly by tension parallel to lines of force.

Mercaptans, $C_nH_{2n+1}HS$.

14. These are purely positive, and rise in power as n increases; they are stronger than the corresponding fatty acids.

Ethyl hydrosulphide acted very well in the plate cell as a conductor, without the jar. The effect from extinction was not very faint, and was slow enough to be characterized as purely positive.

Butyl hydrosulphide is distinctly stronger. As a nonconductor, it gave a faint but clear effect, which was purely positive; and as a conductor, with increasing charges of the jar, it gave a set of increasingly brilliant effects, which were all strengthened by tension parallel to lines of force, and all weakened to perfect extinction by the proper compression.

Amyl hydrosulphide, not a clean sample, acted very like the last, but was rather stronger. As a nonconductor (spark $\frac{1}{4}$ inch) it gave a good though faint effect, which was purely positive; and as a conductor, with moderate charges of the jar, it gave an intense restoration, which was always extinguished perfectly by the proper compression parallel to lines of force.

Formates, $CH(C_nH_{2n+1})O_2$.

15. These are active only as conductors, and certainly positive; but the effects are faint and impure, the flame restored in the polariscope being distorted and streaky. This form of disturbance was noticed already, though in a milder form, in the allyl and ethylene alcohols.

Methyl formate, with moderate charges of the jar, gave an effect which was faint and not at all pure, but certainly positive, strengthened by tension parallel to lines of force, and not sensibly affected by compression.

Ethyl formate, with good charges of the jar, acted rather better. The effect was strengthened by tension parallel to lines of force, and much weakened by compression.

Amyl formate acted very like the other two. With moderate charges, the effect was pretty strong and distinctly positive, strengthened by tension, and either weakened or not affected by compression. When the discharges were intense, the disturbance masked the regular effect completely.

Acetates, $C_2H_3(C_nH_{2n+1})O_2$.

16. These are distinctly positive, and otherwise like the formates; but the effects are purer.

Methyl acetate required the jar. With moderate charges the effect was very abrupt but certainly positive, strengthened

by tension, and not affected by compression. The Ruhmkorff-discharges gave afterwards an effect which was more purely positive, weakened regularly by compression ; but the liquid was now very much disturbed, and there was a large heat-effect after a few discharges.

Ethyl acetate acted hardly so well as the last. With moderate charges of the jar, there was a large deformation of the flame, and the regular effect was very faint, not seizable indeed without the hand compensator ; but it was clearly positive, strengthened by tension, and darkened by compression.

Butyl acetate acted much better. As a conductor, without the jar, it gave a clear effect which was purely positive by both tests ; and with these feeble discharges the disturbance of the flame was hardly noticed.

Amyl acetate acted still better, but not without the jar. With moderate charges it gave a good effect, which was purely positive, strengthened by tension, and extinguished by compression. With strong discharges the effect was still very fine, and purely positive by both tests, the deformation of the flame being hardly sensible.

Butyrates, $C_4H_7(C_nH_{2n+1})O_2$.

17. The regular effects in these compounds are purely positive and undisturbed, much stronger and finer than in the acetates.

Methyl butyrate acted well as a conductor, with or without the jar, giving a series of fine effects with increasing charges, and without any apparent disturbance. The effect, weak or strong, was purely positive, extinguished always by the proper compression parallel to lines of force.

Ethyl butyrate, an impure sample, was hardly so strong as the last, requiring the jar and five or more turns of the plate. With good charges the effect was strong, and purely positive by both tests.

Ethyl isobutyrate was much stronger, acting well as a non-conductor, and reminding me of the weaker hydrocarbons, amylene &c. With strong charges of the jar it gave a strong, slow, and very fine effect, purely positive by both tests, and without sensible disturbance.

Isobutyl isobutyrate gave a fine and purely positive effect as a nonconductor. There was no sensible disturbance even when the electric force was strongest. The action improved evidently for a time as the experiment proceeded, and appeared to be at last as strong as that of benzol.

Valerates, $C_5H_9(C_nH_{2n+1})O_2$.

18. These are clearly positive, acting better than the acetates, but not so well as the butyrates.

Methyl valerate, with strong charges of the jar, gave a faint effect which was distinctly positive, strengthened by tension, and weakened by compression. The effect was very impure, the flame being much deformed.

Ethyl valerate was much stronger, and its action was almost perfectly pure. As a nonconductor, it gave a clear, but very faint, restoration from extinction. As a conductor, with increasing charges, it gave a series of fine effects, all purely positive, strengthened by tension, and extinguished by compression.

Amyl valerate, a much stronger photogyre than either of the other two, acted only as a conductor, and required the jar. The regular effect, not strong in any case, was always purely positive by both tests.

Benzoates, $C_7H_5(C_nH_{2n+1})O_2$.

19. These are purely positive, and about as strong as the butyrates.

Methyl benzoate acted clearly as a conductor ; but the effect without the jar was too abrupt to be characterized. With small charges of the jar the effect was positive by one test, strengthened by tension, and either weakened or not sensibly affected by compression.

Ethyl benzoate, as a nonconductor, gave a faint but very good restoration, which was purely positive. Tried then as a conductor, with and without the jar, it gave an intense and very slow effect, always purely positive by both tests.

Amyl benzoate, not a clean sample, acted only as a conductor. With moderate charges of the jar, it gave an intense and slow effect which was always purely positive, the extinction-bands being brought in very finely by compression parallel to lines of force.

Nitrates, $C_nH_{2n+1}NO_3$.

20. These are weak insulators in comparison with most of the preceding ethers : they are clearly but not very purely positive.

Ethyl nitrate as a conductor, without the jar, gave a restoration which was distinct, but very faint and abrupt. With moderate charges of the jar, the effect was seen to be positive, strengthened by tension, but not affected by compression. With strong charges the effect was still very abrupt, neither pure nor very strong, but certainly positive.

Butyl nitrate is stronger, giving a sure trace of effect as a nonconductor. Tried as a conductor, with moderate charges of the jar, it gave a strong restoration which was certainly positive, strengthened by tension, but not affected by compression. With very strong charges the effect was brilliant, but still too abrupt, being absolutely unaffected by compression parallel to lines of force; but the contrast between the effects of tension and compression was always perfectly clear.

Amyl nitrate, a feeble photogyre, is very like the last, certainly, but not quite purely, positive. With strongest charges of the jar, the effect was still unmanageably abrupt for the second test; and the liquid was sensibly disturbed.

Nitrites, $C_nH_{2n+1}NO_2$.

21. *Ethyl nitrite* was in solution (10 p. c.), and, when tried with strong charges of the jar, gave a very faint effect which was apparently positive.

Amyl nitrite acted well with moderate charges of the jar. Although the liquid was disturbed sensibly by discharge, the regular effect was seen to be certainly positive, strengthened by tension, and either weakened or not affected by compression.

Other compounds of C, H, O.

22. *Aldehyd*, C_2H_4O, is negative, and hardly so strong as ethyl alcohol. With Ruhmkorff-discharges, and with precautions against permanent heat-effects, it gave a good restoration, regular though very faint, which was clearly strengthened by compression but not by tension.

Acetone, C_3H_6O, is stronger than the last, clearly positive, but a very feeble insulator. With good charges of the jar it gave a clear effect, not characterizable. With separate discharges of the coil, it gave a fine but still faint restoration from extinction, which was strengthened by tension, but not affected by compression.

Valeral, $C_5H_{10}O$, is stronger than the last, and clearly positive. With moderate charges of the jar it gave a good but abrupt effect, strengthened by tension, but not affected by compression. With the coil, the effect was very strong, still not affected by compression; but the contrast between tension and compression was regular and perfectly clear, proving the liquid to be purely positive, but a very weak insulator.

Œnanthol, $C_7H_{14}O$, is still stronger, and purely positive. As a conductor, without the jar, it gave a clear though faint restoration. With moderate charges of the jar, the effect was good though still faint, strengthened by tension, and extinguished by compression. With strong discharges, the effect

was more intense but not so pure, the liquid being much disturbed.

Benzoyl hydride, or oil of bitter almonds ($C_7 H_5 O, H$), is positive, and a very weak insulator. Without the jar, it gave a clear restoration, too abrupt for the hand-compensator. With moderate charges of the jar, the effect was still extremely quick but certainly positive, strengthened by tension, and not affected by compression.

Ethylene monacetate, $C_4H_8O_3$, ranks with the other acetates and the formates as very impurely positive. With weak discharges of the jar, the effect was too faint to be characterized, or even regularly seized; and with strong discharges of the coil, the liquid was very much disturbed. With moderate charges the effect was to all appearance positive, but very impure at the best.

Methyl salicylate, or oil of wintergreen, $C_8 H_8 O_3$, is purely positive. As a conductor, without the jar, it gave a sensible but very faint restoration. With strong charges of the jar, the effect was pretty strong and certainly positive, the contrast between tension and compression being clear and regular, though there was no weakening in any case by compression.

The effect was examined afterwards more closely with the Ruhmkorff-discharges, and with a special eye-piece described in one of my former papers—an acute prism of glass, revolving regularly round the line of vision, and exhibiting the progress of the phenomenon in time as an extension in space. Viewed in this way, the effect was strengthened by tension as formerly, and was also extinguished regularly and perfectly by compression. I have already referred to this form of experiment as one that would afford a clear proof of the *purity* of positive or negative character of a dielectric, in cases where the hand-compensator gives a sensible increase of intensity when *conspiring* with the electric strain, but no sensible decrease when *opposing* it.

23. *Beeswax* is purely negative, as are also all the fused fats that have been examined, with the exception of spermaceti, which is strongly positive.

Palm-oil, *cacao-butter*, *lard*, and *tallow* were tried successively in the fusion-cell as examples of the fats. There was generally a little disturbance by movements in the liquid; but the optical effects were distinctly and regularly negative, strengthened by compression, and neutralized by tension.

Spermaceti was then tried in the same way, and was found to be purely positive, like sperm-oil among the fixed oils; but it was much stronger than was expected, stronger indeed than any of the negative fats. As a nonconductor, and with a half

or quarter turn of the plate, it gave a strong and purely positive effect from extinction, without sensible disturbance of any kind.

A thing noticed in most of the fused fats, and strikingly apparent in the case of spermaceti, was the sluggishness of rise and fall of the electro-optic action. When strong electric force was applied, the optical effect rose gradually to its full intensity, through 1 or nearly 2 seconds; and again, when the apparatus was discharged, the light fell to sensible extinction still more slowly, through 20 or more seconds. This was evidently a thing of the same kind as the gradual rise and fall of effect which I observed long ago in glass; but the electric stress in that case was much more intense, and the duration of the phenomenon much greater, extending even to 20 or 30 minutes.

Beeswax, purified but unbleached, was tried in the same way as the fats, and was found to be purely negative, and almost as strong as spermaceti.

White wax, a colourless and very clear plate in the fusion-cell, gave a finer and stronger effect, still purely negative.

Other Compounds of C, H, N.

24. *Aniline*, C_6H_7N, is purely negative. Two samples were examined. One of them, tried with plate machine and strongest charges of the jar, gave a faint effect which was purely negative, strengthened by compression, and extinguished by tension. The other sample, which was much deeper in colour, gave no trace of effect with the jar; but with the Ruhmkorff's coil it gave intense restorations, which were purely negative by both tests.

Allylamine, $C_3H_5NH_2$, is very feebly negative. With the Ruhmkorff-discharges, the restorations from extinction were perfectly clear and regular, but very difficult to characterize. Upon the whole, the effect appeared to be certainly negative: but it was very faint, and somewhat impure at the best.

Capronitrile, $C_6H_{11}N$, is feebly but clearly negative. Was tried in the same way as allylamine, and acted rather better, the restorations from extinction being very pure and regular, but still rather difficult to characterize. On the whole, the effect was clearly negative, strengthened by compression, and not affected by tension.

Benzonitrile, C_6H_5CN, is purely and strongly positive. Tried as a conductor, without the jar, it gave an intense and very fine effect, which was strengthened by tension parallel to lines of force, and extinguished perfectly by compression.

This dielectric reminded me of nitrobenzol; but the action was much less abrupt, and therefore more easily characterized.

Azobenzol, $C_{12} H_{0} N_{12}$, fuses at 65° C. into a clean and transparent liquid of a deep red colour. Tried in the fusion-cell as a conductor, with increasing charges of the jar, it gave a series of fine and increasingly brilliant effects, all purely positive.

Diphenylamine, $C_{12} H N_{11}$, fuses at 54° C. into a transparent, colourless, and very volatile oil. Tried in the fusion-cell as a conductor, with moderate charges of the jar, it gave a pretty strong effect which was purely positive. With strong charges, the regular effect was masked in some degree by movements in the liquid.

Dimethylaniline, $C_8 H_{11} N$, is purely positive. Tried in the plate cell as a conductor, without the jar, it gave a good restoration, which was too quick to be characterized. With small charges of the jar, the effect was very strong and purely positive, strengthened by tension, and extinguished by compression.

Conine gave no trace of effect, even with the strongest discharges of the coil. My sample was almost opaque, and certainly far from pure.

Compounds containing Cl *or* Br.

25. *Benzyl chloride*, $C_7 H_7 Cl$, is clearly positive, but acts only as a conductor. Tried with the jar, it gave no effect with 5 turns, a good effect with 10 or more turns; strengthened always by tension, and sensibly weakened by compression. My sample was not very clean.

Chlorobenzol, $C_7 H_6 Cl_2$, is purely positive. Acted well, though feebly, as a nonconductor, giving an effect which was strengthened by tension and extinguished by compression.

Ethylene chloride, $C_2 H_4 Cl_2$, is uncertain. It did give a faint and apparently regular effect with strong discharges; but the disturbance (as in the formic ethers) was excessive, and made the determination of the sign impossible. The results obtained with the next liquid make it probable, if not certain, that the failure in the present case was due to impurity of the sample.

Ethylene bromide, $C_2 H_4 Br_2$, is purely and strongly positive. Tried as a nonconductor (spark $\frac{1}{8}$ inch), it gave a fine steady restoration, which was strengthened by tension, and extinguished perfectly by compression. As a conductor, with increasing charges of the jar, it gave a series of fine effects, which rose through a large range of intensity, always purely positive.

Chloroform, $C H Cl_3$, is purely negative and pretty strong. As a nonconductor, it gave a fine and purely negative restoration from extinction. As a conductor, with increasing charges, it gave a set of increasingly brilliant effects, always purely negative, extinguished perfectly by tension parallel to lines of force. With strong discharges, there was a fine exhibition of extinction-bands, narrow and strong, brought in by tension parallel to lines of force.

Bromoform, as was expected, is also purely negative, but weaker than chloroform. As a conductor, with the jar, it gave a clear effect with 5 turns, strong with 10 or more turns, and always purely negative.

Acetyl chloride, $C_2 H_3 OCl$, is purely negative. As a conductor, with strong charges of the jar, it gave an effect from extinction which was merely strong enough to be characterized as certainly negative, being strengthened by compression, and weakened by tension. With the coil afterwards, there was a certain disturbance produced in the liquid by each discharge ; but the regular effect was clearer and stronger than formerly, and always negative by both tests.

Chloral, $C_2 Cl_3 OH$, is positive. As a conductor, with the jar, it gave a good restoration with 10 or more turns of the plate. The effect was regularly strengthened by tension, but not sensibly affected by compression.

Chlorpicrin, $CCl_3 NO_2$, is purely negative. Tried as the last, it gave a clear effect with 5 turns, strong with 10 or more turns ; and the restored light was always strengthened by compression, and always extinguished by the proper tension.

26. *Carbon tetrachloride* is purely positive, like the dichloride (formerly examined), but much weaker. Tested for insulation, it gave a good spark ($\frac{1}{4}$ inch) from the prime conductor. The optical effect was not proportionably strong, not visible indeed from pure extinction ; but with the assistance of the hand-compensator it was brought out very clearly, and was found to be purely positive. There was no advantage obtained by trial of the liquid as a conductor, with or without the jar.

Sulphur chloride is purely positive. As a conductor, with strong changes of the jar, it gave a faint but good effect, which was strengthened by tension, and clearly weakened by compression.

Phosphorus trichloride is purely negative, and stronger than the last. As a conductor, with small charges of the jar, it gave a moderately strong restoration from extinction ; and the effect was regularly strengthened by compression, and extinguished by tension.

Other Compounds and Solutions.

27. *Amyl sulphocyanide*, $C_6H_{11}NS$, is positive. As a conductor, without the jar, it gave distinct restorations, too faint and quick to be characterized. With strong charges of the jar, the effect was strong, and always distinctly positive though very abrupt, strengthened by tension, and not affected by compression.

Allyl sulphocyanide, or oil of mustard-seed, C_3H_5CNS, is positive. With strong charges of the jar it gave a clear effect, which was too abrupt for the hand-compensator, but apparently positive. With the Ruhmkorff-discharges the effect was stronger and still very abrupt; but it was certainly positive, strengthened by tension, and not affected by compression. With very strong discharges the liquid was considerably disturbed.

Thialdine, $C_6H_{13}NS_2$, fuses into a transparent plate at 43° C. In the fusion-cell, with moderate charges of the jar, it gave a clear effect, pretty strong but very abrupt, certainly negative, being regularly strengthened by compression, and not affected by tension.

Oil of sage is purely positive. The initial extinction was imperfect, the liquid being a weak photogyre. With small charges of the jar, there was a good and purely positive restoration from extinction.

28. Several aqueous solutions were tried in the plate cell, but with no results worth mentioning, except in the two cases of Cl and SO_2.

Chlorine in water is feebly but purely negative. The water was distilled specially for the experiment, saturated with the gas, and then examined immediately. With strong discharges of the coil, there were distinct restorations from extinction, very faint but perfectly regular, clearly strengthened by compression, and clearly weakened by tension. The effects were to me barely visible; but they were as distinct as any very faint effects could be; they were also recovered regularly on repetition of the experiment, and with two successive charges of the liquid.

Bromine in water had been examined some time before, and had been entered in my note-book as very apparently, but very feebly, negative.

SO_2 in water is also purely negative. The experiment was made immediately after a similar one with water alone; and the contrast between the two was perfect, water being regularly and purely positive, and SO_2 in water as regularly and purely negative, but rather feebler. The solution of sulphurous

acid was not noticeably stronger or weaker electro-optically than the chlorine solution.

Summary.

29. The principal results of the experiments are briefly summed up in the following statements, the numbers of the corresponding articles being attached in each case for reference.

The elementary bodies bromine, phosphorus, sulphur, all in the liquid state, are purely and strongly positive (3).

The hydrocarbons are nonconductors and purely positive, without known exception. The induction effected up to this time is a mere beginning in such an immense field; but it includes examples of the alcohol radicals or their hydrides, of the benzol series, of the olefines, the paraffins, and the terebenes; also cinnamol and naphthalin. Among the hydrocarbons, great density is generally accompanied by high electro-optic power (4).

The common alcohols are negative as a class. From the higher members downwards, the negative electro-optic power diminishes regularly, till it passes, in the last step of the series (ethyl to methyl), from feeble negative to feeble positive. Distilled water also is distinctly positive (5).

Of the series of fatty acids, those liquid at ordinary temperatures are in constant opposition of sign to the corresponding alcohols; they are also very much stronger. Of those that are solid at ordinary temperatures, two have been examined in a state of fusion, and are found, on the contrary, to be strongly negative (6, 7).

Of other alcohols and acids, the allyl, benzyl, cinnyl alcohols are negative; the diatomic glycol and the triatomic glycerin are feebly and impurely negative; phenol is distinctly positive, though in its chemical relations rather an alcohol than an acid; the oleic and lactic acids are both positive, the former strong, the latter very feeble (8).

The oxides of ethyl and amyl are purely negative, and stronger than the corresponding alcohols. Amyl oxide is a moderately good insulator, and one of the best negative dielectrics yet known (9).

The haloid ethers are purely positive, and rise in power from one series to another in the order I, Br, Cl; they generally rise also in power from lower members upwards. Some of them show an extraordinary increase of insulating and electro-optic powers as the experiment proceeds. Amyl chloride is one of the best positive dielectrics known: amyl bromide also is very strong (10, 11, 12).

The sulphides of the alcohol radicals are purely negative and weaker than the corresponding oxides, but much stronger than the alcohols. From lower members upwards they rise in electro-optic power (13).

The hydrosulphides are purely positive, and stronger than the corresponding fatty acids. From lower members upwards they rise in power (14).

Of the compound ethers, all that have been examined are clearly positive; and they include examples of the formates, acetates, butyrates, valerates, benzoates, nitrates, and nitrites. The optical effects are pure in the butyrates, but more or less disturbed in the others, impure in the acetates, very impure in the formates (15–21).

The following table contains a few additional bodies which are found to be electro-optically active. The optical effects are neither impure nor very weak, except in the cases that are so marked. The positive and the negative dielectrics appear always in the left-hand and right-hand columns respectively.

Compounds of C, H, O (22, 23).

Acetone (wk).	Aldehyde (wk).
Valeral.	Palm-oil.
Œnanthol.	Cacao butter.
Benzoyl hydride.	Lard.
Methyl salicylate	Tallow.
Ethene monacetate (imp.).	Beeswax.
Spermaceti.	

Compounds of C, H, N (24).

Benzonitrile.	Aniline.
Azobenzol.	Capronitrile (wk).
Diphenylamine.	Allylamine (wk).
Dimethylaniline.	

Compounds containing Cl *or* Br (25, 26).

Chlorobenzol.	Chloroform.
Benzyl chloride.	Bromoform.
Ethylene dibromide.	Chlorpicrin.
Chloral.	Acetyl chloride (wk).
Carbon tetrachloride.	Phosphorus trichloride.
Sulphur chloride.	

Other Compounds and Solutions (27, 28).

Amyl sulphocyanide.	Thialdine.
Allyl sulphocyanide (wk).	Cl in water (wk).
Oil of sage.	SO_2 in water (wk).

Concluding Remarks.

30. *Bodies under electric stress are birefringent, with reference to line of electric force as axis.*—This property has now been exemplified in such a number and such a variety of dielectrics, that it ought to be admitted as a general property of matter. There do occur cases (in the formic ethers &c.) in which the purity of electro-optic double refraction is lost: but these are no true exceptions to the general statement; they are merely cases of disturbance, cases in which the regular effect is masked, wholly or partly, by the presence of other effects. Gross movements and heat appear to be the principal causes of disturbance.

Dielectrics are divisible into two classes, the positive and the negative, corresponding to the two so-named classes of uniaxal crystals.—This also has been abundantly confirmed by experiment, not as a rough adaptation of the notions and terms of Optics to our present subject, but as an exact physical truth. Except in cases of impure action or of mixed optical effects, cases which are comparatively rare in fact, the experimental contrast between the two kinds of dielectric is always as pure to sense, and under any tests yet applied, as is the contrast between quartz and Iceland spar. The two finest examples of the positive class are carbon disulphide and amyl chloride; and these are far superior in power to any negative dielectric yet discovered. As interesting examples of the negative class, I select the following four, arranging them (according to my samples) in the order of descending powers:—amyl oxide, chloroform, capryl alcohol, aniline.

Among bodies yet examined, the positive dielectrics are more numerous than the negative.—Of 13 amyl compounds, 10 are purely positive, and 3 purely negative; and similarly in other cases. Br, P, S, the only elements yet examined, are all purely positive.

The electro-optic and chemical characters of compounds are closely connected.—The hydrocarbons are positive, the fixed oils and fats negative, the fatty acids positive, the alcohols negative, &c.

The electro-optic character of a chemical compound is not determined by its empirical formula.—Isomeric bodies are sometimes electro-optically similar, and sometimes not. This is shown in the four following sets of isomers or polymers: propionic acid, methyl acetate, ethyl formate—all positive; butyl alcohol and ethyl oxide, both negative; aldehyde, butyric acid, ethyl acetate—the first negative and the other two positive; allyl alcohol, acetone, caproic acid, methyl valerate—the first negative and the others positive.

Every chemically homologous series of bodies yet examined exhibits a certain constancy of electro-optic character.—Thus, the oxides, hydroxides, and sulphides of the alcohol radicals are negative; while the hydrosulphides, the iodides, bromides, chlorides, butyrates, benzoates, &c. are positive.

Through each of these homologous series, there is generally a progression of electro-optic power, the higher members being the stronger.—The common alcohols and the fatty acids (liquid at ordinary temperatures) afford very good illustrations. The two series of bodies are negative and positive respectively; and in each of the series, from member to higher member, there is a regular increase of absolute electro-optic power. The progression is not maintained in the higher parts of the acid series, the palmitic and stearic acids being (as the other fats are) strongly negative.

This progression, in singular cases, takes the form even of a change of sign.—The only examples yet known are afforded by the two last mentioned series, which begin thus:

$$\left.\begin{matrix}\text{methyl alcohol} + \\ \text{ethyl alcohol} -\end{matrix}\right\} \qquad \left\{\begin{matrix}- \text{ formic acid} \\ + \text{ acetic acid}\end{matrix}\right.$$

This occurrence of contrary changes of sign at corresponding points of the two series is surely a very remarkable fact. The two following inferences are perhaps worthy of notice.

Oxygen is probably a negative dielectric.—Putting $n=0$ in the general formulas of alcohol and acid, we obtain H_2O and O_2 as the starting-points of the two series. Water and oxygen should therefore have the same signs as methyl alcohol and formic acid respectively. As far as water is concerned, the inference is confirmed already by experiment.

Our terms "positive" and "negative," borrowed originally from Physical Optics, appear now to be terms intrinsically appropriate to the subject.—For in the acids we see a regular increase of positive power showing itself at one point of the series as a passage from − to +, and, again, in the alcohols a regular increase of negative power showing itself at one point as a passage from + to −.

31. *Electro-optic double refraction* is brought about by *a special state of the dielectric*, a state of *essentially directional strain*, which is a concomitant and a condition of the maintenance of electric stress. This appears to me to be a principle that must underlie any admissible explanation of the phenomena. The carefully grounded and very valuable conceptions of Faraday and Maxwell point in this one direction; and the simple facts of the case are, of themselves, quite decisive. What clearer manifestations of directional strain could be

desired than those that are presented here? There is, first, the *directional transmission of electric force* from limit to limit of the field; there is, secondly, the *special dioptric action of the force-transmitting medium*, an action precisely similar to that of glass *directionally strained by tensions or pressures*; and there is, finally, the *directional rupture of the dielectric* when the intensity of the transmitted force reaches a critical value characteristic of the medium.

I insist upon the essentially directional character of electric strain, in opposition to a theory of electro-optic action which has been advanced by Professor Quincke*.

Electro-optic double refraction may be attributed to a quasi-crystalline and uniaxal structure, which is maintained by electric stress in the medium. I advanced this view of the phenomena in my first paper on the subject; and I still believe that it contains a germ of the truth. I think indeed that, to any one engaged with the facts of Electro-Optics, there is no theoretical conception that can present itself more naturally than that of an electrically induced structure, a regular arrangment of polarized molecules, roughly illustrated by the common "magnetic curves."

XXXII. *On the Electric Resistance of Carbon under Pressure.* *By* Professor Silvanus P. Thompson, *B.A., D.Sc.*†

§ 1. It has often been stated that the electric resistance of carbon decreases when subjected (1) to an increase of temperature, (2) to a mechanical compression. The first of these statements has been verified by so many authorities that there can hardly be any question of its correctness. The second I believe to be wholly misleading; for some careful experiments that I have lately made lead to the conclusion that the effect of mechanical compression upon the electric resistance of dense carbon is almost, if not quite, *nil*, and that what has been mistaken for an increase in electric conductivity is in reality merely better contact at the points of junction with the circuit.

§ 2. A preliminary experiment to test whether the alleged decrease of resistance by pressure was due to a true increase in specific conductivity or to better end-contact, was made in the following manner:—A thin cylindrical rod of Carré's dense artificial carbon (such as is used in electric lamps) was taken, its length being 72 centimetres. At a point about one

* Phil. Mag. for July 1880, p. 37.

† Communicated by the Physical Society, having been read at the Meeting on February 25, 1882.

fourth of its length from the end a groove was filed round it, and around it was bound tightly the end of a clean thin copper wire. It was placed vertically upon a piece of copper in an upright frame, so that pressure could be applied longitudinally at the top ; a flat piece of copper was placed upon the top of it and pressed lightly upon it. The point at which the thin wire was bound round it was then about 18 centim. from the lower end. The upper and lower portions were then connected up with a Wheatstone's bridge, provided, as in Kirchhoff's pattern, with a wire of German silver stretched over a divided scale, the resistances of the two parts of the wire right and left being (when balance was obtained in the galvanometer) proportional respectively to the resistances of the two portions of the carbon rod. The relative, not the absolute, resistances of the two portions were therefore being measured. Two bichromate cells supplied the requisite current, the galvanometer being a short-coil astatic instrument of simple form.

On trying the resistances, it appeared that the resistance of the longer part, which was uppermost, was somewhat greater in respect to that of the lower and shorter part than would have been expected from their relative lengths ; the ratio of their resistances being 81 : 19, or about 4·25 : 1, whereas their lengths were as 3 : 1 almost exactly. After taking the rod out of its place and putting it back again under light contact at the top as before, the ratio was found to be 82 : 18, or 4·55 : 1. The rod was thus removed and replaced several times; and the ratio of the resistances was found to differ somewhat every time, the figures varying from 4·7 : 1 to 3·92 : 1. Pressure was now applied at the top of the rod, and the ratio of the resistances was again measured. With a load of 5 kilogrammes (as much as it was judged the rod would bear without risking breaking it), the ratio of the resistances was found to be much more constant and much nearer to the ratio of the lengths of the two portions, being 75·3 : 24·7, which is not very different from 3 : 1. It was therefore clear that the previous values had been greatly affected by the differences in contact at the two ends; the lower contact having less resistance than the upper, in consequence of the superincumbent weight of the rod and connexions—about 19 grammes in total.

§ 3. Another rod of Carré's carbon was next examined, and its actual resistance measured in ohms. Its length was 42·6 centim., its diameter 0·48 centim. To prepare it for the experiment, it was electroplated with a thin coating of copper to the length of about 1 centim. at each end, the extremities being afterwards scraped bare of copper so that end-contacts

should be made against the carbon itself. Copper wires were then carefully soldered to the copper coating at about 0·5 centim. from the ends. The object of this arrangement was to render possible a comparison between the resistance of the rod when there was merely end-contact—which might be more or less perfect according to the pressure—and the resistance of the rod as measured when there was a perfect contact through the deposited coatings of copper. The rod was then laid in a horizontal frame, where it reposed on two Y-shaped bearings—one end pressing against a lever-arrangement for the purpose of putting on a measurable amount of pressure, the rod being fixed at the other end by abutting against a brass set-screw. A copper piece was introduced between the lever and the extremity of the rod, in order to provide an end-contact; and arrangements were made whereby the rod could be connected up in a Wheatstone-bridge, the connexion being made at pleasure either through the end-contacts or through the copper-plated junctions.

The resistance of the rod between the copper-plated junctions was then measured, the rod being free at both ends. It was found to be $0{\cdot}56 \pm 0{\cdot}007$ ohm. The end-contacts were then made to touch lightly (the circuit through them remaining open). The resistance through the copper-plated junctions showed no change. Pressure was then applied to the rod longitudinally, and augmented until it began to show lateral distortion, the effective force along the rod being 4150 grms., equivalent (if the area of cross section of the rod be taken as 0·18 square centim.) to 23,055 grms. per square centimetre. Yet, even under this pressure, not the smallest change could be detected in the resistance between the copper-plated junctions. If there was any, it was certainly less than 0·005 ohm, or less than 1 per cent. of the whole resistance.

The circuit was now made through the end-contacts by moving the set-screw until the lightest possible contact was obtained, the connexions through the copper-plated junctions having been thrown out of circuit. The resistance thus determined was 1·1 ohm. Pressure was applied as before. The resistance fell to 0·72 ohm when the pressure of 23 kilogrammes per square centimetre was reached. On releasing the pressure, the resistance again rose until contact became as light as possible. The resistance attained 1·08 ohm, when it rose abruptly to infinity as the set-screw ceased to touch the end of the rod. The battery and galvanometer used throughout were the same as described above.

Nothing could be more significant than these observations. When perfect contact was ensured by electroplating, pressure

produced no effect on the resistance of the carbon rod, or one inappreciably small. When circuit was made by pressing pieces of copper and brass against the rough ends of the carbon rod, contact was only imperfectly obtained, and the resistance varied with the pressure because increased pressure brought about better contact, or contact at a greater number of points.

The bearing of these observations upon the theory of the carbon rheostat, the carbon relay, the carbon transmitting-telephone, and the carbon microphone is obvious.

University College, Bristol,
February 1882.

XXXIII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part V. *By* C. R. Alder Wright, *D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School*.*

On the Relationships between the Electromotive Force of a Daniell Cell and the Chemical Affinities involved in its Action.

102. In accordance with the theorem stated in § 61, the E.M.F. that would be requisite to break up a given electrolyte under given conditions into the "nascent" products of electrolysis would be a constant amount, were it not that the secondary physical and chemical actions of the electrodes, and of dissolved gases, &c. upon the nascent products give rise to the development of an amount of heat, the energy equivalent to which diminishes the work that would otherwise be done by the current whilst effecting electrolysis; so that the E.M.F. corresponding to the net electrolytic work actually done is less than the constant amount that would be requisite in the absence of these interfering circumstances; under certain conditions, the diminution in the work is so great that work is gained instead of spent, when the cell becomes an electromotor. Experiment shows that, *ceteris paribus*, the amount of diminution is less the more rapid the rate of current-flow; so that in a decomposing cell, in which, on the whole, work is spent during the passage of the current in doing electrolysis (the heating effect due, in accordance with Joule's law, to the resistance proper of the cell being left out of consideration), the counter E.M.F. set up (representing the work so spent) is of + sign, and increases in magnitude with the rate of current-flow; whilst in an electromotor, in which, on the whole, work is gained during the passage of the current, the counter E.M.F. set up is of − sign (*i. e.* is a

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direct E.M.F.), and decreases in magnitude with the rate of current-flow.

This decrease in magnitude, although a phenomenon well known under the name of "polarization of the cell," has nevertheless been less thoroughly investigated than is desirable. Thus, for instance, in the case of a given Daniell cell it is unknown to what relative extents the diminution is due to each of three entirely different possible causes, viz.:—first, the formation, in consequence of the electrolytic actions going on, of a stronger zinc-sulphate solution round the zinc plate, and of a weaker copper-sulphate solution round the copper plate, than were there originally; secondly, the more or less incomplete action as regards setting up E.M.F. of the energy gained by the solution of the zinc, and displacement thereby of copper from the copper-sulphate solution; and, thirdly, the somewhat analogous want of completeness in transformation into E.M.F. (and quantity of electricity jointly) of the energy gained by the transformation into ordinary copper of the nascent metal thus set free. In order to refer briefly to this possible want of completeness in development of E.M.F., it will be convenient to term that portion of the energy due to the various actions taking place in the cell that does contribute to the setting-up of difference of potential, the "adjuvant" portion of this energy; whilst the remainder is spoken of as the "non-adjuvant" energy. Of course the non-adjuvant energy in practice makes its appearance in the form of heat developed *ab initio*, and not in accordance with Joule's law—*i. e.* not due simply to the passage of a current through a resistance.

As regards the possible non-adjuvancy of energy thus indicated, it is to be noticed that whilst the observations of numerous experimenters agree in showing that, under certain conditions, at least an approximate equality subsists between the electromotive forces actually developed in a Daniell cell, and in various analogously constructed cells, and those corresponding to the net chemical changes taking place therein (viz., in the case of a Daniell cell, the displacement of copper from copper sulphate by zinc), this approximate equality does not exist under all conditions even in a Daniell cell, inasmuch as, first, considerable discrepancies exist between the values obtained by different observers working under different conditions, and, secondly, the same cell exhibits values varying with the rate of current-flow through so-called "polarization;" whilst, on the other hand, with certain forms of cell the maximum E.M.F. developed falls considerably short of that corresponding to the net chemical action. These discrepancies and amounts of falling short appear to be in certain cases considerably greater than can be accounted for by the

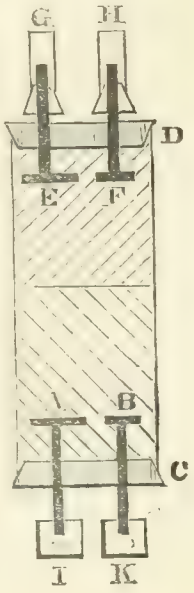
formation of solutions of zinc and copper sulphates &c. of different densities through "migration of the ions," thus indicating considerable extents of non-adjuvancy.

In order to obtain further information upon these points, a large number of observations have been made upon various forms of Daniell cell and allied combinations; the general results of which are that, with the normal Daniell combination (zinc, zinc sulphate, or dilute sulphuric acid, copper sulphate, copper), the amount of non-adjuvant energy with suitable plate-surfaces and with feeble rates of current-flow is insensible, but becomes very considerable with more rapid rates of flow, or with certain impure forms of metallic plate-surfaces—the non-adjuvancy being partly due to incomplete development as E.M.F. of the energy due to the solution of the zinc, but more especially to the imperfect development as E.M.F. of that due to the transformation of nascent into ordinary copper; whilst the formation of solutions of densities different from those of the fluids originally employed also contributes to the diminution in the effective E.M.F. of the cell. With certain other forms of cell, more or less non-adjuvancy exists under all circumstances. In the following paper only those experiments referring to the normal Daniell cell are described, the remainder being postponed to a later occasion.

Experiments made to determine the total fall in E.M.F. through so-called Polarization, occurring in variously arranged Daniell Cells for definite amounts of increase in the rates of Current-flow.

103. A gravity Daniell cell was constructed (fig. 1) with two zinc plates, A and B, supported so that their upper surfaces were in the same horizontal plane, by means of stout wires passing through an indiarubber cork, C, the under surfaces of the plates, and the wires between the cork and plates, being covered with gutta percha. This cork fitted into the lower end of a wide glass tube some 4 or 5 centim. in diameter and 12 or 15 long; at the other end was a precisely similar cork arrangement, D, carrying two copper plates, E and F, of which only the lower surfaces were uncovered with gutta percha. The plates A and E were each of such size as to expose precisely 2·5 square centim. of surface, whilst the plates B and F each exposed double that area. In order to arrange the cell as a gravity battery, a concentrated zinc-sulphate solution (sp. gr. nearly 1·4) was run into the cell until half full, and

Fig. 1.



then a cold-saturated solution of copper sulphate (sp. gr. somewhat below 1·2) was carefully floated on the top of the zinc-sulphate solution. In some experiments this disposition of the plates was reversed, the copper plates being lowest and the zinc plates highest; in these cases saturated copper-sulphate solution was first poured in, and then a lighter zinc-sulphate solution. To avoid the almost inevitable contamination of the zinc-sulphate solution with faint traces of copper sulphate which occurs when the former is poured on to the latter, no matter how carefully done, it was found more satisfactory to fill the cell half full of the zinc-sulphate solution, and then slowly to introduce the copper sulphate at the base of the cell through a U-tube passing through the cork C, the end inside the cell being drawn out to a point and bent downwards, so that the copper-sulphate solution flowed in gradually and lifted up the zinc-sulphate solution without passing into it as a jet and so more or less impregnating it with copper.

It is evident that, since A and B, E and F are respectively in the same horizontal planes, the resistance of the column of fluid between the plates, R, must be sensibly the same, whether the smaller pair of plates, B and F, or the larger pair, A and E, be employed to generate a current; either of which can at pleasure be done by simply connecting the mercury-cups G H I K, attached to the appropriate plates, with the extremities of a known external resistance. By measuring the difference of potential E subsisting between the ends of this resistance the current passing, C, is known; hence if R be known, the E.M.F. of the cell, e, is known, being given by the equation

$$e = E + CR.$$

The value of R can be deduced, with a fair amount of precision, from the results of two series of observations with varying currents, made, first, with the smaller, and, secondly, with the larger pair of plates, in the following way:—By dividing the actual current-strengths by 2·5 and 5·0 (the superficies in square centimetres of the plates respectively), two series of values of E for corresponding "current-densities" (rates of flow per square centimetre of plate surface) are obtained, by interpolation from which, for a given current-density D, two values, E_1 and E_2, are deduced for the smaller and the larger pair of plates respectively. The E.M.F. with the smaller pair, e_1, is manifestly

$$e_1 = E_1 + 2{\cdot}5\,DR,$$

whilst that with the larger pair is

$$e_2 = E_2 + 5{\cdot}0\,DR.$$

It is evident that, when the density of the current is the same, and the surfaces of the two copper and of the two zinc plates are respectively in the same conditions, the effect on the E.M.F. caused by the passage of a current must be sensibly the same, inasmuch as the same amount of zinc is dissolved and of sulphate of zinc formed, and the same quantity of copper is deposited and of copper sulphate decomposed, per square centimetre of plate surface ; so that e_1 must sensibly $= e_2$.

Hence, since

$$E_1 + 2{\cdot}5\,CR = E_2 + 5{\cdot}0\,CR,$$

it results that

$$R = \frac{E_1 - E_2}{2{\cdot}5\,D}.$$

By contrasting in this way the values of the E's obtained with various current-densities, a set of values for R are obtained, fairly concordant when the observations are carefully made and the plates of such materials as to remain in the same condition of surface throughout the experiment, or nearly so, so that on introducing a given external resistance into the circuit, sensibly the same values for the E's are uniformly obtained. With sulphate-of-zinc solution surrounding the zinc plates, and with amalgamated plates (copper as well as zinc), this permanence is more readily ensured than when dilute sulphuric acid is used (with amalgamated zinc plates), or when the copper plate is not amalgamated, but only freshly coated with electro-deposited metal. Indeed, to obtain a sufficient number of readings when dilute sulphuric acid is employed, it is preferable to discharge the cell after an hour's use and recharge it, amalgamating the plates afresh, and filling up with the same solutions as before to exactly the same levels (ensured by suitably marking the glass), so that the resistance of the cell may vary as little as possible, the temperature being so adjusted as to be sensibly the same on the average throughout. Thus the following series of values was obtained as the average result of four sets of readings alternately with gradually increasing and gradually diminishing external resistances, with a cell containing nearly saturated copper-sulphate solution, freshly electro-coated copper plates, amalgamated zinc plates, and dilute sulphuric acid of 1·045 sp. gr., the plates being reamalgamated and reelectro-coated respectively for each successive series—all the observations being reduced to the same standard as that adopted throughout this paper, viz. the average reading at 15·5 of a large number of Clark's cells taken as 1·457 volt*.

* The exactness of this value depends not only on how far the average of these cells is identical with the average of those which served as the basis

Smaller plates.			Larger plates.		
C_1.	D_1.	E_1.	C_2.	D_2.	E_2.
·001738	·0006952	·869	·00385	·000770	·770
·000960	·0003840	·960	·001854	·0003708	·927
·000509	·0002036	1·018	·000999	·0001998	·999
·0002112	·00008448	1·056	·0005195	·0001039	1·039
·0001075	·00004300	1·075	·0002136	·00004262	1·068
·0000543	·00002172	1·085	·0001085	·00002170	1·085
·0000273	·00001092	1·092	·0000546	·00001092	1·092
·00001057	·00000423	1·097	·0000262	·00000524	1·097
·00000533	·00000213	1·100	·0000108	·00000216	1·100
·00000215	·00000086	1·103	·00000535	·00000107	1·103
·00000107	·00000043	1·103	·00000215	·00000043	1·103
·00000052	·00000021	1·103	·00000105	·00000021	1·103

104. From these figures the following values for R are deduced—the first four determinations only of each series being employed, on account of the smallness of the differences between E_1 and E_2 in the other cases:—

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of Clark's valuation (Proc. Roy. Soc. xx. p. 444), but also on the exactness with which the B.A. unit of resistance is determined. If this latter be too small—as appears probable from the experiments of Joule, and of the writer and Mr. Rennie (Phil. Mag. March 1881, p. 169), from the results of Rowland, and from the recent experiments of Lord Rayleigh and Prof. Schuster—the true value of an average Clark's cell is below 1·457 to the same proportionate extent: thus, if the B.A. unit be really 0·99 $\frac{\text{earth-quadrant}}{\text{second}}$, the E.M.F. of an average Clark's cell is only $0{\cdot}99\times1{\cdot}457\times10^{8}=1{\cdot}442\times10^{8}$ C.G.S. units. In view, however, of the fact that the question of the amount and even of the direction of the error (if any) in the B.A. resistance unit is not yet absolutely settled, it is assumed in this paper that there is no error at all.

For analogous reasons the value of J is assumed, as previously, to be 42×10^{6} ergs, the evidence in support of its having a higher value still being not inconsiderable; although the probability is that, if the B.A. resistance unit be only 0·99 $\frac{\text{earth-quadrant}}{\text{second}}$, J is close to $41{\cdot}5\times10^{6}$. The value of χ (the electrochemical constant defined in § 7) deduced in § 9 as the most probable, viz. ·000105, is also adhered to, notwithstanding that Mascart's recent experiments (*Comptes Rendus*, xciii. p. 50) tend to indicate that this value is too large, ·0001044 representing his final result: this value is 0·8 per cent. lower than ·00010527, the mean value deduced from Kohlrausch's experiments. If J be taken $=41{\cdot}5\times10^{6}$, and χ be assumed $=$ ·0001048 (mean of Mascart and Kohlrausch's results), the value of χJ, the factor for reducing gram degrees to C.G.S. E.M.F. units, becomes 4349, or 1·4 per cent. less than 4410, the value hitherto assumed, and still retained in this paper.

Prof. S. P. Thompson applies the term "Faraday coefficient" to the numerical value χ (Journ. Soc. Arts, xxx. p. 34); should this term be generally accepted, the letter F might gracefully be used instead of χ to indicate the factor, just as J is used to indicate the Joule coefficient.

D.	E_1.		E_2.		E_1-E_2.	$R=\frac{E_1-E_2}{2\cdot5\times D}$.
	Observed.	Interpolated.	Observed.	Interpolated.		
·000770	...	·847	·770	...	·077	4·00 ohms.
·0006953	·869	...	...	·799.	·070	4·03 „
·0003840	·960	...	...	·921	·039	4·05 „
·0003708	...	·965	·927	...	·038	4·11 „
·0002036	1·018	...	...	·997	·021	4·12 „
·0001998	...	1·019	·999	...	·020	4·04 „
·0001039	...	1·050	1·039	...	·011	4·24 „
·0000845	1·056	...	...	1·047	·009	4·28 „
					Average	4·11 ohms.

Taking 4·11 ohms as the average value of R, the following numbers are calculated from the above observations, the values of E_1 and E_2 being obtained by interpolation:—

Values from observations with smaller plates.				Values from observations with larger plates.			Average value of E.M.F. of cell.	Fall.
D.	E_1.	$2\cdot5\times DR$.	$e_1=E_1+2\cdot5DR$.	E_2.	$5\cdot0\times DR$.	$e_2=E_2+5\cdot0DR$.		
0	1·103	...	1·103	1·103	...	1·103	1·103	0
·000001	1·103	...	1·103	1·103	...	1·103	1·103	0
·000002	1·100	...	1.100	1·100	...	1·100	1·100	·003
·000005	1·096	...	1·096	1·097	·001	1·098	1·097	·006
·00001	1·092	·001	1·093	1·092	·002	1·094	1·094	·009
·00002	1·086	·002	1·088	1·086	·004	1·090	1·089	·014
·00005	1·072	·005	1·077	1·065	·010	1·075	1·076	·027
·0001	1·051	·010	1·061	1·041	·021	1·062	1·062	·041
·0002	1·019	·021	1·040	·999	·041	1·040	1·040	·063
·0004	·957	·041	·998	·916	·082	·998	·998	·105
·0007	·868	·072	·940	·798	·144	·942	·941	·162

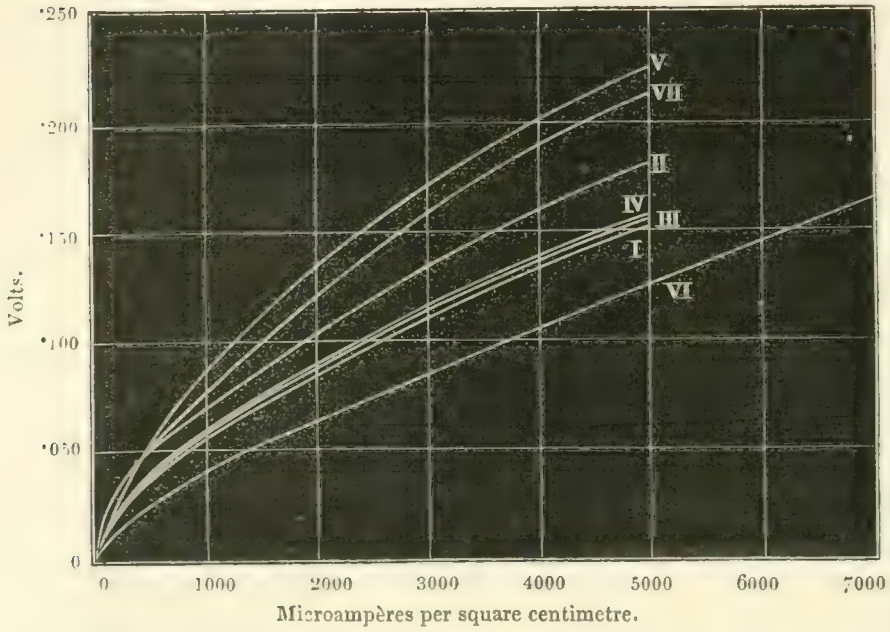
It is evident that the values of e_1 and e_2 accord so closely that their average may fairly be taken as representing, with but little error, the E.M.F. of a cell containing copper and zinc plates in the condition of those experimented with in this instance*.

105. In various other analogous experiments the concordance was usually not quite so close as in this example, a smaller number of readings (one or two sets only) being made;

* It might appear at first sight that a third valuation of the E.M.F. of the cell might be deduced as follows:—Since $e_1=E_1+2\cdot5DR$, and also $e_2=E_2+5\cdot0DR$, it results that $e_1=e_2=2E_1-E_2$; but it is evident that this value is such that e_1 is the arithmetical mean between e_2 and $2E_1-E_2$, and hence that the average of the three values must be identical with e_1, and therefore less exact than the mean of e_1 and e_2.

but the discrepancy was in no case so great as materially to influence the general character of the curve representing the variation in the value of the E.M.F. with varying currents, obtained on plotting the results by making the currents abscissæ and the electromotive forces ordinates. The following table (p. 273) exhibits, side by side, the values obtained in various acses selected as specimens: in numerous other cases not quoted, the curves obtained were considerably similar to those indicated by the figures given in the table and represented in fig. 2. The

Fig. 2.



resistance of the cell, R, is stated in ohms, and the electromotive forces in volts; the "maximum E.M.F." indicates the average value of the highest E.M.F. observed throughout the various series of readings, this value being always observed when either no current at all circulated, or a current of less magnitude than about 8 microampères* per square centimetre. The copper plates were uniformly surrounded by nearly saturated copper-sulphate solution (sp. gr. 1·175); the zinc plates were sometimes surrounded by nearly saturated zinc-sulphate solution (sp. gr. about 1·4), and were then lowest; in other cases they were highest, and were then surrounded either by zinc-sulphate solution of sp. gr. 1·10, or by dilute sulphuric acid of sp. gr. 1·045.

* In accordance with the nomenclature adopted by the recent International Electrical Congress, the term *ampère* is used throughout this paper to indicate what in the former papers of this series was designated a *weber*, viz. 0·1 C.G.S. current-unit; so that a microampère = ·0000001 C.G.S. current-unit = 10^{-7} ampère.

	I.	II.	III.	IV.	V.	VI.	VII.
Zinc plates	Pure metal scraped bright.	Bright pure metal.	Commercial zinc plate scraped bright.	Coated over with a film of copper by immersion in weak copper-sulphate solution.	Pure metal amalgamated.	Pure metal amalgamated.	Pure metal amalgamated.
Copper plates	Freshly electro-covered.	Freshly electro-covered.	Freshly electro-covered.	Freshly electro-covered.	Electro-metal thoroughly amalgamated.	Freshly electro-covered.	Electro-metal thoroughly amalgamated.
Fluid surrounding zincs	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·10.	Zinc-sulphate solution, sp. gr. 1·10.	Zinc-sulphate solution, sp. gr. 1·42.	Dilute sulphuric acid, sp. gr. 1·045.	Dilute sulphuric acid, sp. gr. 1·045.
R	11·2	13·0	9·5	5·4	11·15	4·11	4·2
Maximum E.M.F.	1·098	1·093	1·091	1·050	1·094	1·103	1·096

Current-density.		Observed amounts of Fall in Electromotive Force.						
C.G.S. units.	Microampères.							
·000001	10	...	...	·001	·001	...	0	...
·000002	20	...	...	·006	·006	...	·003	...
·000005	50	·009	·010	·013	·011	·005	·006	·008
·00001	100	·018	·021	·019	·018	·011	·009	·016
·00002	200	·025	·032	·028	·028	·023	·014	·028
·00005	500	·038	·049	·044	·046	·048	·027	·050
·0001	1000	·052	·069	·056	·060	·083	·041	·082
·0002	2000	·082	·101	·085	·087	·134	·063	·120
·0003	3000	·106	·130	·107	·110	·174	·084	·160
·0005	5000	·149	·179	·151	·155	·223	·125	·212
·0007	7000	...	...	...	...	...	·162	...

It is noticeable that whilst experiments Nos. I. and II. show that the curves obtained are by no means necessarily identical even when the conditions are sensibly the same (owing apparently to differences in the character of the copper deposited during the action of the cell), experiments Nos. I., III., and IV. indicate that but little difference in the curve is brought about by using commercial instead of pure zinc, or by altering the surface of the zinc by covering the bright metal with a film of copper (although more or less marked depressions in the maximum E.M.F. are occasioned thereby); on the other hand, experiments V. and VII., as compared with the others, indicate that amalgamating the copper renders the rate of fall in E.M.F. sensibly more rapid. But little difference, on the whole, is apparently occasioned in the curves by the use of zinc-sulphate solution (whether stronger or weaker than the copper-sulphate solution), as compared with dilute sulphuric acid; what difference is brought about is of this kind—that the sulphuric-acid curves slightly underlie the zinc-sulphate curves.

In none of the experiments made was any measurable depression of the E.M.F. of the cell brought about when the current flowed at a rate not exceeding 8 microampères per square centimetre; and in several cases this rate of flow might be doubled before any depression greater than ·001 volt (0·1 per cent.) was occasioned. As a rule, when the current-density was from 30 to 50 microampères per square centimetre, a diminution in the E.M.F. of from 0·5 to 1 per cent. was brought about; whilst diminutions of 10 per cent. and upwards were occasioned when the current-density exceeded 3000. Supposing the same values to hold for ordinary Daniell cells (which is probably not quite the case, as the zinc and copper plates are usually unequal in size), it results that, with cells of ordinary dimensions (*e. g.* holding a litre and exposing a surface of 500 square centimetres), no appreciable diminution in the E.M.F. would be brought about when the current does not exceed $500 \times 8 = 4000$ microampères (·004 ampère); whilst diminutions of several tenths per cent. would be occasioned with currents of fivefold magnitude (·02 ampère), and diminutions of 10 per cent. and upwards when the current exceeds $500 \times 3000 = 1{,}500{,}000$ microampères (1·5 ampère).

Experiments made to determine the maximum Electromotive Forces of variously arranged Daniell Cells containing Zinc-sulphate solution around the zinc, and the maximum proportion of the Fall in the E.M.F. of the Cell with gradually increasing currents, that could be due to accumulation round the plates of fluids of different densities through the migrations of the ions.

106. A long series of experiments was next made with the object of determining how far the very considerable diminution in the E.M.F. of a Daniell cell, above shown to exist when moderately strong currents are generated, can be accounted for by the strengthening of the solution of zinc surrounding the plate, and the weakening of the copper-sulphate solution round the copper plate, which necessarily take place in consequence of the migration of the ions accompanying the passage of the current. Inasmuch as the use of dilute sulphuric acid introduces complications, these observations were made in the first instance with zinc-sulphate solutions only round the zinc plates; later on (§ 111), similar experiments with cells containing sulphuric acid are described.

It results from the experiments of Moser (*Annalen der Physik*, iii. p. 216) and H. F. Weber (Phil. Mag. [5] viii. pp. 487 & 523), that when a stronger solution of zinc (or copper) sulphate diffuses into a weaker one of the same salt, plates of zinc (or copper) placed in the two solutions acquire different potentials, that in the stronger solution being at the higher potential: the potential difference reckoned per a constant difference in specific gravity of solution (*e. g.* a difference of 0·1) is not constant, but depends on the actual values of the specific gravity, being less the stronger the solutions. The maximum value obtained (in the case of zinc-sulphate solutions containing respectively 60 and 1 per cent. of crystallized salt) was only ·036 Daniell, or about ·040 volt; whence it would seem that if the effect produced by zinc-sulphate solution in diffusing into copper-sulphate solution is of the same order of magnitude as that produced by diffusing into another zinc-sulphate solution of strength equivalent to that of the copper-sulphate solution, the effect on the E.M.F. of a Daniell cell, due to migration of the ions, cannot possibly materially exceed ·04 volt; whilst, whatever the magnitude of the effect, it must tend to diminish the E.M.F. of the cell, since it partially equalizes the difference of potential between the zinc and copper plates set up by the chemical action alone. In order to find out the actual magnitude of the diminution due to this diffusive action in various cases, a number of determi-

nations were first made of the E.M.F. set up in various forms of Daniell cell when generating currents of magnitude not exceeding 8 microamperes per square centimetre (and usually when generating no current at all),*the zinc- and copper-sulphate solutions being in any given case of the same specific gravity**. The average values being thus fixed, the observations were then repeated, using solutions not of the same specific gravity. It is particularly noteworthy in this connexion, that the E.M.F. of a Daniell cell was found to be *sensibly independent of the strength of the solutions when both are of the same specific gravity—i. e.* the deviations observed from equality were less than the experimental errors.

The cells employed were constructed of two small beakers —one containing zinc-sulphate solution and plates of zinc (scraped bright, covered with electro-deposited metal, or amalgamated with pure mercury), and the other copper-sulphate solution and similar copper plates. The two beakers were connected, in the way described by Raoult (*Ann. de Chim. et de Phys.* 4th series, ii. p. 317, and iv. p. 392), by means of an inverted Y-tube, the ends of which, dipping into the two beakers respectively, were covered over with thin bladder, the tube being filled with the zinc-sulphate solution. Each one of the zinc and copper plates used was soldered to a platinum

* Although two solutions of zinc and copper sulphate, respectively of the same specific gravity, are not absolutely chemically equivalent to one another (*i. e.* do not contain precisely equivalent percentages of the two salts), yet the difference in specific gravity between any two solutions of equivalent strengths is so small that, for all practical purposes, it may be assumed that when the specific gravity is the same the solutions are of equivalent strengths. Direct determinations of the specific gravities of various solutions of equivalent strengths (made by dissolving known weights of the air-dry pure salts to known weights of aqueous solutions), gave the following results:—

Percentage of $CuSO_4$, $5H_2O$.	Specific gravity at 18°.	Equivalent percentage of $ZnSO_4$, $7H_2O$.	Specific gravity at 18°.
1	1·006	1·15	1·007
3·75	1·023	4·3	1·026
7·5	1·047	8·6	1·053
15	1·098	17·2	1·107
22·5	1·156	25·9	1·163
30	1·214	34·5	1·223

The 30-per-cent. solution of copper sulphate was slightly supersaturated, and deposited crystals on standing in a closed vessel; saturated zinc-sulphate solution has a specific gravity upwards of 1·4. It is noteworthy that these figures indicate that when given bulks of water and of either zinc- or copper-sulphate solution are mixed, an *increase* in bulk occurs, thus agreeing with J. Thomsen's result that dilution of a solution of either salt is accompanied by heat-absorption (§ 113).

wire fused into a piece of glass tubing, forming a mercury-cup; the soldering and the whole length of the platinum wire being thickly covered with gutta percha, so that only zinc (or copper) was exposed to the fluid. The plates were then connected with a series of mercury-cups in such a way that, by simply moving a double switch connected at one end with two mercury-cups in connexion with the electrometer-quadrants, and dipping at the other end into two of the series of cups, any required pair of zinc and copper plates could be brought into connexion with the electrometer. Fig. 3 shows the arrangement used for two pairs of plates, and fig. 4 that for

Fig. 3.

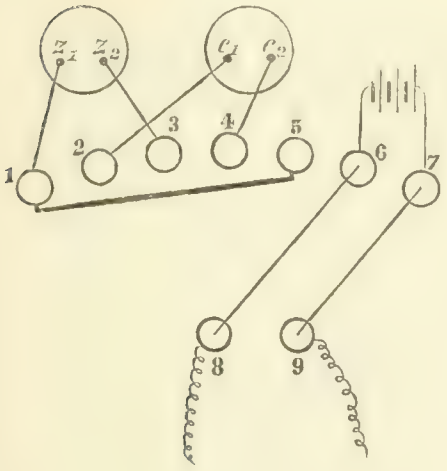
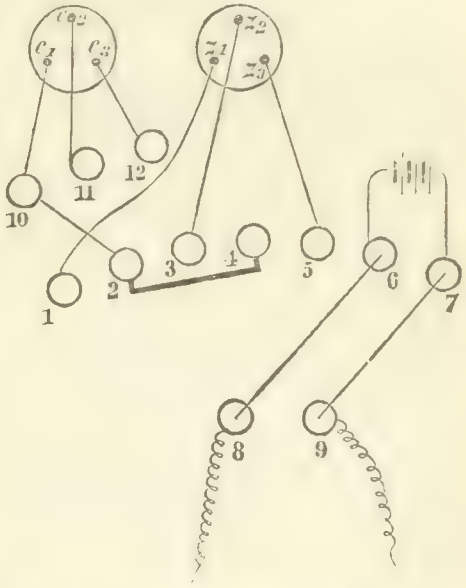


Fig. 4.



three pairs. In the former the two zinc plates, z_1 and z_2, are connected with mercury-cups Nos. 1 and 3 (No. 1 being also connected with No. 5), whilst the two copper plates, c_1 and c_2, are connected with cups Nos. 2 and 4 respectively. Cups Nos. 6 and 7 are connected with a standard cell (the error of which, in reference to the average taken as 1·457 volt, is known). By means of a double switch any pair of the series of cups 1 to 7 can be connected with cups 8 and 9, which are connected with the electrometer through the usual reversing-gear; so that when Nos. 1 and 2, 2 and 3, 3 and 4, and 4 and 5 are thus connected, the electromotive forces due to the combinations z_1c_1, c_1z_2, z_2c_2, c_2z_1 are respectively read off; whilst when 6 and 7 are connected (as represented in the figure) the electrometer-scale is standardized. In actually taking readings a double set was always employed, the switch being successively used to connect the four combinations and the standard with the electrometer, and then to connect them

again in reversed order; so that, by taking the averages of the two sets, any error due to running down of the electrometer during the readings might be eliminated (in practice the running down during the period was insensible, the variation being usually at most only 2 or 3 per cent. during the whole day, and often much less). When three pairs of plates were used, the three zincs were connected respectively with cups 1, 3, and 5; whilst cup No. 2 was connected with No. 4, and also, by means of a movable wire, with either of three other cups, Nos. 10, 11, and 12, with which respectively the three copper plates were connected: so that when No. 2 was connected with 10, as represented in the figure, the combinations c_1z_1, c_1z_2, and c_1z_3 could be read off by connecting the double switch with 1 and 2, 2 and 3 (or 3 and 4), and 4 and 5 successively; and similarly for the other combinations.

107. The ultimate results of upwards of a hundred series of valuations of the electromotive forces of various combinations, mostly lasting over four hours, were as follows:—

(1) With the stronger solutions used (specific gravities 1·100 to 1·175) the E.M.F. set up after the first few minutes remained sensibly constant for several hours (the temperature being constant), never differing from the final average of the four average sets of readings made in each of the first four hours by amounts outside the limits of observational error. For instance, the following values were obtained in one experiment, in which the temperature throughout was close to 18°, the specific gravity of the solutions being 1·175:—

Combination.	Average E.M.F. determined during the				Final average.
	1st hour.	2nd hour.	3rd hour.	4th hour.	
Amalgamated zinc—electro-copper ...	1·112	1·113	1·114	1·114	1·1132
Bright zinc—electro-copper	1·110	1·109	1·112	1·110	1·1102
Amalgamated zinc—bright copper ...	1·119	1·122	1·122	1·120	1·1207
Bright zinc—bright copper	1·117	1·115	1·118	1·118	1·1170

Very similar results were obtained in all the other cases. After twenty-four hours the E.M.F. usually diminished to a greater or less extent. These changes are referred to later on (§ 108), being probably due to oxidation of the metals by dissolved air.

(2) With weaker solutions (sp. gr. 1·0065 to 1·050) the E.M.F developed during the first half hour or so was usually

slightly lower than the value attained subsequently; which value remained sensibly constant for several hours, and then fell to a greater or lesser extent, as with the stronger solutions. Accordingly, in such cases the lower readings during the first half hour or so were not taken into account in the final average. For any given combination of plates, the final average thus obtained with weaker solutions was sensibly identical with that obtained with the stronger solutions*.

(3) The combinations that gave the most constant results on repetition of the experiments were those containing amalgamated zinc and either electro-copper or amalgamated copper; next to which were those with electro-deposited zinc and these same kinds of copper plates. Combinations containing either bright zinc or bright copper (*i. e.* rods of fused metal or sheets of rolled metal filed, scraped, or sand-papered to perfect brightness) exhibited a considerably wider range of variation. By comparing various bright copper plates with one and the same amalgamated zinc plate, or various bright zinc plates with one and the same electro-copper plate, it was found that differences, amounting nearly to 0·010 volt in the

* The conclusion that the E.M.F. developed by a given pair of plates immersed, the one in zinc-sulphate, the other in copper-sulphate solution, is sensibly independent of the strength of the solutions when both are of the same density (or, at least, that the variation in E.M.F. caused by variation in strength of the solutions is not outside the limits of experimental error), is further corroborated by the results of still more direct experiments on the matter. Three cells were arranged, containing solutions respectively of the specific gravities 1·010, 1·090, and 1·175, each containing a recently electro-coppered plate and a freshly amalgamated zinc plate. A number of readings were taken of the potential-differences subsisting between the plates in each case; and then the plates were exchanged—the pair from the first cell being placed in the second, that originally in the second being transferred to the third, and so on. After a new set of readings had been taken, the plates were again exchanged and a third set of readings taken; so that each pair of plates was read in each cell. The following figures were finally obtained, all readings taken during the first half hours after immersion of the plates being rejected:—

Specific gravity 1·010.		Specific gravity 1·090.		Specific gravity 1·175.	
1st pair of plates	1·1127	2nd pair of plates	1·1122	3rd pair of plates	1·1125
2nd „ „	1·1130	3rd „ „	1·1115	1st „ „	1·1118
3rd „ „	1·1140	1st „ „	1·1135	2nd „ „	1·1133
Mean	1·1132	Mean	1·1124	Mean	1·1125

In each case the value found as the mean for the three pairs of plates differs from the general average 1·1127 by an amount so small as hardly, if at all, to be outside the limits of experimental error.

most extreme instances, were observable in each case with different bright plates as compared with one another. On the other hand, on comparing various electro-copper or amalgamated copper plates with one and the same zinc plate, or various amalgamated zinc plates with one and the same copper plate, the extreme ranges of fluctuation were found to be not more than half those observed with bright plates, and usually did not exceed $\pm$·001 as compared with the average.

(4) As the ultimate average result of all the determinations made, it was found that an amalgamated zinc plate gave, when opposed to a given copper plate, an E.M.F. lower by ·002 volt than a bright zinc plate, and lower by ·001 volt than an electro-zinc plate. The actual differences in various experiments ranged in the former case between +·006 and −·004, the bright zinc plate sometimes giving a higher value, and sometimes a lower value, than the amalgamated plate—more frequently the former. (In the example quoted above, the bright zinc plate gave a value lower by ·0030 when opposed to an electro-copper plate, and by ·0037 when opposed to a bright copper plate.) With electro-zinc as compared with amalgamated zinc, the difference ranged between +·004 and −·003, the electro-zinc sometimes giving a higher and sometimes a lower value than the amalgamated zinc, more usually the former.

(5) Similarly, the effect of substituting a bright copper plate for an electro one in any given combination was uniformly to cause an increase in the E.M.F. by an amount varying between ·001 and ·010 volt, and averaging, on the whole, ·006. (In the example quoted above, bright copper gives a higher value than electro-copper by ·0075 when opposed to amalgamated zinc, and by ·0068 when opposed to bright zinc.) The effect of amalgamating a copper plate was found to be, on the whole, to give an E.M.F. lower by ·001 than that given under the same conditions by a freshly electro-coppered plate, the actual difference ranging between +·002 and −·003, the amalgamated plate sometimes giving a higher value and sometimes a lower one than the electro-plate, more frequently the latter. It is worthy of notice that no sensible difference was observable whether the plate amalgamated were previously of bright rolled metal or of electro-metal; so that, on the whole, the effect of amalgamating a bright rolled plate was to depress the E.M.F. by ·006 + ·001 = ·007.

(6) The average results of all the experiments made are contained in the following tabular statement: increase in E.M.F. is indicated by the + sign, and diminution by the − sign:—

Variation in the E.M.F. due to the use of zinc- and copper-sulphate solutions of different strengths between the limits of sp. gr. 1·0065 and 1·175, both solutions being of the same specific gravity in any given case . . . } Less than ±·001.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for fresh electro-copper :—				
Bright rolled copper sheet	+·010	+·001	·009	+·006
Amalgamated copper (surface wet with fluid mercury)	+·002	−·003	·005	−·001
Effect of substituting for fresh electro-zinc :—				
Bright cast zinc	+·005	−·005	·010	+·001
Amalgamated zinc (surface wet with fluid mercury)	+·003	−·004	·007	−·001
Electromotive force of combinations :—				
Amalgamated zinc—Amalgamated copper	1·115	1·110	·005	1·113
,, ,, Electro-copper	1·116	1·111	·005	1·114
,, ,, Bright copper	1·124	1·115	·009	1·120
Electro-zinc—Amalgamated copper	1·116	1·110	·006	1·114
,, ,, Electro-copper	1·118	1·111	·007	1·115
,, ,, Bright copper	1·124	1·114	·010	1·121
Bright zinc—Amalgamated copper	1·119	1·109	·010	1·115
,, ,, Electro-copper	1·121	1·110	·011	1·116
,, ,, Bright copper	1·126	1·112	·014	1·122

108. The above figures are, as already stated, derived from the observations made during the first four hours after setting up the cells, the values registered during the first half hour or so with weaker solutions being rejected on account of their frequently being lower than the sensibly constant values attained to subsequently. When the cells were allowed to stand for twenty-four hours, or for longer periods, a greater or less fall in the E.M.F. was usually noticed: by taking out any pair of the plates (*e. g.* the amalgamated zinc and the electro-copper plates) and replacing them by a freshly prepared similar pair, it was found that the value rose again to sensibly the same value as on the previous day when only set up a few hours; so that by taking out first one and then the other of the pair of plates, determinations could be made of the amount of the total fall attributable to alterations of either plate separately. The actual values thus obtained fluctuated considerably. As a general rule, it was found that bright copper plates gave the same value after twenty-four hours' immersion as they did at first; but occasionally the value was lowered by ·002 to ·004. Electro-copper plates usually gave values less by ·002 or ·003

after twenty-four hours than at first; and in some instances, when the pink electro-metal was sensibly browned or discoloured by oxidation, either all over or here and there in spots, the diminution was even greater, sometimes as much as ·010. Amalgamated copper plates, if the surface were still white and brilliant after twenty-four hours, gave the same value as at first; but if the mercury had sunk into the copper, and brown spots of oxidized metal were here and there visible, the E.M.F. was a few thousandths of a volt lower than at first. With the zinc plates greater diminutions were, as a rule, observed. In some cases amalgamated plates showed little or no diminution after twenty-four hours; but generally a diminution of ·002 to ·005 was observed; whilst with bright and electro-zinc plates diminutions of from ·001 to ·015 were noticed. On the whole, after twenty-four hours the E.M.F. was sometimes unchanged, and sometimes less by ·020. After forty-eight hours the fall was more perceptible still, the few combinations that had not appreciably altered during twenty-four hours always showing a decided fall after a longer period. It is noticeable in this connexion, that cells after Daniell's construction, but containing other metals than copper, did not always give the same results as normal Daniell cells. Thus, for instance, whilst cells containing cadmium sulphate and cadmium plates behaved like copper Daniells, in that the E.M.F. was sensibly steady for some hours after first setting up, and only exhibited a measurable fall after several hours had elapsed, and not always then, analogous cells containing silver sulphate and silver plates invariably showed a perceptible fall in less than an hour after first setting up, the diminution becoming progressively greater as a longer time elapsed. That this diminution was due to a change (presumably oxidation by dissolved air) induced on the surface of the zinc plate was rendered evident by the fact that, on taking out from such a zinc-silver cell the zinc plate after the lapse of an hour or more, and opposing it to electro-copper in an ordinary zinc-copper Daniell, an E.M.F. was indicated considerably less than the value given by a fresh zinc plate, and usually just about as much less as represented the fall in E.M.F. observed with the zinc-silver cell at the end of the period during which it was observed, as compared with the E.M.F. at the beginning of that period, when it was newly set up.

109. It is further to be noticed, that all the above-mentioned figures were obtained with cells the nature of the construction of which was such that diffusion of copper-sulphate into the zinc-sulphate solution, and consequent deposition of copper on the surface of the zinc, *did not take place at all* during the whole time that the observations lasted. With ordinary

gravity-cells it is almost impracticable to prevent traces of copper reaching the zinc after some twenty-four hours at latest: the effect of the deposition of even the faintest traces of copper on the zinc surface is to cause a considerable fraction of the energy due to the solution of the zinc to become non-adjuvant, and thus materially to diminish the E.M.F. Thus, for instance, the following figures were obtained with one cell, and similar ones in numerous other cases:—

E.M.F. of gravity-cell newly set up: zinc plate wholly free from copper	1·103
After 8 hours: faint tarnish visible on the zinc .	1·095
„ 24 hours: slight film of copper on the zinc .	1·070
„ 48 hours: thick film of copper on the zinc .	1·045

In much the same way, the presence of even small quantities of impurities in the zinc causes an appreciable diminution in the E.M.F. In all the above-described observations, some of the purest zinc that could be bought was employed, being fashioned into plates and rods by melting in a porcelain crucible, pouring out on a fire-clay tile, and cutting into slips with a chisel, &c. When commercial sheet or cast zinc was used, or when pure zinc was amalgamated with imperfectly purified mercury, the E.M.F. developed when such zinc was opposed to a given copper plate was often very materially less than the value obtained with pure zinc, or pure zinc and pure mercury. Thus, whilst values varying between 1·111 and 1·116 were obtained with pure amalgamated zinc opposed to fresh electro-copper as above described, values varying from 1·080 to 1·109 were obtained with commercial zinc amalgamated with pure mercury, and with pure zinc amalgamated with impure mercury, similarly opposed.

It is abundantly evident from the above-described results (not to speak of those detailed later on), that "the E.M.F. of a Daniell cell" is a unit of comparison subject to decidedly wide limits of fluctuation; but that it is possible to reproduce a standard cell of the kind within a maximum limit of variation of about $\pm 0{\cdot}25$ per. cent., by using Raoult's form of construction together with a recently electro-coppered or amalgamated copper plate, and a pure zinc plate amalgamated with pure mercury, the zinc- and copper-sulphate solutions used being both of the same specific gravity, the precise value of the specific gravity being immaterial. Even when made, however, such a standard cell cannot be relied on for more than a few hours. It will be shown in a subsequent paper that whilst Latimer Clark's mercurous-sulphate cell is subject to an even wider range of fluctuation in E.M.F. than the best forms of Daniell cell, its permanence

is very far superior, a well-constructed cell giving absolutely the same value (when used in conjunction with a quadrant electrometer only) for months and months together.

110. The above described experiments having given results indicating the average values of the electromotive forces developed with different characters of plates when the specific gravities of the solutions surrounding the plates are the same, further series of observations were made with cells in which the zinc- and copper-sulphate solutions were not of the same specific gravity, the mode of operating being otherwise the same as before. As predicable from Moser's figures, and as previously observed by H. F. Weber, it was found that when the copper-sulphate solution was the more dense of the two, the E.M.F. was higher than with solutions of equal density, and *vice versâ* when the zinc-sulphate solution was the stronger. The average results of various observations, mostly lasting over four hours as before, are contained in the following table; in every case the zinc plate employed was of pure metal and amalgamated with pure mercury.

Effect of increasing the strength of the Zinc-sulphate solution relatively to that of the Copper-sulphate.

Specific gravity of solutions used.		Nature of copper plate.	Average E.M.F. at 18° of combination.			Ratio of difference in E.M.F. to difference in specific gravity.
Zinc sulphate.	Copper sulphate.		Observed.	Previously found for solutions of equal specific gravity.	Difference.	
1·145	1·010	Electro.	1·099	1·114	·015	
		Bright.	1·107	1·120	·013	
					·014	$\frac{·014}{·135} = ·105$
1·175	1·050	Electro.	1·102	1·114	·012	
		Bright.	1·108	1·120	·012	
					·012	$\frac{·012}{·125} = ·096$
1·260	1·050	Electro.	1·096	1·114	·018	
		Bright.	1·103	1·120	·017	
					·0175	$\frac{·0175}{·021} = ·083$
*1·395	1·010	Electro.	1·077	1·114	·037	
		Bright.	1·086	1·120	·034	
					·0355	$\frac{·0355}{·0385} = ·092$
*1·395	*1·175	Electro.	1·097	1·114	·017	
		Bright.	1·104	1·120	·016	
					·0165	$\frac{·0165}{·22} = ·075$

Effect of increasing the strength of the Copper-sulphate solution relatively to that of the Zinc-sulphate.

1·010	1·090	Electro.	1·129	1·114	·015	
		Bright.	1·135	1·120	·015	
					·015	$\frac{·015}{·080}$ = 0·188
1·010	*1·175	Electro.	1·132	1·114	·018	
		Bright.	1·141	1·120	·021	
					·0195	$\frac{·0195}{·165}$ = ·118
1·050	*1·175	Electro.	1·126	1·114	·012	
		Bright.	1·133	1·120	·013	
					·0125	$\frac{·0125}{·125}$ = ·100
1·090	*1·175	Electro.	1·123	1·114	·009	
		Bright.	1·128	1·120	·008	
					·0085	$\frac{·0085}{·0085}$ = ·100

It is evident from these figures that the accumulation round the zinc plate of zinc sulphate, and the exhaustion of the copper sulphate in the solution round the copper plate, even if carried out to the utmost possible extent, could not diminish the E.M.F. of a Daniell cell by more than ·03 to ·04 volt; whilst it is hardly probable, even with tolerably rapid currents, that the accumulation would suffice to diminish the E.M.F. by more than half that amount—a diminution almost negligible in comparison with the much larger amounts, 0·1 volt and upwards, found above to be due to this cause of diminution and non-adjuvancy jointly.

It is noticeable in passing that the above figures show that when two solutions of zinc and copper sulphates interdiffuse, the E.M.F. set up (like that produced by the interdiffusion of two zinc-sulphate, or of two copper-sulphate solutions, as studied by Moser) is of such a character that the stronger solution acquires the higher potential; the actual value of the E.M.F. developed also *is less for a given difference in specific gravity the stronger the solutions*, and, so far as the two sets of results can be compared, agrees fairly with the values deducible from Moser's experiments—indicating that the difference between the E.M.F. set up when two different solutions of $\left\{\begin{matrix}\text{zinc}\\\text{copper}\end{matrix}\right.$ sulphate diffuse into a constant $\left.\begin{matrix}\text{copper}\\\text{zinc}\end{matrix}\right\}$ sulphate solution is substantially the same as the E.M.F. set up when these two different $\left.\begin{matrix}\text{zinc}\\\text{copper}\end{matrix}\right\}$ sulphate solutions diffuse into each other.

* Solution almost saturated at ordinary temperatures.

Moreover the effect of a given alteration in the strength of the zinc-sulphate solution (every thing else remaining the same) is sensibly equal in amount, but opposite in sign, to that of a similar alteration in the strength of the copper-sulphate solution; it is evident that only when this is the case can the E.M.F. of the cell be the same whether the solutions be strong or weak. It will be shown in a future paper that this property, though not absolutely peculiar to the normal Daniell cell, is still rather the exception than the rule with analogous voltaic combinations.

Experiments with Daniell Cells containing dilute Sulphuric Acid.

111. In all the above described experiments, the solution surrounding the zinc plate was one of pure zinc sulphate. Various previous experimenters, notably H. F. Weber, have found higher values for the electromotive forces of Daniell cells containing dilute sulphuric acid than for those containing zinc-sulphate solution (*vide* § 113): *à priori* a higher value might be anticipated, because a notable amount of heat is evolved on further diluting even weak sulphuric acid; so that the total energy gained in the cell is not merely that due to the displacement of copper from copper sulphate by zinc, but also that gained in the dilution of the sulphuric acid through the diffusion which necessarily goes on. On trying experiments of the same kind as those just described with cells containing dilute sulphuric acid of various strengths (the zinc being amalgamated), it was found that not only was there a considerable want of permanence in the E.M.F. set up, the values perceptibly decreasing after a period of time (varying in different cases from half an hour to several hours) had elapsed since setting up the cells, but, further, that two cells, apparently set up in identically the same way, exhibited much greater differences in their readings during the period before the E.M.F. began to diminish, than were observed in the zinc-sulphate cells examined as above described. On the whole, however, the average values obtained distinctly pointed to the conclusion that, when the acid and copper-sulphate solutions are of the same specific gravity, the E.M.F. rises with strength of the solution; and that when they are not of the same specific gravity (the acid not being stronger than specific gravity 1·18), the E.M.F. is sensibly that due to a cell containing liquids both of specific gravity equal to that of the acid in the cell examined, corrected by the addition (or subtraction) of a quantity representing the difference in specific gravity of the solutions multiplied by the numerical value

deduced from the zinc-sulphate cell experiments, representing the difference in E.M.F. produced by a variation in the specific gravity of the copper-sulphate solutions equal to that between the acid and copper sulphate in the cell examined: *i. e*, if, for example, the E.M.F. of a cell containing both liquids of sp. gr. 1·100 be E, that of a copper-cell containing acid of sp. gr. 1·100 and copper-sulphate solution of sp. gr. 1·010 will be $E-(1{\cdot}100-1{\cdot}010)\times\alpha$, where α is the factor expressing the diminution per unit difference of specific gravity in the E.M.F. between the limits of sp. gr. 1·010 and 1·100 for copper-sulphate solution. Similarly, that of a cell containing acid of sp. gr. 1·100 and copper-sulphate solution of sp. gr. 1·175 would be $E+(1{\cdot}175-1{\cdot}100)\beta$, where β is the corresponding factor for a difference in specific gravity between the limits 1·100 and 1·175.

Thus, for instance, the following values were obtained with a cell containing fluids both of sp. gr. 1·175, the temperature being close to 18° throughout.

Period since setting up.	During 1st hour.	2nd hour.	3rd hour.	4th hour.	Average.
Electro-copper plate	1·161	1·162	1·163	1·160	1·1615
Bright „ „	1·167	1·168	1·168	1·166	1·1672

In most cases, however, a distinct fall of upwards of ·005 volt occurred in less than four hours.

A number of similar series of observations (upwards of thirty) with various other cells, in which the fluids were always of equal specific gravity, gave the following results, the observations being only continued as long as the E.M.F. remained sensibly constant—*i. e.* for a period of time varying from thirty minutes to four hours, and averaging about two hours. In all these experiments electro-copper and amalgamated pure zinc plates were employed.

Specific gravity of fluids.	Electromotive force set up, in volts.		
	Maximum.	Minimum.	Average.
1·010	1·143	1·121	1·129
1·050	1·150	1·128	1·139
1·090	1·155	1·137	1·148
1·175	1·179	1·161	1·169

Evidently, even with the weakest acid, the E.M.F. is sensibly above that developed with zinc-sulphate cells—viz. 1·114;

whilst with stronger solutions the difference is yet more marked.

112. A number of analogous observations were made with cells containing dilute sulphuric-acid and copper-sulphate solutions, not both of the same specific gravity: the average results were as follows:—

Specific gravity of solutions.		Excess of specific gravity of copper sulphate over acid solution.	Approximate correction for excess of specific gravity of copper sulphate.	Average E.M.F. observed.	Observed E.M.F. corrected for excess of specific gravity of copper sulphate.
Acid.	Copper sulphate.				
1·010	1·050	+·040	−0·008	1·147	1·139
1·010	1·090	+·080	−0·015	1·135	1·120
1·010	1·175	+·165	−0·019	1·137	1·118
				Average.....	= 1·126
1·050	1·175	+·125	−0·012	1·148	1·136
1·175	1·050	−·125	+0·012	1·168	1·180
1·175	1·090	−·085	+0·008	1·164	1·170
				Average.....	= 1·175

The final averages representing the E.M.F. corrected to the uniform specific gravities 1·010, 1·050, and 1·175 respectively, do not differ from the values directly obtained as just described for these specific gravities by amounts outside the limits of experimental error in this class of the various experiments made.

A peculiar result was obtained with cells containing sulphuric acid of sp. gr. 1·265, and nearly saturated copper-sulphate solution of sp. gr. 1·175. The E.M.F. was considerably depreciated, the average value in four sets of experiments with electro-copper and amalgamated pure zinc plates being only 1·084 (maximum 1·095, minimum 1·067). On standing a few hours, copper-sulphate crystals formed at the junction of the two fluids, showing a much less degree of solubility of the salt in the acid fluid formed than in pure water.

Relationships between the maximum E.M.F. developed by a Daniell Cell and the Energy due to the net Chemical action taking place therein.

113. The above-described results afford a ready explanation of the discrepancies between the valuations of the E.M.F. of a Daniell cell that have been made in absolute measure by various observers, amongst the more important of which may be cited those of Bosscha (Pogg. *Ann.* ci. p. 517, 1856), von Waltenhofen (Pogg. *Ann.* cxxxiii. p. 478, 1868),

Kohlrausch (Pogg. *Ann.* cxli. p. 456, and *Ergänz.* vi. p. 35), H. F. Weber (Phil. Mag. 1878, [5] v. p. 189), and J. Thomsen (Wied. *Ann.* xi. p. 246, 1880), all of which valuations lie between 1·088 and 1·132 volt when reduced to that unit (and, in the case of Bosscha's results, corrected for an error of about 8 per cent. in the value of the coil used by him). To these may be further added the electrostatic valuations of Sir W. Thomson and Latimer Clark, both of which lie near to 1·11 volt. Favre (*Comptes Rendus*, lxix. p. 35) and Raoult (*Ann. Chem. Phys.* [4] ii. p. 338, and iv. p. 392) obtained by methods involving calorimetric measurements numbers representing the "galvanic heat" of a Daniell cell, and equivalent to considerably smaller electromotive forces, their valuations (23993 and 23900 gramme-degrees respectively) corresponding to 1·058 and 1·054 volt. In these instances, and in the case of the lower values obtained by other observers, doubtless the "polarizations" produced by the passage of the tolerably powerful currents employed were considerable. The highest values were obtained with cells in which dilute sulphuric acid was used; thus, H. F. Weber found that a perceptibly higher value was obtained with such a cell than with one containing zinc-sulphate solution, viz. 1·1317 and 1·1286 (mean = 1·1301), as compared with 1·0954. That this should be the case is predicable from the nature of the heat-evolutions taking place when zinc is dissolved in acid of various strengths. Let an amount of heat, H_1, be evolved when a gramme-equivalent of zinc oxide is dissolved in sulphuric acid of given strength, SO_4H_2, m H_2O; and let H_2 be the heat evolved on its solution in acid of a different strength SO_4H_2, n H_2O, n being less than m. Let the solution SO_4Zn, nH_2O, resulting in this latter case, evolve h_1 of heat on the addition of $(m-n)$ H_2O, so as to form the solution SO_4Zn, m H_2O; and let the heat evolved on the addition of this quantity of water to SO_4H_2, n H_2O, so as to convert it into SO_4H_2, m H_2O, be h_2. Then, if the zinc oxide were dissolved in the stronger acid, and the zinc sulphate diluted, the heat-evolution would be H_2+h_1; whilst if the acid were diluted first, and the zinc oxide were then dissolved in it, the total heat evolved would be H_1+h_2. Since of necessity the two amounts of heat, H_2+h_1 and H_1+h_2, must be equal, it results that $H_2=H_1-h_1+h_2$. Now h_2 is a considerable positive quantity in all cases; whilst Thomsen's results on the heat evolved on solution of salts in water (*Deut. chem. Ges. Berichte*, 1873, p. 710) indicate that when the solution is accompanied by heat-absorption (as is the case with zinc sulphate), a further heat-absorption takes place on diluting a stronger solution of the salt with water, so that h_1 has a negative value. Hence, on both

accounts, H_2 is greater than H_1; that is, the work gained in the synthesis ZnO, SO_3 x aq. increases as x diminishes. Since the net chemical action in a Daniell cell is equivalent to the result of the actions (Zn, O)+(ZnO, SO_3 x aq.)−(Cu, O, SO_3 y aq.), it finally results that the chemical action taking place in the cell develops an amount of energy which increases, *ceteris paribus*, as x diminishes, *i. e.* is greater the more concentrated the acid surrounding the zinc.

On the other hand, when the zinc plate is surrounded by zinc-sulphate solution instead of sulphuric acid, the effect of variation in the strengths of the copper- and zinc-sulphate solutions will be comparatively but small when both are of the same degree of molecular concentration (which, as shown above, is very nearly the case when they are of the same density). If SO_4Zn, nH_2O evolves, as before, h_1 on addition of $(m-n)H_2O$, and SO_4Cu, nH_2O evolves h_3 on a similar addition, and if H_3 and H_4 are respectively the heats evolved when zinc displaces copper from $CuSO_4, nH_2O$, and $CuSO_4, mH_2O$, it results that, if zinc displace copper from the stronger solution and the resulting SO_4Zn, nH_2O be diluted to $ZnSO_4, mH_2O$, the heat evolved will be H_3+h_1; whilst if the copper-sulphate solution be first diluted and then the zinc displaces the copper, the heat-evolution is H_4+h_3. Since, of necessity, $H_3+h_1=H_4+h_3$, it follows that $H_3=H_4+h_3-h_1$. Now, since the solution of zinc and copper sulphates (crystallized) is in each case accompanied by heat-absorption, it results that h_3 and h_1 are both negative, and hence that h_3-h_1 is negligible if h_3 is any thing like comparable with h_1 in magnitude; so that in this case the energy developed by the net chemical action taking place in a Daniell cell must be practically independent of the degree of concentration of the solutions.

114. The earlier calculations of J. Thomsen, referred to in §16, as to the heat evolved in the displacement of copper from copper sulphate by zinc, are for various reasons probably less accurate than the later results obtained by him (*Journ. prak. Chem.* [2] xi. p. 412, and xii. p. 271); these different values may be thus contrasted, the values being gramme-degrees per gramme-molecule:—

Values from experiments by Andrews, Dulong, Hess, Favre and Silbermann, and J. Thomsen.	Values from later experiments of J. Thomsen.
Zn, O, SO_3 aq. = 108460	106090
Cu, O, SO_3 aq. = 56216	55960
Difference = 52244	50130
Corresponding in volts (per gramme-equivalent) to 1·152	.... 1·105

The earlier value is deduced from observations in which the heat of formation of copper oxide from the metal by combustion is involved, the copper being in a more or less *compact* state, filings &c.; the latter involves the determination of the heat-evolution during the precipitation of *spongy* copper from copper sulphate by iron. Leaving out of sight other sources of difference between the two values, this affords a reason why the former value should be the higher, since heat is evolved in the transformation of spongy into compact copper *. On the whole, it is evident that the net chemical change taking place in a Daniell cell (*i. e.* the displacement of copper from copper sulphate by zinc) corresponds to an E.M.F. which is a little higher than 1·105 volt by an amount which is the greater the more compact the copper precipitated, and is approximately constant when the zinc plate is surrounded by zinc-sulphate solution of the same equivalent strength as the copper-sulphate solution surrounding the copper plate, but is influenced by the strength of the dilute sulphuric acid when the fluid surrounds the zinc plate. The amount of this influence can be approximately calculated from Thomsen's determinations of the heat developed in the formation of the solutions of strengths indicated by H_2SO_4, nH_2O where n varies (*Dent. chem. Ges. Berichte*, iii. p. 496). Thomsen finds in gramme-degrees per gramme molecule :—

n	gramme-degrees.	n	gramme-degrees.
9	14940	199	17056
19	16248	399	17304
49	16676	799	17632
99	16850	1599	17848

From which table the values for any intermediate values of n can be obtained by interpolation. In the experiments

* That this is so is shown by the circumstance that if a current be sent through a decomposing-cell containing copper-sulphate solution and copper electrodes, of which the positive one is of compact rolled metal, a considerably higher difference of potential is set up, under any given conditions and with a steady current, than is set up when the + electrode is replaced by one covered with freshly electro-deposited metal. The more spongy texture of the latter corresponds to a greater heat-development during solution than that taking place with the compact metal, and hence to a diminution of the work that has to be done by the current in passing; with not very powerful currents the difference often exceeds ·02 or ·03 volt, corresponding to 450 to 700 gramme-degrees per gramme equivalent. In a somewhat similar way, but using the mercurial calorimeter, Favre found (*Comptes Rendus*, lxxiii. p. 1258) that electro-copper gave out about 1000 gramme-degrees more heat than rolled metal per gramme equivalent; this would correspond to an E.M.F. of ·044 volt.

leading to the valuation 50130 gramme-degrees for the heat developed during the precipitation of a gramme molecule of copper by zinc, Thomsen used fluids containing altogether 800 molecules of water to one of zinc sulphate, &c. Hence, were the acid used in the synthesis Zn, O, SO_3 aq., to be H_2SO_4, 17 H_2O (corresponding nearly to the sp. gr. 1·175), instead of H_2SO_4, 800 H_2O, the heat-development would be greater than 106090 by 17632−15986=1646 gramme-degrees per gramme molecule (15986 being the heat of dilution of sulphuric acid when $n=17$, deduced from the above table): this corresponds to 823 gramme-degrees per gramme equivalent =·036 volt; *i. e.* the E.M.F. corresponding to the heat-development during the displacement of copper from copper sulphate by zinc would be greater than 1·105 volt by ·036, or would be 1·141 volt. To this amount should also be added the value of the E.M.F. equivalent to the heat-absorption during the dilution of $ZnSO_4$, 17 H_2O to $ZnSO_4$, 800 H_2O. In a similar fashion it is calculable that to the value 1·105 should be added the amounts ·023, ·019, and ·008 volt when sulphuric acid of sp. gr. 1·090, 1·050, and 1·010 respectively surrounds the zinc, giving the sums 1·128, 1·124, and 1·113 respectively. Hence, finally, the following tables of values result:—

Zinc surrounded by Zinc-sulphate solution.

Electromotive force corresponding to net chemical action.	Electromotive force observed.	
$1·105+x$, where x is a small quantity varying with the physical condition of the copper deposited.	Bright zinc opposed to bright copper	1·122
	Bright zinc opposed to electro-copper	1·116
	Bright zinc opposed to amalgamated copper . . .	1·115
	Amalgamated zinc opposed to bright copper	1·120
	Amalgamated zinc opposed to electro-copper	1·114
	Amalgamated zinc opposed to amalgamated copper . .	1·113
	Electro-zinc opposed to bright copper	1·121
	Electro-zinc opposed to electro-copper	1·115
	Electro-zinc opposed to amalgamated copper . . .	1·114

Zinc surrounded by dilute Sulphuric Acid.

Strength of acid.	Calculated electromotive force.	E.M.F. observed with electro-copper opposed to amalgamated zinc.
$H_2SO_4, 358H_2O$ = sp.gr. 1·010	$1·113+x+y$	1·129
$H_2SO_4, 67H_2O$ = „ 1·050	$1·124+x+y$	1·139
$H_2SO_4, 37H_2O$ = „ 1·090	$1·128+x+y$	1·148
$H_2SO_4, 17H_2O$ = „ 1·175	$1·141+x+y$	1·169

where y is a small quantity corresponding to the heat absorbed on dilution of the zinc sulphate to $ZnSO_4, 800H_2O$.

It is hence evident that in all cases the agreement between the E.M.F. actually developed and that due to the net chemical and physical actions taking place is so close, that what differences exist lie within the limits of experimental error; so that, finally, the conclusion may be drawn that, under favourable conditions, the E.M.F. of a Daniell cell is that due to the net resultant of the various physical and chemical actions taking place, the whole of the energy being adjuvant, viz. that gained by the displacement from copper-sulphate solution of copper by zinc, together with that gained by the transformation into ordinary electro-metal of the "nascent" copper first thrown down by the action: whilst under other conditions the E.M.F. falls below this amount, even after making allowance for the effect of the migration of the ions in causing solutions of different specific gravities to accumulate round the plates, indicating non-adjuvancy of one or other or both of these component portions of the total energy gained.

Experiments made with a view to find whether the Fall in E.M.F. on increasing the Current-density is mainly dependent on changes taking place in connexion with the actions at the surface of the Zinc or at that of the Copper plate.

115. In order to trace out somewhat more completely, it possible, how far that amount of fall in the E.M.F. of a Daniell cell taking place as the current generated increases, which is not due to the accumulation of solutions of zinc and copper of different densities round the plates, can be attributed to actions taking place at one or the other plate respectively, the experiments described above (§103, 104) were repeated, with the difference that, instead of two sets of readings only being made (viz. when the two larger and the two smaller plates respectively were opposed), four sets were made—(1) when the two larger plates were opposed, (2) with the larger zinc and smaller copper, (3) with the two smaller plates, and

(4) with the smaller zinc and larger copper plates opposed. By interpolation from the direct observational results, the differences of potential between the plates for any constant current-value were then calculated in each of the four cases. By comparing the values thus obtained in cases (1) and (2) and in (4) and (3), two sets of differences were obtained, indicating the effects produced by halving the area of the copper plate, every thing else being the same throughout, saving that with results (1) and (2) the larger zinc plate, and with the other pair of results the smaller zinc plate, was opposed to the two copper plates respectively: although this modified the actual values obtained in each of the original sets of readings, yet it produced practically no effect on the differences. In just the same way, by comparing the interpolation values in cases (1) and (4) and in (2) and (3), two corresponding sets of differences were obtained, indicating the effects produced by halving the area of the zinc plate; as before, the two sets substantially coincided. Various experiments of this kind were made with different plate-materials and fluids surrounding them: whilst the numerical values obtained were found to be to some extent variable with these conditions, yet, on the whole, it was always found that *the effect of halving the area of the copper plate notably exceeded that of halving the area of the zinc plate*. For instance, the following numbers were obtained in one set of observations with a cell containing bright pure zinc plates surrounded by zinc-sulphate solution of sp. gr. 1·42, and freshly-coated electro-copper plates surrounded by copper-sulphate solutions of sp. gr. 1·175, the larger plates exposing a surface of 5·0 square centimetres, and the smaller ones exposing 2·5 square centimetres.

	Effect of halving the area of the copper plate.						
Current, in micro-ampères.	Larger zinc plate opposed.			Smaller zinc plate opposed.			Mean difference.
	(1) Larger copper.	(2) Smaller copper.	Difference.	(4) Larger copper.	(3) Smaller copper.	Difference.	
100	1·082	1·078	·004	1·076	1·073	·003	·0035
200	1·071	1·066	·005	1·064	1·060	·004	·0045
500	1·061	1·055	·006	1·052	1·047	·005	·0055
1,000	1·045	1·036	·009	1·035	1·026	·009	·0090
2,000	1·026	1·015	·011	1·011	·999	·012	·0115
5,000	·964	·947	·017	·950	·929	·021	·0190
10,000	·876	·843	·033	·854	·820	·034	·0335
20,000	·729	·672	·057	·698	·639	·059	·0580

Current, in micro-ampères.	Effect of halving the area of the zinc plate.						
	Larger copper plate opposed.			Smaller copper plate opposed.			Mean difference.
	(1) Larger zinc.	(4) Smaller zinc.	Difference.	(2) Larger zinc.	(3) Smaller zinc.	Difference.	
100	1·082	1·076	·006	1·078	1·073	·005	·0055
200	1·071	1·064	·007	1·066	1·060	·006	·0065
500	1·061	1·052	·009	1·055	1·047	·008	·0085
1,000	1·045	1·035	·010	1·036	1·026	·010	·0100
2,000	1·026	1·011	·013	1·015	·999	·016	·0145
5,000	·964	·950	·014	·947	·929	·018	·0160
10,000	·876	·854	·022	·843	·820	·023	·0225
20,000	·729	·698	·031	·672	·639	·033	·0320

116. In precisely the same way, the following mean difference-values were obtained in two other analogous experiments, in the first of which the zinc plates were amalgamated and immersed in zinc-sulphate solution sp. gr. 1·42, the copper plates being also amalgamated and immersed in copper-sulphate solution sp. gr. 1·175; and in the second of which electro-copper plates and copper-sulphate solution sp. gr. 1·175 were employed, conjoined with amalgamated zinc plates immersed in dilute sulphuric acid sp. gr. 1·045. The above mean difference-values are also exhibited in the table.

Current, in micro-ampères.	Amalgamated zinc and amalgamated copper—zinc sulphate solution.		Amalgamated zinc and electro-copper—dilute sulphuric acid.		Bright zinc and electro-copper—zinc-sulphate solution.	
	Effect of halving area of		Effect of halving area of		Effect of halving area of	
	Copper.	Zinc.	Copper.	Zinc.	Copper.	Zinc.
100	·001	·001	·001	·001	·0035	·0055
200	·004	·002	·002	·003	·0045	·0065
500	·009	·003	·003	·005	·0055	·0085
1,000	·015	·004	·005	·007	·0090	·0100
2,000	·024	·005	·011	·010	·0115	·0145
5,000	·040	·009	·020	·015	·0190	·0160
10,000	·049	·017	·041	·020	·0335	·0225
20,000	·058	·026	·056	·032	·0580	·0320

Much the same kind of result was obtained in various other similar experiments; the effect of halving the area of the copper plate was, especially with the stronger currents, much more marked than that of halving the area of the zinc

plate. The actual value of the depreciation produced with the stronger currents in either case, moreover, clearly indicates that the diminution cannot pssibly be solely due to the formation of stronger zinc-sulphate solution round the zinc plate, and weaker copper-sulphate round the copper plate, in the case of the smaller areas, than are produced with the larger ones; and hence the conclusion is arrived at that when, by reason of the production of a current, the E.M.F. of a Daniell cell is diminished, and the energy developed becomes proportionately non-adjuvant, the non-adjuvancy is ascribable, not merely to actions taking place at the surface of the zinc plate, but also, and more particularly, to those taking place at the surface of the copper plate. In the former case, the energy gained during the conversion of the metallic zinc of the plate into solution of zinc sulphate makes its appearance under such conditions partly as heat from the very commencement of the action, and is never wholly manifested as electric action expressible in volt-coulombs. In the latter case, the effect of the chemical action of the cell is to set free copper, which, in its transformation from the condition in which it is first set free (nascent copper) to the condition ultimately assumed (more or less compact electro-deposited soft coherent metal), causes a gain of energy which, like that due to the solution of the zinc, is partly manifested as heat *ab origine*, and is never, under such conditions, obtained wholly as electric action. The actual proportion of the energy due to the solution of the zinc or to the agglomeration (or allotropic modification) of the copper which is thus non-adjuvant, is variable within certain limits with the conditions of the experiment, the nature of the plate-surfaces and of the liquids in the cell, &c., but, *cæteris paribus*, is greater the stronger the current: with very feeble currents (of density not exceeding some 8 microampères per square centimetre), the proportion of non-adjuvant energy is too small to be measurable.

Summary of Results.

117. The above-described experiments, and the conclusions to be drawn from them, may be thus summarized:—

1. When a Daniell cell is constructed with equal-sized plates of pure zinc and pure copper (either compact bright metals, amalgamated plates, or plates covered with electro-deposited metal) immersed respectively in solutions of pure zinc and copper sulphates of the same specific gravity, and is made to generate a current not exceeding in density some 8 microampères per square centimetre, an E.M.F. is set

up varying within certain small limits according to the precise condition of the surfaces of the metals as regards polish, oxidation, &c., but always lying fairly close to 1·115 volt, and practically identical with the E.M.F. corresponding to the energy gained in the net chemical change ensuing, viz. the displacement of copper from copper-sulphate solution by zinc; so that under these conditions practically all the energy gained is adjuvant, whether due to displacement of copper by zinc, or to transformation into ordinary metal of the "nascent" copper thus set free.

2. When impure zinc, or pure zinc amalgamated with impure mercury, is used, a greater or less amount of the energy gained is non-adjuvant, even under conditions such as would with pure zinc cause all the energy to be adjuvant. The source of this non-adjuvancy evidently lies in the nature of the actions taking place at the surface of the zinc plate; the maximum E.M.F. that such a cell can generate is more or less considerably below 1·115 volt, in some instances by several per cent.

3. When the density of the current exceeds 8 micro-ampères per square centimetre, the E.M.F. of the cell falls more or less below 1·115 volt, owing to three causes, each of which produces an effect in the direction of diminishing the E.M.F. First, according as the current density is greater or smaller, a greater or lesser degree of non-adjuvancy of the energy gained in the conversion of metallic zinc into zinc-sulphate solution is brought about. Secondly, a greater or lesser degree of non-adjuvancy is similarly brought about in the energy gained by the transformation into ordinary copper of the "nascent" metal liberated at the surface of the copper plate; other things being equal, this source of non-adjuvancy distinctly predominates over the other source just mentioned. Thirdly, the passage of the current causes a weaker solution of copper sulphate to be formed round the copper plate, and a stronger one of zinc sulphate to be produced round the zinc plate, than were originally used: this sets up an inverse E.M.F., and diminishes the effective E.M.F. of the cell. The maximum possible diminution due to this cause does not exceed ·04 volt; whilst with a current the density of which amounts to ·003 ampère per square centimetre and upwards, the total diminution due to this cause together with the non-adjuvancy amounts to several times this maximum possible value. The diminution due to these various causes jointly constitutes what is sometimes spoken of as the "polarization" of the cell.

4. When the solutions of zinc and copper sulphate em-

ployed are not of the same specific gravity, the E.M.F. of the combination differs from that which would have been set up had both been of the same specific gravity by an amount which increases with the difference in specific gravity of the two solutions: if the copper-sulphate solution is the stronger, the E.M.F. is increased, and *vice versâ*. The amounts of increase and decrease are sensibly the same as the electromotive forces generated when two copper-sulphate or two zinc-sulphate solutions interdiffuse, the specific gravities of which are identical respectively with those of the two fluids actually present in the cell examined. So long as the two solutions are of the same specific gravity, the E.M.F. set up is, *cæteris paribus*, sensibly independent of the actual value of this specific gravity; at least the fluctuations observed are not outside the range of experimental errors.

5. When dilute sulphuric acid is used instead of zinc-sulphate solution, its specific gravity being the same as that of the copper-sulphate solution, an increase in the E.M.F. of the cell is produced which sensibly corresponds with the increase in the "heat of formation" of zinc sulphate when sulphuric acid is employed of the strength used in the cell, as compared with acid diluted to a larger extent (H_2SO_4, 800 H_2O). If the copper-sulphate solution differs from the acid in specific gravity, the latter not exceeding 1·18 in specific gravity, the E.M.F. is raised above or lowered below what it would have been had the copper sulphate been of the same specific gravity, by an amount which is sensibly the same as the E.M.F. generated by the interdiffusion of two copper-sulphate solutions the specific gravities of which are identical respectively with those of the two fluids actually present in the cell examined.

6. Owing to the diminution in the E.M.F. of a Daniell cell when generating a current, the fluctuations in the maximum values obtainable with any given cell with the physical condition of the surfaces of the plates and the time that has elapsed since its construction, the tendency to deposition of copper on the zinc by diffusion and the consequent diminution in E.M.F., and the variation in E.M.F. according as dilute acid of different strengths or zinc-sulphate solution is used to surround the zinc plate, it results that "the E.M.F. of a Daniell cell" is a very variable standard of E.M.F. and one singularly devoid of permanence. By taking suitable precautions in the construction of a cell (using pure zinc amalgamated with pure mercury, amalgamated or electro-copper, and pure zinc- and copper-sulphate solutions of the same specific gravity), a cell may be obtained the E.M.F. of

which does not differ more than $\pm 0{\cdot}25$ per cent. from 1·113 or 1·114 volt, according as amalgamated or electro-copper is used; but such a cell cannot be kept many hours without altering in value materially, and is in practice a very far less convenient standard than the mecurous-sulphate cell of Latimer Clark; for, notwithstanding that the limits of variation between two cells of this latter kind, similarly prepared, are somewhat greater than those of the best form of standard Daniell cell, it nevertheless possesses the valuable property of remaining sensibly constant (the temperature being the same) for many months, and even one or more years, as will be more completely shown in a future paper.

XXXIV. *Notices respecting New Books.*

Roorkee Hydraulic Experiments. *By* Captain Allan Cunningham, *R.E.* Vol. I., Text pp. 399+xvi. Vol. II., Tables—(1) Detailed Tables, (2) Abstract Tables, pp. 156+xii; pp. 49. Vol. III., Plates, lii. Roorkee: Thomason College Press, 1880, 1881.

THESE volumes give an account of experiments on Flow of Water in a Canal, which lasted about four years (December 1874 to March 1879, with some intermissions). We can hardly be expected to go through this mass of calculation, but will endeavour to put our readers *en rapport* with the results arrived at by Captain Cunningham in his valuable labours, as here carefully digested and served up for all whom they may concern. The mode of performing each experiment is given in great detail, not by any means a fault in investigations of this nature. In condescension, however, to the weakness of that bugbear, the general reader, a brief preface, or *résumé* of contents is attached to each chapter, giving "the gist of the chapter without experimental or argumentative detail." Whatever data or results appear to the author to be doubtful are indicated by a query (?) attached. River-experiments on a small scale are well known not to be applicable to large bodies of water. India, with its many large canals, affords favourable opportunities for such experiments, as our author observes, "with a fair expectation of a practically useful result."

The objects contemplated by Captain Cunningham were shortly these:—(1) discovery of a good method of discharge-measurement; (2) testing the applicability of known mean-velocity formulæ; (3) discovery of a good approximation to mean velocity: all three for large canals. The main results which are stated to have been arrived at are:—Loaded tube-rods give a rapid and sufficiently close approximation to mean velocity past a vertical; with good arrangements discharge-measurements obtained by the method advocated under similar conditions may be expected not to differ more than 3 per cent.; none of the known mean-velocity formulæ appear to be of really general applicability; central mean velocity-measurement

appears to be the best means of *rapid approximation* to mean (sectional) velocity, but the reduction must (at present) be effected by a coefficient to be *found by previous special experiment* at each site. Besides these the author enumerates several minor results of interest. Enough has been said to show that the work is a highly technical one; but though it is *caviare* to the general, it appears to be a most carefully compiled account of a series of experiments of great interest in its special department; and every thing has been done, in the way of tables, plates, and description, to put students of this particular branch in a position to follow the lines on which the experiments were carried out.

Experimental Researches into the Properties and Motions of Fluids, with Theoretical Deductions therefrom. *By* W. Ford Stanley. London: E. and F. N. Spon, 1881, pp. 550+xvi.

"I had taken for the amusement of my leisure an experimental examination of the undulatory theory of light, which I could not satisfactorily comprehend. In following up my experiments for two years, I found my eyesight impaired, and was advised that it would be necessary to leave these experiments, and also close application to reading, for some years, which I did very reluctantly. One branch of experiment, somewhat relative to my former studies, however, appeared open to me. The theory of undulation of light was generally introduced to our conception by philosophers by similitudes of the motions of water-waves and sound-waves; I thought I would investigate experimentally, as far as possible to me, to be assured our conceptions of these motions were real, upon inductive principles, similar to those I had been employing for investigation of light. In this subject, taking no preconceived theory whatever for my experiments, I soon became absorbed in observations of the motive effects evident in the directions taken by impressed forces in fluids under various conditions of resistance: wherein it appeared to me quite evident that there was yet an immense amount of work to be done in researches in the motions of fluids, before theoretical principles of the sciences of hydrodynamics and acoustics could be fixed upon mechanical principles with any great precision. It was therefore clear to me that in this direction I might, if I had the ability, enter upon fields of research quite as new as in my former studies."

This quotation will serve a double purpose: it will show that the author's style requires the aid, which he himself states that his book lacked, "of some well-read or well-instructed student" to prune it; and also how the present work has come into existence, in being the record of a very great number of apparently carefully performed experiments.

There is an immense deal of matter, accompanied by numerous plates, which may possibly in skilled hands be worked up, in some future treatise on hydrodynamics, into a more elegant form. In the

meanwhile, Mr. Stanley deserves our thanks for having carried on these researches, and for having, to the best of his ability, put them on record.

It was the author's idea to divide his book into four sections; the fourth however, on sound-motions in fluids, is for the present kept in abeyance, though we are told that it is written. It would now, of course, have to discuss the recent discoveries of telephones, microphones, phonographs, *et id genus omne*.

The first section treats of theoretical conditions of the fluid state and motive properties of forces in fluids. Here, in the second chapter, Mr. Stanley goes against the theory accepted from Segner to Clerk Maxwell, and takes the view that the surfaces of liquids are *extensile*, and not *tensile*. He, however, modestly enough thinks that he may have insufficiently worked out this subject, "although my experiments in this, as in most other subjects taken, are ten times the number given." Throughout there is the same diffidence. Thus, again, in his eighth chapter, on resistance of solids, he remarks that "it is very incomplete on certain points for want of sufficient research into the works of others, and more experiment; therefore it may be considered to be in a certain degree speculative."

The fifth chapter discusses the projection of fluids within like fluids, the principles of conic resistance, and planes of fracture and tension. This contains many matters of interest, amongst others the interesting experiments of Tait and O. Reynolds on smoke-rings, with some of the author's own experiments.

The second section is devoted to the discussion of cosmical phenomena; and herein he follows to some extent ideas put forth by Dr. Carpenter and other writers.

The third section is concerned with waves, experiments on which were made before the writer was acquainted with Scott-Russell's experiments. "As I was working for the demonstration of principles only, I do not regret the want of this knowledge at first; for if I had possessed it at the time it would have materially curtailed the interest I took in my experiments. . . . I have, however, replaced Mr. Russell's work for my own, where it appeared more demonstrative."

Mr. Stanley has received very little help, except from books, references to which are given throughout: he, however, warmly thanks Professors Fuller and Stokes for "valuable occasional critical notes" on the earlier portion. The book is well printed and illustrated.

XXXV. *Intelligence and Miscellaneous Articles.*

NOTE ON MR. J. J. THOMSON'S INVESTIGATION OF THE ELECTROMAGNETIC ACTION OF A MOVING ELECTRIFIED SPHERE. BY GEORGE FRANCIS FITZGERALD, FELLOW OF TRINITY COLLEGE, DUBLIN, ETC.

IN the April number of the Philosophical Magazine for the present year Mr. J. J. Thomson has given an interesting investigation of the electromagnetic action of a moving electrified sphere.

On the fourth page (*loc. cit.* p. 232) of this investigation, he makes an assumption, which he does not justify, in order to make the components of the vector-potential of electromagnetic induction satisfy the condition

$$\frac{dF}{dx}+\frac{dG}{dy}+\frac{dH}{dz}=0.$$

As it seemed very likely that they ought to satisfy this condition, I thought it worth while bringing before the Society a justification of his assumption, which, however, leads to slightly different equations from his, though his final result is unaffected.

Mr. Thomson has not touched the question of the discontinuity at the surface of the sphere, nor what becomes of the displacement when the sphere passes over a point. We may assume that the point remains in its displaced position (and this is practically what Mr. Thomson assumes); but if we do, the above condition is not fulfilled. We may assume that it returns to its original position, so that no permanent displacement takes place in the track of the sphere as would occur on Mr. Thomson's assumption. This, however, does not satisfy the condition either; and I have been led to assume that the particle does return to its original position, but that in some way or other the discontinuity at the surface acts as if a moving quantity of electricity acted like an element of an electric current. This may seem like begging the question; but it is only doing explicitly what Mr. Thomson does implicitly. It is evidently impossible that the electromagnetic action of moving electricity can be due entirely to the electromagnetic action of the displacement-currents in the dielectric: for in the case of a plane moving parallel to itself there are none of these displacement-currents, and yet that is the only case that has been experimentally verified.

To show that my assumption leads to equations satisfying the condition, and leading to practically the same results as Mr. Thomson's, does not require much work.

Consider an elementary volume $dx\,ds\,.\cos\theta$, where ds is an element of the surface of the sphere, and θ the angle the radius makes with x. The displacement in this volume is $=\delta$, the superficial density, while after the time dt it is zero on my assumption.

* From the Scientific Proceedings of the Royal Dublin Society, vol. iii. part 4, having been read November 21, 1881. Communicated by the Author.

Hence calling the displacement D, we have

$$\frac{d\mathrm{D}}{dt}dt = -\delta \,.\, dx\, ds \,.\, \cos\theta.$$

Hence

$$\dot{\mathrm{D}} = -\delta \,.\, \frac{dx}{dt} \,.\, ds \,.\, \cos\theta.$$

Now $\frac{dx}{dt} = p$, the velocity of the sphere which is supposed to be moving along x. Hence the components of $\dot{\mathrm{D}}$ are $\dot{f}, \dot{g}, \dot{h}$; and observing that $ds = a^2\, d\mu\, d\phi$, where $\cos\theta = \mu$, and a is the radius of the sphere, while $4\pi a^2 . \delta = e$, the total quantity of electricity on the sphere,

$$\dot{f}\, dx\, dy\, dz = -\frac{ep}{4\pi}\mu^2\, d\mu\, d\phi,$$

$$\dot{g}\, dx\, dy\, dz = -\frac{ep}{4\pi}\mu\cos\phi\, d\mu\, d\phi,$$

$$\dot{h}\, dx\, dy\, dz = -\frac{ep}{4\pi}\mu\sin\phi\, d\mu\, d\phi.$$

Hence the components of the electromagnetic potential are at a point at a distance

$$\mathrm{F_s} = -\frac{ep}{4\pi}\iint\frac{\mu^2}{r}\, d\mu\, d\phi,$$

$$\mathrm{G_s} = -\frac{ep}{4\pi}\iint\frac{\mu\cos\phi}{r}\, d\mu\, d\phi,$$

$$\mathrm{H} = -\frac{ep}{4\pi}\iint\frac{\mu\sin\phi}{r}\, d\mu\, d\phi.$$

These are the components of the electromagnetic potential due to this superficial change of displacement that I have assumed. When integrated over the surface of the sphere, they give at a point distant R from its centre, and whose polar angles are α and ϵ,

$$\mathrm{F_s} = -\frac{ep}{3\mathrm{R}} - \frac{epa^2}{5\mathrm{R}^3}(\cos^2\alpha - \tfrac{1}{3}),$$

$$\mathrm{G_s} = -\frac{epa^2}{5\mathrm{R}^3}\cos\alpha \,.\, \sin\alpha \,.\, \cos\epsilon,$$

$$\mathrm{H_s} = -\frac{epa^2}{5\mathrm{R}^3}\cos\alpha \,.\, \sin\alpha \,.\, \sin\epsilon.$$

If we add to these the components calculated by Mr. Thomson as due to the external displacement-currents, and given by him (*loc. cit.* p. 233), namely

$$\mathrm{F_e} = \frac{ep}{10\mathrm{R}^3}(5\mathrm{R}^2 - 3a^2)(\cos^2\alpha - \tfrac{1}{3}),$$

$$\mathrm{G_e} = \frac{ep}{10\mathrm{R}^3}(5\mathrm{R}^2 - 3a^2)\cos\alpha\sin\alpha\cos\epsilon,$$

$$\mathrm{H_e} = \frac{ep}{10\mathrm{R}^3}(5\mathrm{R}^2 - 3a^2)\cos\alpha\sin\alpha\cos\epsilon.$$

we get, as the resultant values of the components produced by the displacements assumed,

$$F_d = -\frac{ep}{3R} + \frac{ep}{2R^3}(R^2-a^2)(\cos^2\alpha - 3),$$

$$G_d = \frac{ep}{2R^3}(R^2-a^2)\cos\alpha\sin\alpha\cos\epsilon,$$

$$H_d = \frac{ep}{2R^3}(R^2-a^2)\cos\alpha\sin\alpha\sin\epsilon,$$

which, however, do not satisfy the condition

$$\frac{dF_d}{dx} + \frac{dG_d}{dy} + \frac{dH_d}{dz} = 0.$$

Now it is very easy to calculate the action of the superficial moving electricity if it be assumed to act like an electric current. Each element of the surface will act as if it had an electric $\dot{e}\,.\,p$ on it: and the x components of the electromagnetic potential will evidently be the same as the electrostatic potential, while the y and z components will vanish. Hence

$$F_e = \frac{\mu ep}{R}, \quad G_e = H_e = 0.$$

My assumption is that the complete components are

$$F = F_e + F_d, \quad G = G_e + G_d, \quad H = H_e + H_d;$$

and it is easy to see that they can be put into the form

$$F = \frac{\mu ep}{R} + \frac{\mu ep}{6}\left\{(R^2-a^2)\frac{d^2}{dx^2}\cdot\frac{1}{R} - \frac{2}{R}\right\},$$

$$G = \frac{\mu ep}{6}(R^2-a^2)\frac{d^2}{dx\,dy}\cdot\frac{1}{R},$$

$$H = \frac{\mu ep}{6}(R^2-a^2)\frac{d^2}{dx\,dz}\cdot\frac{1}{R};$$

and these satisfy the condition

$$\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} = 0.$$

These may be further simplified by assuming a function

$$\chi = -\frac{\mu ep}{2}\left(x + a^2\frac{d}{dx}\right)\frac{1}{R},$$

$$F = -\frac{\mu ep}{R} + \frac{d\chi}{dx},$$

$$G = \frac{d\chi}{dy},$$

$$H = \frac{d\chi}{dz}.$$

We hence easily see that, in the general case where the sphere is moving with component velocities p, q, r, we must take

$$\chi = -\frac{\mu e}{z}\left\{px+qy+rz+a^2\left(p\frac{d}{dx}+q\frac{d}{dy}+r\frac{d}{dz}\right)\right\}\frac{1}{\mathrm{R}};$$

or, calling

$$p\frac{d}{dx}+q\frac{d}{dy}+r\frac{d}{dz}=\frac{d}{d\theta},$$

and observing that

$$p\frac{x}{\mathrm{R}}+q\frac{y}{\mathrm{R}}+r\frac{z}{\mathrm{R}}=\dot{\mathrm{R}},$$

we have

$$\chi = -\frac{\mu e}{2}\left(\dot{\mathrm{R}}+a^2\frac{d}{d\theta}\cdot\frac{1}{\mathrm{R}}\right);$$

and then we have

$$\mathrm{F}=\frac{\mu e p}{\mathrm{R}}+\frac{d\chi}{dx},$$

$$\mathrm{G}=\frac{\mu e q}{\mathrm{R}}+\frac{d\chi}{dy},$$

$$\mathrm{H}=\frac{\mu e r}{\mathrm{R}}+\frac{d\chi}{dz}.$$

From these it appears at once that the magnetic effect of the displacement-currents is *nil*. For the components of the magnetic forces α, β, γ are

$$\alpha=\frac{d\mathrm{G}}{dz}-\frac{d\mathrm{H}}{dy},$$

$$\beta=\frac{d\mathrm{H}}{dx}-\frac{d\mathrm{F}}{dz},$$

$$\gamma=\frac{d\mathrm{F}}{dy}-\frac{d\mathrm{G}}{dx};$$

and χ disappears from this, and there only remains the magnetic action due to the current that I have assumed to represent the moving superficial electricity. The effect of this is obviously the same as if the whole quantity of electricity were moving at its centre: and this is the same as Mr. Thomson's result (*l. c.* p. 236).

It is to be observed that Mr. Thomson has intentionally omitted the self-induction of these displacement-currents on one another; and it may legitimately be omitted when the motion is comparatively slow; but a complete solution of the question would be most interesting.

It may be worth while remarking, that no effect except light has ever yet been traced to the displacement-currents assumed by Maxwell in order to be able to assume all currents to flow in closed circuits. It has not, as far as I am aware, been ever actually demonstrated that open circuits, such as Leyden-jar discharges, produce exactly the same effects as closed circuits; and until some such effect of displacement-currents is observed, the whole theory of them will be open to question.

ON A VIBRATORY MOTION AT THE ORIGIN OF A JET OF VAPOUR.

BY M. TH. VAUTIER.

In the course of some researches which I have commenced upon the flow of gases and vapours, I have observed a singular phenomenon, which was first signalized in 1826, by an engineer of Fouchambault's iron-works:—When a jet of gas or vapour flows, under pressure, through an orifice, if a plate be placed normal to the axis of the jet, at a certain height, it is repelled; but when the plate is lowered parallel to itself, it is attracted, and spontaneously supports itself at about 0·2 millim. from the orifice, oscillating about a position of equilibrium and emitting a sound.

I have succeeded, by means of a very simple arrangement, on fixing some plates in that situation, in making them give forth sounds high, intense, as prolonged as I pleased, and directly register those vibrations. I have obtained in the tracings, undulations of remarkable regularity and amplitude.

I have directly registered the vibrations, thus kept up by a jet of vapour, of a plate giving a note close to the $la^{\overline{3}}_{6}=7250$ single vibrations per second. An electrodiapason simultaneously registered its vibrations. Pressure in the boiler, 4·5 atm.; diameter of the orifice, 2·7 millim. Diameter of the plate, 6 millim.; thickness, 1·5; distance from the orifice, 0·2; amplitude of the vibrations, 0·7. Thus, then, we have a chronograph directly registering $\frac{1}{7250}$ of a second.

The sound which I get is intenser than that of the diapasons which give the same note, and of which it has not, up to the present, been possible to sustain the motion electrically in a practical manner, and, consequently, to register it continuously. The apparatus which I have prepared traces, as long as I please, in sharp outline, regular vibrations of sufficient amplitude to be subdivided in the usual manner. This subdivision is facilitated by the fineness of the tracing, which is obtained by means of very sharp styles, which, pressed lightly upon sheets of mica either smoked or not, leave a thin stroke engraved upon those surfaces.

Hitherto, at least to my knowledge, it has been found possible at most to register, directly and continuously, the thousandth part of a second, which is afterwards divided. The apparatus which I use registers directly the seven-thousandth of a second; and the tracing, very thin, favours the usual divisions. The results obtained by me quite recently permit me to expect to register sounds of still higher pitch. I purpose to apply this chronograph to the measurement of some rapid phenomena.—*Comptes Rendus de l'Académie des Sciences*, March 6, 1882, t. xciv. p. 642.

ON THE COMPRESSIBILITY OF GASES. BY M. E. SARRAU.

1. In a memoir on the compressibility and expansion of carbonic acid, very important when regarded from the thermodynamic point of view, M. Clausius has proposed for this gas the following relation between the pressure (p), the volume (v), and the absolute temperature (T):—

$$p=\frac{RT}{v-\alpha}-\frac{K}{T(v+\beta)}\cdot\cdot\cdot\cdot\cdot\cdot\cdot\quad(1)$$

The eminent physicist has verified, with the aid of Andrews's experiments, that this relation represents exactly the transformations of gaseous or liquid carbonic acid; and he thinks that the same formula, with suitable values of the constants R, K, α, β, must be correct for all gases.

Among the consequences resulting from this formula must be noticed those concerning the *critical point*. Indeed it follows, from the interpretation given by M. Clausius of the results of his formula, that when the critical point is reached the function p satisfies the two conditions $\frac{dp}{dv}=0$, $\frac{d^2p}{dv^2}=0$. By combining these conditions with the relation (1) we get three equations, from which we deduce the following values of v, T, p corresponding to the critical point:—

$$v_c=3\alpha+2\beta\,;\quad T_c=\left(\frac{2}{3}\right)^{\frac{3}{2}}\left(\frac{K}{R}\right)^{\frac{1}{2}}(\alpha+\beta)^{-\frac{1}{2}}\,;\quad p_c=6^{-\frac{3}{2}}(KR)^{\frac{1}{2}}(\alpha+\beta)^{-\frac{3}{2}}\quad(2)$$

2. I proposed to myself to verify M. Clausius's relation for other gases than carbonic acid, making use of the extensive experiments of M. Amagat, in which the temperature varied from 15° to 100°, and the pressure from 25 to 320 metres of mercury.

The numerical determination of the coefficients being not without difficulties, it is not needless to indicate the course which I have uniformly pursued to accomplish it. Let p and p' be the pressures corresponding to one and the same value v at two different temperatures T, T'; we have the two relations

$$p=\frac{RT}{v-\alpha}-\frac{K}{T(v+\beta)^2},\qquad p'=\frac{RT'}{v-\alpha}-\frac{K}{T'(v+\beta)^2}.$$

Putting

$$x=\frac{T'^2-T^2}{p'T'-pT},\qquad y=\sqrt{\frac{T'^2-T^2}{TT'(p'T-pT')}},$$

we deduce from the above two equations, by successively eliminating K and R,

$$\frac{v-\alpha}{R}=x,\quad \frac{v+\beta}{\sqrt{K}}=y.\quad\ldots\quad(3)$$

Therefore knowing two corresponding values of v and x suffices for obtaining R and α, and two corresponding values of v and y for obtaining K and β; but if we take a superabundant number of simultaneous values (v, x) and (v, y), we can verify the exactness of the relations (3) and make all the equations contribute to the calculation of the coefficients. M. Amagat's experiments do not give directly the pressures which at different temperatures correspond to the same volume: I obtained them by interpolation. The mode of calculation adopted employs for the values of the coefficients the units employed by M. Amagat. The pressures are expressed in metres of mercury, or, dividing by 0·760, in atmospheres. The unit of volume is not specified; but without knowing this we can determine the values possessed by the constants by taking for unit the volume of the gas under the atmospheric pressure and at zero. In fact, in consequence of the change of unit, v must be re-

placed by $\frac{v}{\epsilon}$, ϵ being a conveniently chosen factor. Relation (1) thus becomes :—

$$p = \frac{R\epsilon T}{v - \epsilon\alpha} - \frac{K\epsilon^2}{T(v+\epsilon\beta)^2}.$$

The volume of the gas under the normal pressure at zero being taken as unit, it follows from the properties of perfect gases that $R\epsilon$ becomes $= \frac{1}{273}$. The factor ϵ is therefore determined.

3. *The Compressibility and Critical Point of Oxygen.*—The calculation, applied to two series of experiments on oxygen *, leads to the following values :—

$$\log R = 1{\cdot}23676\,; \quad \log K = 7{\cdot}08424\,; \quad \alpha = 4{\cdot}19\,; \quad \beta = 3{\cdot}23.$$

If with these coefficients, and employing measured volumes, we calculate the corresponding pressures, we find the numbers in the following Table, from which the accuracy of the formula can be appreciated :—

Temperature 14°·7.				Temperature 100°·2.			
	Pressure.				Pressure.		
Volume.	Measured.	Calculated.	Difference.	Volume.	Measured.	Calculated.	Difference.
	atm.	atm.	atm.		atm.	atm.	atm.
40·90	113·4	113·6	−0·2	45·57	141·1	141·9	−0·8
32·78	141·1	141·1	0·0	35·97	181·1	181·4	−0·3
25·67	181·1	180·5	+0·6	27·52	240·9	241·5	−0·6
19·56	240·9	241·7	−0·8	20·20	342·1	342·8	−0·7
14·59	342·1	344·3	−2·2	17·16	418·9	418·1	+0·8
12·74	418·9	414·8	+4·1				

Adopting for the unit of pressure the atmospheric pressure, and for the unit of volume the volume of the gas at zero under the pressure of the atmosphere, the four constants of the formula are, for oxygen,

$$R = 0{\cdot}003663, \quad K = 0{\cdot}5475, \quad \alpha = 0{\cdot}000890, \quad \beta = 0{\cdot}000686\,;$$

and for the critical point the values of the variables, calculated by the formulæ (2), are

$$v_c = 0{\cdot}004042, \quad t_c = -105{\cdot}4, \quad p_c = 48{\cdot}7 \text{ atm.}$$

These numbers lead to a remarkable consequence. M. Raoul Pictet measured the density of solid oxygen at the temperature of about −110°, under the pressure of 470 atmospheres. If we seek, by means of the formula, the corresponding value of v, we find 0·00136. Applying this result to the unit of weight, and taking the litre as unit, we find $0{\cdot}00136 \times 697{\cdot}2 = 0{\cdot}948$. The density, therefore, is very close to unity, in accordance with the valuation of M. Raoul Pictet. In an early communication I will make known the results relative to the other gases investigated by M. Amagat. —*Comptes Rendus de l'Académie des Sciences*, March 6, 1882, t. xciv. pp. 639–642.

* *Comptes Rendus*, Nov. 15, 1880.

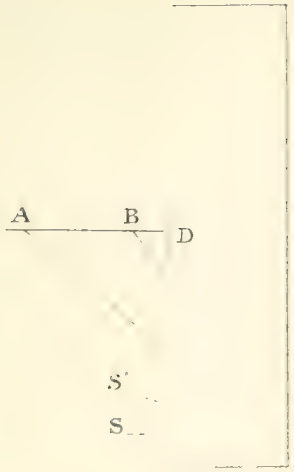
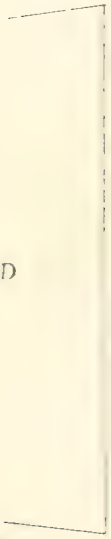


Fig. 6.
Scale $\frac{1}{2}$



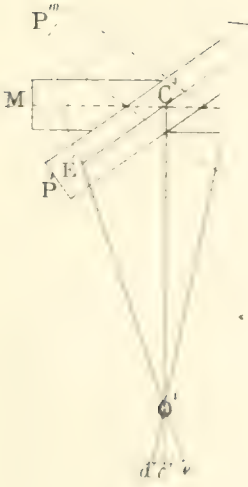


Fig. 7.

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MAY 1882.

XXXVI. *Physiological Perspective.*

By W. LE CONTE STEVENS*.

[Plate VIII. figs. 1–5.]

1. *Brewster's Experiment.*

SIR DAVID BREWSTER, in his essay 'On the Knowledge of Distance given by Binocular Vision' (1844), and in his subsequent volume on the Stereoscope (1856), describes the binocular union, by optic convergence, of similar pictures regularly recurring on large surfaces. The result of viewing, by cross vision, flowers twelve inches apart, on a papered wall at three feet distance, he describes partially as follows†:—"The whole papered wall, with all its flowers, will be seen suspended in the air at the distance of six inches from the observer. At first the observer does not decide upon the distance of the suspended wall from himself. It generally advances slowly to its new position; and when it has taken its place it has a very singular character. The surface of it seems slightly curved."

The geometric explanation of the position of the phantom wall, as given by Brewster‡, is easily understood. Let R and L (fig. 1) be the optic centres of the right and left eyes respectively, and A, B, C, A′, B′, C′, equidistant points on the wall. If the right eye is directed to A and the left to A′, the intersection of visual lines is at *a*. The points *b* and *c* are determined in like manner; and if the interocular line

* Communicated by the Author.

† Brewster on the Stereoscope, London, 1856, p. 91.

‡ *Ibid.* p. 95.

R L be parallel to the wall, it follows that the phantom surface must be a plane S S′ parallel to the wall. This explanation fails to account for the apparent curvature S″ S‴; and on actually trying the experiment it will be found that the convexity is very slight unless the optic convergence be strong.

2. *The Binocular Eye.*

In examining this phenomenon it is indispensable that the observer distinguish between subjective effects and objective realities, between a sensation and its exciting cause. In seeing with two eyes, the sensation is the same as if both were fused into a single binocular eye, midway between them, whose optic centre is the point of origin in all estimates of direction and distance. The two visual lines are subjectively combined into a single line, extending from the yellow spot of the binocular eye as far as the external point to which the observer mentally refers the combined retinal picture. There can hence be no perceived intersection of visual lines as assumed by Brewster. The recognition of optic convergence or divergence is through the muscular sense; and the localization of objects in the field of view is the interpretation of a complex sensation, not a simple geometric determination. I have recently devised a refracting stereoscope *, by means of which the same stereograph, within a few seconds of time, may be viewed successively by optic divergence, with natural relief, and by slightly greater convergence with reversion of relief. The binocular image is seen either alone or accompanied by a pair of monocular images, as may be preferred, and varies in apparent distance and size according to the degree of muscular strain attendant upon binocular vision in each case. The appearance of monocular images can be easily secured with the ordinary lenticular stereoscope, by removing the longitudinal screen while the relation between the two visual lines is kept unchanged. The subjective effect is that the binocular image, in full relief, remains apparently single and directly in front of the binocular eye, while on each side of it a separate monocular image is perceived by indirect vision. This is true for optic divergence as well as convergence; and the recognition of the subjective union of the eyes must underlie any attempt to explain the phenomena of binocular vision.

3. *Gradation in Retinal Fusion.*

However mistaken Brewster may have been in his theory

* American Journal of Science, March 1882.

of visual triangulation, he was right in laying stress upon the successive changes of relation between the visual lines in attaining the illusion of binocular relief. In many cases this is indispensable to perfect success; but it is not a fully sufficient explanation. When the stereoscopic displacement on the conjugate pictures is small, the perception of relief is instantaneous, as has been abundantly established by the experiments of Dove, Helmholtz, Le Conte, and others, who illuminated the stereograph with the electric spark. But when the stereoscopic displacement is large, if a pair of properly constructed diagrams be employed, the superposed external images may be made apparently to spring into full relief only by play of the eyes, and to flatten out into a confused network of intersecting and partially coalescent lines by making the gaze rigidly fixed. The perception of relief is then confined to those parts where the displacement is small. The fact that this residual relief is always more or less perceptible, indicates that the generally accepted theory of corresponding retinal points cannot be interpreted mathematically. The so-called *minimum visibile* is not a point, but corresponds to a retinal area whose diameter is variously estimated from ·003 mm. to ·005 mm. It is impossible to perceive separately an object whose retinal picture is smaller than this. But it may be quite possible for the *quality* of a retinal sensation to be modified while the additional impression, apart from that which it modifies, would be imperceptible. This indeed is quite comparable to our perception of musical quality by the ear. Through the auditory nerve it has always been possible to distinguish between notes of the same fundamental pitch from different sources. It was reserved for Hemholtz to analyze by instrumental aid what had previously eluded analysis by the unaided ear, and show that minute modifications upon the sensations that had been regarded as identical were due to additional impressions that were separately imperceptible without resonators. The sensible coalescence between a fundamental and the overtones which give it character, or between the sensations produced by two well-trained unisonant voices, each with its attendant overtones which make them produce slightly dissimilar sound-images, is in no way more remarkable than the sensible coalescence of slightly dissimilar light-images, with the production of new and recognizable quality. Whether the complete analysis of such complex images will ever be made is for the future to develop.

Wheatstone therefore was correct in his theory of mental fusion of retinal images, so far as this goes; but it does not

go far enough. Brewster was right in considering it inadequate, and emphasizing the fact that while the foreground is seen single the background must be double ; but the qualifying statement is necessary that this is perceptible only when the stereoscopic displacement is considerable. Returning to the comparison with sound-perceptions, two strings of different material may be mistuned so as to produce beats ; but by successive alterations of tension they may be made sensibly unisonant, with alternately higher and lower pitch, yet the resultant quality in each case is perceived to be different from that of the sound from either string alone. In like manner, with retinal sensations, we may pass from sensible coalescence with distinctions of relief in the foreground, while the background is double, to similar coalescence in the background, leaving the foreground double. The simultaneous relaxation of muscular tension produces the impression of greater distance of the new points thus combined by modified fusion ; for in normal binocular vision the internal rectus and ciliary muscles become relaxed in withdrawing the attention from a very near body and bestowing it upon one that is distant.

4. *Physiological and Physical Perspective.*

Wheatstone and Brewster thus separately emphasized the importance of phenomena that are complementary, not contradictory. Wheatstone's theory of binocular perspective was based mainly upon physiological considerations; and he indicates that he regards convergence of visual lines as invariably accomplished in all binocular vision, though he cites some exceptional cases in which it seems not to determine the apparent position of the object viewed. Brewster's theory was distinctively geometric ; he considered variation of relation between visual lines to be important because the successive points in the field of view were to be *determined* by their intersection ; and hence he concludes that " we see distance."* Subsequent investigation now proves that the localization of what is viewed in the stereoscope is in no way dependent upon such intersection, but that binocularly we simply estimate distance as in monocular vision. The estimate is indeed for short distances far more definite, in both normal and stereoscopic vision, than when a single eye is employed, because the muscles of the two eyeballs are called into associated action, independently of the sensible coalescence of slightly dissimilar retinal images in the binocular eye. Certain muscular contractions are habitually associated with certain motions of the body viewed, or certain relations between its different parts.

* Brewster on the Stereoscope, p. 50.

If the binocular examination of the stereograph necessitate these contractions, the corresponding judgment which follows is dependent upon the observer's ability to interpret nerve-impressions, muscular as well as retinal, and not on the mathematical relation between his visual lines. For the apparent relation between objects in the binocular field of view, as influenced by the temporary condition of the muscles of the eyes, I have proposed the name of physiological perspective *, in contrast with what is suggested by the ordinary elements, whose total effect may be called physical perspective. The judgment suggested by the former may either coincide or conflict with that suggested by the latter. The result is either enhancement, or partial reversion, or total reversion, according to the arrangement of conditions. The convexity of the spectral wall which Brewster observed, and which is inexplicable on his theory of visual triangulation, is an illustration of both physiological and physical perspective; and by varying the conditions reversion is easily produced. In discussing it, any reference to corresponding retinal points must be understood in the modified sense already explained, and not mathematically.

5. *A New Mode of Stereoscopy.*

Brewster's experiment may be performed more satisfactorily by constructing on cardboard a pair of perfectly similar figures, each consisting of a series of concentric circles. Combining the images of these by bringing the card quite near and crossing the visual lines at an angle of at least 60°, the binocular image appears suspended in mid air, diminished in size, more especially so in a direction parallel to the interocular line, but convex toward the observer, to whom the appearance is that of a miniature elliptic buckler. The experiment is quite straining upon the muscles of the eyes. A far easier method is to cut the card across at the middle, and incline the two halves so that at the edges in contact the two faces shall form a dihedral angle of 120°, or less, opening toward the observer. If the interval between the two centres be equal to the observer's interocular distance, or slightly exceed this, he may easily secure binocular combination with the visual lines parallel or divergent; and the convex shield is seen, larger than in the previous experiment, and sensibly a little more distant than when either picture is viewed separately as in ordinary vision. If there be difficulty in relaxing the muscles of the eyes sufficiently, the cards may be properly held in front of the semi-lenses of an ordinary

* American Journal of Science, Nov. 1881, p. 361.

stereoscope, though vision with the unaided eyes is preferable. The visual result is thus obtained without strain, and by optic parallelism or divergence, either of which conditions makes Brewster's theory of intersection inapplicable. By reversing the inclination of the cards to the parallel visual lines, the apparent convexity is changed into concavity. We have thus stereoscopy obtained from a pair of perfectly similar conjugate pictures, and capable of reversion at will without conscious motion of the eyes or removal of the stereograph.

This is, so far as I have been able to learn, a new phenomenon in binocular vision. Brewster's unexplained observation had apparently passed into oblivion on account of the exceeding strain upon the eyes which it involved. The curvature of the phantom wall, in a plane passing longitudinally through the observer's body, has lately been rediscovered by Professor Joseph Le Conte, and shortly afterwards its curvature in all directions by myself. On working out the explanation, which would not have been possible except by rejecting Brewster's theory of triangulation, I at once devised a simple attachment for my adjustable stereoscope, capable of use however with any such instrument, if so arranged that the stereograph may be rested upon a cross bar, instead of sliding within a box. Let MN (fig. 2) be such a cross bar, in front of the eyes whose optic centres are at O and O′. A pair of extra short bars PQ and P′Q′ are pivoted at C and C′, over which rest the centres of two similar series of concentric circles whose horizontal diameters are included between E and D, E′ and D′, respectively, the plane of each card being perpendicular to the page. The central visual lines CO and C′O′ are parallel, but for obvious reasons made very short in the diagram. The retinal projections of ECD and E′C′D′ are *ecd* and *e′c′d′* respectively. Because the triangles EOD and E′O′D′ are oblique, their medians divide the angles at O and O′ unequally; hence $dc > ce$ and $d'c' < c'e'$. The retinal images in the two eyes are hence dissimilar; and this dissimilarity may be made so great by increasing the angles NCD and MC′E′, that the binocular image becomes indistinct if the eyes are not made to play rapidly over the picture. If the attention be momentarily withdrawn from C and C′ to D and D′, the visual lines become divergent to an extent measured by the difference of the angles dOe and $d'O'c'$. The associated contraction of the external rectus muscles which this necessitates, at once produces the sensation that habitually accompanies recession of the object binocularly viewed. The same is true if the attention be restored to C and C′, and then given to E and

E′. The binocular image of the horizontal diameters must hence be perceived as a curve, convex toward the observer.

Each circle, obliquely viewed, must be seen approximately as an ellipse, the ratio of whose axes is readily calculable if the angle of inclination be known. But the retinal ellipses are no longer concentric (figs. 3 and 4), the extent of retinal displacement depending on the extent of minor axis in each. If the successive vertices be connected, we have two curved lines, A C B and A′C′B′. If these be binocularly combined and externally projected, since C′C is less than A′A and B′B, optic divergence becomes necessary in transferring the attention from C and C′ to A and A′ or B and B′. The binocular image of the vertical diameters must hence be perceived as a curve convex toward the observer.

Let F and G (fig. 3) be points symmetrically situated with regard to the vertical diameter, and hence equidistant from D and E respectively. When the card is revolved, as in fig. 2, the distance O E exceeds O D, and hence the visual angle subtended by E G is less than that subtended by D F. Every ellipse therefore is distorted; but since the distortion is equal and opposite on the two retinas, it is perfectly corrected in the binocular combination of each pair of curves. To each eye separately the effect is the same as if every major axis were bent, and every point of each curve, above and below the horizontal axis, were correspondingly displaced: F and G′ are elevated, F′ and G depressed; hence F and F′ differ in retinal latitude as well as longitude, but are nevertheless binocularly combined. This confirms the views of Wheatstone in opposition to those of Brewster.

If a pair of small circles whose vertical diameters are *a b* and *a′b′* be viewed above the large circles, the visual lines directed to their centres are similarly oblique to their vertical diameters. They are therefore retinally projected as approximate ellipses; and when these are thence externally projected, their upper vertices are further apart, and their lower vertices nearer together, than their centres. The binocular combination is hence an ellipse whose plane is oblique, the upper vertex being further from, and the lower vertex nearer to, the observer. A pair of small circles below the large ones are binocularly combined with opposite obliquity.

No explanation is now needed to show that if the planes of the cards be revolved into the positions P″ Q″ and P‴ Q‴ (fig. 2), the binocular combination of the concentric circles must present a concave surface, and the obliquity of the plane of each pair of conjugate small circles, when binocularly viewed, must be reversed in sense.

Brewster's remark about the phantom wall, that "it generally advances slowly to its new position," is now easily understood. At that time the nature of focal adjustment by action of the ciliary muscle in each eye was not known. When E and E′ (fig. 2) are binocularly viewed, since E O exceeds E′O′, there must be dissociation between the two focal adjustments which are generally adapted to the same distance. To this must be added the necessary dissociation between axial and focal adjustments, the former being for an infinite distance, the latter for the distance O C, when C and C′ are binocularly viewed. To untrained eyes this unusual muscular action is not always easy, and cannot be secured without effort that may consume several seconds of time. The result is momentary confusion ; and many persons are hence unable to decide at first whether the surface appears convex or concave. Despite these difficulties, the image soon becomes clearly defined when the experiment is performed with axial parallelism. With strong axial convergence, as in Brewster's experiment, the dissociation is far more difficult, on account of the extreme muscular tension made necessary.

6. *The Theory of Associated Muscular Action.*

The theory of visual triangulation seems to have been first put forth in 1604 by Kepler*, who said that the distance between the two eyes is the base which we employ in measuring the distance of objects, that in so doing we learn to estimate distances with a single eye, and hence that the magnitude of a heavenly body as perceived in the eye would serve as a base for distances relatively slight. Brewster†, in opposition to Berkeley‡, elaborated the idea still further, making it the foundation of his theory of the stereoscope, which has kept its place in our textbooks of Physics to the present day. The retention of this theory, in the face of facts to the contrary, is explicable only in consideration of the circumstances attendant upon normal binocular vision, and the great authority attached to Brewster's views on account of his discoveries as a physicist. Probably neither he nor his followers undertook to ascertain whether the limit of optic parallelism was ever passed in the actual use of the stereoscope. In 1861, two Germans, Rollet and Becker§, published a mode of securing binocular fusion of similar images with the visual lines slightly diver-

* *Paralipomena*, 1604, pp. 62–66.

† Brewster on the Stereoscope, p. 50.

‡ 'An Essay towards a New Theory of Vision': Dublin, 1709.

§ *Wiener Sitzungsberichte*, May 10, 1861, xliii. ; or Helmholtz, *Optique Physiologique*, p. 827.

gent, the possibility of this having been already mentioned by Burckhardt*, but with no reference to the possibility of stereoscopy from these similar pictures. In 1862, Brewster's theory was criticised by Professor C. F. Himes, of Pennsylvania, who described his experiments in stereoscopy from dissimilar pictures with optic divergence, and called attention to the necessity of modifying the current theory†. Helmholtz related experiments in optic divergence‡, but, unfortunately, did not enter fully into an analysis of the visual results. My own attention was drawn to this subject by the discovery of my power to secure stereoscopy with the unaided eyes by voluntary optic divergence. In a paper read before the New-York Academy of Sciences on the 6th of June 1881, I rejected Brewster's theory; and the phenomena which he had explained by visual triangulation I referred to the associated action of the muscles of the eyes, applying this to both convergence and divergence. It has been very gratifying, therefore, to find my views confirmed by Brigade-Surgeon Tyler Oughton, A.M.D., in two able articles published in the London 'Lancet,' of October 22 and December 31, 1881. These articles were quite independent of my own, each writer having been unknown to the other. In 1877 Mr. Oughton had referred certain phenomena of "erect vision" to the sensation of muscular contraction. My experiments in stereoscopy with perfectly similar pictures, by optic convergence, parallelism, or divergence, with direct or inverse relief at will—experiments capable of ready accomplishment by any one with untrained eyes—still further show that there is no longer any room for the application of geometry to the physiology of vision, beautiful and tempting as may be the theory of visual triangulation.

It is but due to add that the theory of associated muscular action in relation to the stereoscope was virtually stated by Professor Huxley§ in 1868, and in such a way as quite plainly to indicate its applicability to the phenomena of optic divergence. That he did not elaborate it in refutation of Brewster's theory was probably due to the fact that the fallacy and wide spread of this theory had not been brought specially to his attention.

* *Verhandlung. d. naturforsch. Ges. zu Basel*, i. p. 145; or Helmholtz, *Opt. Phys.* p. 827.

† American Journal of Photography, September 1, 1862; also British Journal of Photography, 1864.

‡ *Optique Physiologique*, pp. 616 & 828; also 'Popular Lectures on Scientific Subjects,' p. 307 (transl. 1873).

§ 'Elementary Physiology,' p. 286 (Macmillan and Co.).

7. *Voluntary Control of the Focal Adjustment.*

Experiments in stereoscopy sufficiently show the office of the rectus muscles in modifying the interpretation put upon retinal sensations. If the visual lines be kept as nearly parallel as possible, and the observer's distance from an object regarded be kept constant, the retinal sensation may still be modified by direct voluntary contraction of the ciliary muscle. It is accompanied by contraction of the pupillary opening; and this can be approximately measured by an assistant. The experiment is straining, but can be performed with very little associated contraction of the rectus muscles. Within two seconds of time the pupillary opening of my eye has thus been often changed to one tenth of its previous area, the contraction of the iris quickly following that of the ciliary muscle. The visual effect is first a slight enlargement of the retinal image as it grows dim; this is due to the production of diffusion-circles, which encroach upon the surrounding retinal area as well as upon the image. The object then apparently diminishes in size to a marked extent. As its absolute distance and diameter, however, are constant, the retinal image cannot be diminished except by encroachment of diffusion-circles. The apparent diminution is far greater than can be explained on this ground, and can only be referred to an error in judgment induced by abnormal muscular conditions that have been voluntarily imposed. The experiment has been many times repeated; and the uniformity of results leaves no room to attribute the illusion to accidental circumstances. The apparent distance of the object to which the gaze is directed becomes indeterminate. Ciliary contraction suggests the idea of its nearness; but the dimness resulting from imperfect focalization suggests its remoteness. The impression of its contraction in area is unmistakable and striking. Since the contractions of the ciliary and internal rectus muscles are usually associated in normal vision, the considerations just expressed explain why so acute an observer as Wheatstone* should have noticed the apparent decrease in size of the image when strong convergence of visual lines was produced by pulling forward the arms of his reflecting stereoscope, but failed to note the corresponding variation in apparent distance; though he observes that it seems changed in position, but does not say whether the change is that of increase or decrease of distance.

8. *Effect of varying the Locality of the Image.*

When strong muscular tension is voluntarily induced, the modification of retinal sensation seems to be greatest at the

* Philosophical Magazine, 1852, pp. 507 & 508.

yellow spot, and to diminish toward the margins. Let a pair of circles, A and A′ (fig. 5), be drawn, a few inches apart, and below A a third circle, A″, equal to each of the others. Bringing the face as near to the page as is consistent with distinct vision, let the right eye be directed to A′ and the left to A. A diminished elliptic binocular image is seen directly in front, apparently in mid air; on each side of which is a monocular image, sensibly circular, or but slightly elliptic, distinctly larger than the binocular image, but smaller than either circle seen by normal vision. Even though distinct vision is attained under these conditions, the ciliary muscles are strongly contracted, consensually with the internal rectus muscles. On closing the right eye, relaxation follows, while the direction of the visual line for the eye remaining open is easily kept unchanged. The right monocular image disappears; the elliptic image apparently recedes and moves to the right, growing larger in doing so; but an interval of one or two seconds may elapse before normal monocular vision is restored, and now both circles have recovered their original size. The experiment is very easy.

This process can be reversed, and made as gradual as is desired, by slowly increasing the convergence of visual lines and carrying it beyond the point necessary for binocular combination. As soon as double vision is produced, the two interior images are perceived to be smaller than the exterior ones. They continue to grow steadily smaller and apparently nearer to the observer, until they coalesce in front; but the diminution continues after they have been made to pass each other. On the usually accepted theory, the apparent contraction is referred to the illusive estimate of diminished distance determined by the crossing of the visual lines at the moment the binocular combination is secured. But the falsity of this is additionally shown by regarding the circle A″ while A and A′ are binocularly combined. Although necessarily seen only monocularly with each eye, the left-eye image appears directly under the binocular image and of the same size, being much smaller than the right-eye image, which appears under the corresponding one of A.

9. *Physiology of Retinal Sensation.*

These experiments, taken in connexion with those on stereoscopy, conclusively show that not only is convergence of visual lines not necessary for binocular vision and for the localization of objects in the field of view, but that even change in such localization and in apparent magnitude can be secured without binocular fusion, as a consequence of muscular tension

alone. They indicate a closer relation between the nerves of the ciliary muscle and those of the retina than has been commonly supposed. Through the sympathetic system of nerves the mere action of light on the retina excites reflex muscular contraction of the iris. Without the agency of light, this may be effected by ciliary contraction alone. It is by no means impossible that through this same sympathetic system the impression conveyed to the brain may be modified by such muscular contraction, while the retinal area impressed remains unchanged. The interpretation is unconscious and independent of the will; it is dependent upon the excitement of several different nerves. The thing interpreted is the product of forces from without that operate in accordance with well-known mathematical laws. The velocity of propagation of a nerve-impression has been approximately determined by Helmholtz; but beyond this there has been little success in bringing the physiology of sensation within the domain of mathematics.

That the interpretation of retinal impressions should be more readily modified by association with muscular action when they are produced upon central rather than marginal portions of the retina, is entirely in accordance with the empiristic theory, which Helmholtz, Brücke, and other German physiologists have upheld. In all ordinary vision the attention is habitually given almost exclusively to objects corresponding to retinal images on or near the yellow spot. If any conditions are imposed that tend to induce misinterpretation of our sensations, it might be expected that the most sensitive parts of the retina would be most affected. It is impossible to prove that there is any peculiar necessary relation between the nature of a retinal sensation and the distance of the object pictured, or any innate power to recognize distance. Visual experience begins in infancy, long before the power to analyze sensations; and, in our interpretation of these, association is the chief determinant. Strong ciliary contraction is habitually, though unconsciously, associated with nearness of the object viewed; and this is pictured upon or very near the yellow spot of the retina. No one whose eyes are perfectly healthy has any consciousness of possessing a retina except in relation to external objects of sight, or a ciliary muscle except in relation to variation in distance of the point upon which attention is fixed through the medium of a simultaneous retinal sensation. We might expect, therefore, that unusual muscular conditions would cause misinterpretation regarding the whole field of view, but especially those parts which correspond to the central portion of the retina. This reasoning

applies to variations resulting from tension in the rectus muscles, external and internal, as well as from ciliary change.

10. *Binaural Audition and Binocular Vision.*

The discoveries in relation to binaural audition, made during the last few years independently by Professor Silvanus P. Thompson*, of England, and Professor Alfred M. Mayer†, of this country, are interesting, not only as additions to our knowledge of physiological acoustics, but also in connexion with the phenomena of physiological perspective. The localization of sounds has been found to be much affected by the mode in which the waves are conveyed to the separate ears. The same tone may be perceived as coming from the back of the head, or from the two sides, or from a point obliquely in front, while the position of the true source is unchanged—the perception being involuntary, while the conditions are adjusted at will. The judgment of distance by the ear is far more uncertain than by the eye, there being no criterion other than intensity of the sound perceived; but the perception of direction may be modified by imposing unusual physiological conditions, such as fatiguing one ear with a given tone and then listening to the same with both ears. For a fixed position of the eye, the perception of direction may be modified by changing the medium or pressing upon the eyeball; while that of distance is subject to variable physiological conditions, largely controllable by the will. Although the binaural estimate of direction and distance may be made less uncertain by properly adjusting the position of the head to the wave-fronts of the successive vibrations, no one has attempted to apply geometry to the localization of sounds through binaural audition. Brewster's attempt to do this for binocular vision was based upon two assumptions, each of which has been proven incorrect. The first was, that "the line of visible direction does not depend on the direction of the ray, but is also perpendicular to the retina"‡. The latter clause has been disproved by Helmholtz§. The second was, that convergence of visual lines is necessary in binocular vision, whether normal or stereoscopic‖. The falsity of this has been abundantly shown by recent experiments, in addition to those of Burckhardt, Himes, and others. A third fundamental statement by him was, that "distinct vision is obtained only on one point of the retina"¶. While this opinion is still supported by many, it is incapable

* Philosophical Magazine, Oct. 1877, Nov. 1878, and Nov. 1881.
† 'Researches in Acoustics.'
‡ Brewster on Optics, p. 246. § *Optique Physiologique*, p. 93.
‖ Brewster on the Stereoscope, pp. 90–106. ¶ *Ibid.* p. 50.

of strict mathematical interpretation. His theory of visual triangulation is inaccurate at best, and almost totally inapplicable in the very department to which it has been most applied —that of stereoscopic perspective.

40, W. 40th St., New York,
February 24, 1882.

XXXVII. *An Adjustable and Reversible Stereoscope.* *By* W. Le Conte Stevens*.

[Plate VIII. figs. 6 & 7.]

THE experiments which formed the basis of my recent papers on Physiological Optics† were, for the most part, made without the aid of any stereoscope, except the modification of Wheatstone's reflector, with which various values of the optic angle were measured. As few persons, however, have sufficient muscular control over the eyes to test my results, I devised an instrument which renders possible for untrained eyes quite a number of experiments which with the ordinary lenticular stereoscope would be either difficult or impossible.

The principal objects to be attained were as follows:—

I. To secure ready motion to the semi-lenses, so that they may be adapted in position for any pair of eyes, whatever may be the distance between the pupils, and for any stereograph, whatever the interval, within the usual limits, between corresponding points on the two pictures.

II. To secure the possibility of removing the semi-lenses at will, so that they may be reversed in relative position, or be replaced by prisms with their bases turned toward each other, so that, without uncomfortable muscular strain, the left picture may be viewed by the right eye and the right picture by the left eye simultaneously, thus producing reversion of relief in the binocular image, if desired.

III. To secure ready motion to the screen, so that to each eye either the whole stereograph, or its right half or its left half separately, may be visible.

IV. To secure the means of examining the binocular image either alone or attended by monocular images, so that the difference between the two kinds of vision may be noted.

V. To secure the means of using the same instrument either with glasses or for binocular combination of images by direct

* Communicated by the Author.

† Proceedings of the American Association for the Advancement of Science, Cincinnati Meeting, August 1881: Transactions of the New York Academy of Sciences, Oct. 24, 1881, and Feb. 12, 1882; American Journal of Science, November and December 1881; Philosophical Magazine, December 1881.

vision, and to reduce to the utmost the difficulty usually attendant upon stereoscopic vision by the latter method.

VI. To secure the means of producing stereoscopy from perfectly similar pictures, by making the retinal images of these dissimilar by oblique vision.

It gives me pleasure to acknowledge my indebtedness to Messrs. E. and H. T. Anthony and Co., of this city, for every facility that could be asked in testing such devices as presented themselves to my mind; and the form finally adopted gives entire satisfaction, the instrument being compact, inexpensive, and easily managed.

The semi-lenses, *l*, *l* (fig. 6), instead of being cemented to wood or brass, are gently pressed into a pair of boxes, in the opposite walls of which square openings have been cut to transmit the light from the stereograph, which is held on the cross bar, *a*, by pressure between a pair of brass springs, and may be adjusted to any desired position by sliding this along the longitudinal bar. A pair of adjusting-screws, *s*, *s*, come into contact with the bases of the semi-lenses, and are opposed by a pair of springs, against which the thin edges press. If the semi-lenses are thus pushed together as near as possible, the rays which enter the observer's eyes pass through the thicker parts, and hence are but little deviated. If they be as far apart as possible, the increase of deviation makes it convenient to employ a stereograph on which the interval between corresponding points is greater than on those generally prepared. This interval is ordinarily limited to about three inches. The focal length of the semi-lenses being seven inches, it has been found that a stereographic interval of four inches can be allowed without any very objectionable coloration due to lack of achromatism, the visual lines of the observer being now parallel or slightly divergent. This divergence may be increased to any extent that he is willing to endure, by turning the adjusting-screws, so as to press the semi-lenses closer together. Binocular fusion of images is thus retained; but the associated contraction of the external rectus muscles produces the illusion of slight recession of the object viewed and increased depth of relief. Similar effects may be obtained by using conjugate pictures separated by the ordinary stereographic interval; but they are much less noticeable, because there is less disturbance of the ordinary condition of the muscles.

In front of the partition between the lens-boxes is hinged a pair of narrow brass screens, *c*, *c*, which, in the experiments just described, are pressed flat as in fig. 6. The left picture is thus hidden from the right eye and the right picture from the left eye. By folding them together, as in fig. 7, neither

picture is hidden by them from either eye. There is a second sliding cross bar, to which is attached by hinges a light wooden screen, *b b*, which in the same experiments is pressed down, as in fig. 6. By lifting this, as in fig. 7, the opening at the middle leaves the left picture visible to the right eye while the right picture is hidden from it; and *vice versâ* for the left eye. The semi-lenses may now be taken out; and for them are substituted a pair of prisms, *p*, *p* (fig. 7), whose refracting-angle is 12° or 15°. The bases of these are pressed against the springs. The cross bar on which the stereograph rests (*a*) is pushed back to the end of the longitudinal bar; and on looking through the prisms the binocular image is seen in pseudoscopic relief. This effect is very striking if the stereograph selected be one in which ordinary perspective is not strong; that of the full moon has been found best. The cross bar may now be pulled as near as convenient, the visual lines becoming more convergent, but not uncomfortably so. Folding down the wooden screen (*b b*), a pair of monocular images of the moon are seen, one on each side of the concave binocular image, but themselves presenting no relief. They appear very perceptibly larger than the binocular image, but slightly smaller than either picture when viewed with normal vision over the stereoscope. Since the prisms have no magnifying- or diminishing-power, this illusion cannot be referred to the effect of refraction, but is explicable only on physiological grounds.

By removing both prisms and semi-lenses the instrument becomes a direct-vision stereoscope, for use with the unaided eyes. To secure natural perspective, arrange the screens as in fig. 6, but push the cross bar as far off as possible. By now gazing, as if through the stereograph at a very distant point, with the muscles of the eyes perfectly relaxed, double vision results, and the two interior images are seen to overlap and then coalesce. For most persons this experiment is difficult if the stereographic interval much exceeds the observer's interocular distance, and it is not at first easy to secure enough muscular relaxation. Some have found it better to draw the cross bar up as near as possible, and then push it off after coalescence of images has been attained, the binocular image being at first very dim.

To secure the pseudoscopic effect by direct vision, arrange the screens as in fig. 7, but push each of the cross bars out as far as possible. Fix the gaze upon the projection (*a*) at the top of the opening in the wooden screen, and keep it thus fixed while the latter is pulled nearer. When this almost reaches the position *b b*, in fig. 7, the interior images beyond

will be seen to coalesce; and the attention can then be transferred to their resultant combination. This is a hemispherical lustrous cup, if the stereograph be that of the moon, which is smaller and nearer than that obtained by use of the prisms. By pulling the cross bar nearer, the cup becomes smaller, relatively shallower, and, for obvious mathematical reasons, elliptic with the minor axis horizontal. The wooden screen can now be folded down, as before, and attention be given to the variable size of the image as determined by the conditions under which it is seen.

The semi-lenses may be restored, and the screens arranged as in fig. 6. The cross bar, *a*, is removed; and in its place is put one upon which are pivoted a pair of revolving bars, whose springs hold in position a pair of cards on which are perfectly similar outline figures, such as series of concentric circles. The production of stereoscopic effects, either orthoscopic or pseudoscopic, at will, by oblique projection upon the curved surfaces of the retinas, has been already fully discussed* and now needs no explanation.

40 W. 40th St., New York,
February 27, 1882.

XXXVIII. *On Pseudo-Carbons.*

By C. F. Cross, *B.Sc., and* E. J. Bevan†.

THERE is a general knowledge amongst chemists that the term "carbon" has been loosely applied to a number of substances whose composition and properties, on the experimental side, and the consideration of the condition of whose formation, on the theoretical side, completely prove their non-elementary nature. Notwithstanding this, a glance at the most modern text-books will show that *coal* is still spoken of as "a form of carbon;" *charcoal* also; and the action of sulphuric acid on sugar is still taught to consist in the resolution of the *carbohydrate* into carbon and water‡, the *dehydrating* energy of sulphuric acid being the efficient cause. It is necessary therefore that these defects in our terminology and classification should be stated, in order that they may be removed. In reviewing this subject we would first account for the existence of the errors in question. Certainly the cause would seem to lie in the inertia of the chemical mind—a desire to *know* without *ascertaining*, a tendency to accept a simple

* See the preceding article.
† Communicated by the Authors.
‡ This explanation may be dismissed without discussion.

hypothesis framed upon similitudes, and to forsake the rigid requirements of the experimental method where it offers a vista of complexity and the higher mathematics. But perhaps we ought rather to regard the errors in question in the light merely of a confession of postponement of a problem which the science has not yet been in a position to attack, or in favour of the larger number of those more immediately pressing. Still we cannot avoid the conclusion that this has been rather thought than expressed, at least in writing; and whatever be the cause, the persistent use of the terms of which we are speaking amounts to a really grave error. At the outset, therefore, we would propose the term *pseudo-carbons* for the black substances, containing a more or less high percentage of carbon, which are formed in various modes of resolution of the carbon compounds,—including thereby the *coals* and *graphites* of "natural" origin, the charcoals &c. of pyrogenic origin, and those substances which are formed by "carbonizing" organic substances by chemical treatment.

The main aspect of these various modes of resolution is undoubtedly the accumulation of carbon; and the conclusion is unavoidable that they are conditioned by the tendency of the carbon atoms to unite with one another, *i. e.* to form condensed molecules. This property of the element is the very basis of its special chemistry, and of the widest significance in the living world; and in this its last expression (i. e. *tending* to pass from the combined to the elementary condition) we see the crowning chapter of its remarkable history. In our interpretation, however, of this chapter we should guard against a spurious ideal, and ask ourselves, Are there any facts which warrant the conclusion that the elementary condition could be attained by any process of cumulative resolution of carbon compounds? and, further, Does not the mathematical analogy of series proceeding by a definite law of development (and that these bodies have been or can be developed through infinite series is an unavoidable inference) indicate its impossibility, except under the operation of a new cause?

Berthelot*, we believe, was the first to apply this mode of reasoning to the case in question, and to regard the hydrogen and oxygen present in these pseudo-carbons as essential constituents, and the pseudo-carbons themselves, therefore, as a complex of compounds (C, H, O) of very high molecular weight. Mills has given us a more general view of such molecular condensations in his theory of Cumulative Resolution†; but the attendant phenomena are in this group of resolutions too complex, and as yet insufficiently investigated, to allow of

* *Ann. Chem. Phys.* [5] xvii. p. 139. † Phil. Mag. Suppl. June 1877.

exact representation. The ultimate solution of these problems cannot, therefore, be more than indicated; and it is thrown upon the experimentalist to supply the requisite data. It is an interesting fact that as early as 1805 Hatchett* published the results of an investigation of the action of nitric acid upon these pseudo-carbons, in which he established, as a general property, that they are by this action converted into soluble bodies having the astringent and other characteristics of the tannins. His results are, it is true, of a somewhat empirical and qualitative order; they are nevertheless worthy of much more attention than they have received; and their recognition would have kept prominent the compound character of the pseudo-carbons. It is noteworthy that Berzelius† was sufficiently impressed with their importance to devote considerable space in his treatise to their reproduction. Modern writers, on the other hand, have ignored them, probably from a sense of their lacking theoretical coordination. Hatchett himself appears conscious of this, and also of some virtue in undertaking the investigation of these bodies—the products of the action of the acid,—from which, as he says, chemists have doubtless been deterred, or even repelled with disgust, by reason of the "proteus-like" changes which they undergo whilst under experiment.

In ignorance of his results, we were led to investigate certain of these pseudo-carbons, from the altogether different standpoint of their relationship to the carbohydrates. The black "carbonaceous" substance which we obtained‡ by the action of sulphuric acid at 70° C. upon cellulose, we found to be converted by the action of chlorine into a bright yellow chlorinated substance, soluble in alcohol, and resembling in composition and properties the product of the action of this gas upon the jute-fibre substance—the latter being an undoubtedly aromatic derivative, having many points of resemblance to tetrachloroquinone, and standing in close relationship to the natural group of astringents or tannins. It is not our intention to reproduce the long synthetical proof by which we sought to establish, from the chemical side, the developmental connexion of the tannins with the carbohydrates. Suffice it to say that the most direct point in the proof lay in the chemical characteristics of these pseudo-carbons; and from a study of these, as yet only superficial, we are impressed with the relations of both to the carbohydrates, the connexions being completed through infinite series of naturally-occurring

* Phil. Trans. 1805.

† *Traité de Chimie* (translated from 4th German ed., by Valerius), iii. p. 218.

‡ Brit. Assoc. Reports, 1881.

substances. The subject is one that may be safely left to develop itself in the minds of all who contemplate the science in relation to plant-life, and will lead to the conviction of the indefinitely wide potentiality of cellulose.

Although the passage of cellulose into coal is an obvious fact, the processes by and through which the transformation has occurred lie outside the range of observation, and must remain therefore to that extent a matter of speculation. On the negative side, however, greater certainty is attainable; and the statements of Zirkel* and Richters†, which formulate the older views of the composition of coal, viz. that it is a mixture of pure carbon with bituminous substances more or less, may be altogether dismissed. Balzer, we believe, was the first to combat these loose ideas; and in his excellent little treatise on the Formation of Coal‡, he advances, on the basis of a careful consideration of the origin and chemical characteristics of the coal-substance, the hypothesis that it consists of a mixture of complicated compounds, genetically if not homologously related, the most important feature of whose atomic structure is the union of the carbon atoms with one another, progressive condensation in this direction being expressed by decreasing susceptibility to the action of reagents. This treatment of the subject, however imperfect its development within the narrow limits of the treatise in question, has the merit of throwing the onus of proof of the presence of elementary carbon in coals and other pseudo-carbons upon those who have assumed it, and of opening up the question in its objectivity. We do not propose to enter into it more fully at present, than to say that we have obtained, by the action of chlorine upon a (Wigan) coal, a chlorinated derivative similar to those obtained by us from lignified fibres, and also observed that the whole coal-substance was so profoundly modified by the action of hydrochloric acid (diluted) and potassium chlorate as to be subsequently entirely soluble in alkalies. Our investigation of these points is proceeding; but even at this stage they indicate very clearly the developmental connexion of the coal with the cellulose constituents of growing plants.

We cannot, however, leave Balzer's brochure without noticing an incidental allusion§ to the reducing-action of the pseudo-carbons, notably charcoal, upon sulphuric acid, in which he states, as a well-received fact, that the reducing-agents are the compound constituents of the pseudo-carbons, and not elementary carbon. We have made a number of experiments upon all the pseudo-carbons, both in their normal

* *Petrographie*, i. p. 361. † *Chem. Centr.* 1870. p. 245.
‡ *Vierteljahrss. Zürch. naturf. Ges.* xvii. II. 1 (1872). § Page 15.

state and after ignition in chlorine gas; and we found that the amount of sulphurous acid formed was inversely as the percentage of carbon, those which had been ignited in chlorine exerting no reducing-action whatever. We have not been able to find that this fact is at all generally recognized; and therefore we do not think it superfluous to state it here, especially as it is an important criterion of the position of any pseudo-carbon in the series.

There is yet one other important bearing of these views which remains to be stated; and that is, their relation to the Chemical Theory of Gunpowder. If from the analysis of a normal ordnance gunpowder* the atomic ratios of the constituents, including the hydrogen and oxygen present in the charcoal, be calculated, we obtain the following numbers:—

		Pseudo-Carbon.		
KNO_3	S	C	H	O
13	7	16	7	2

(excluding ash and water).

Any chemical theory of gunpowder which fails to give an account of the H and O, existing, as they do, in such important (molecular) quantity, is necessarily incomplete. Those who are, by their special circumstances and experience, in a position to investigate this point, should certainly measure the influence of the hydrogen, and the compounds of which it is a constituent, upon the properties of gunpowder. What, as an extreme case, would be the behaviour of a powder made from charcoal after ignition in chlorine? Is it not at least certain that the character of a gunpowder would be influenced by the chemical constitution of its "carbonaceous" constituents?

On the more practical side, have we any certainty that the pseudo-carbons used in the manufacture of gunpowder are those best adapted to the purpose? This is surely a matter that should not be allowed to rest on an empirical basis; and we are not aware that it has received a thorough scientific treatment. We refrain from further discussion at the present time of this and cognate subjects: we shall have achieved our object if the term proposed by us as alone applicable to the substances of which we have treated recommend itself to the judgment of chemists, and still more if the recognition which it contains of their distinctly non-elementary character prove an aid to classification.

* Karolyi, Phil. Mag. October 1863.

XXXIX. *On the Constitution of the Lines forming the Low-Temperature Spectrum of Oxygen.* *By* PIAZZI SMYTH, *Astronomer Royal for Scotland**.

I HAD added (at p. 140 of my paper on "Gaseous Spectra") to the "small dispersion" account therein given of the above-mentioned spectrum of luminous oxygen gas, that two, if not four, of its very few and scanty lines appeared, when viewed with much higher dispersion, to be double, but that I hoped to give a more exact account of them, after completing some arrangements then in progress for increasing both the dispersion and magnifying-power of my spectroscope.

These improvements, together with a great advance in definition, were finished last November; and almost the first result they yielded was, to much more than confirm what I had only suspected before, and to do so, moreover, with such vigour and certainty as to make me inquire right and left for several weeks, to ascertain if what I then saw was really a new discovery, or had perhaps been known long before to older and better spectroscopists.

So far as I have been able to gather, the thing is new, and promises to be important to theorists in molecular vibrations, on account of what it fulfils. To explain this, let me refer to Dr. Arthur Schuster's valuable paper in the Transactions of the Royal Society, London, for 1879, "On the Spectra of the Metalloids: Spectrum of Oxygen." Beginning his exposition of the low-temperature spectrum (after having treated very fully of the exceedingly different high-temperature spectrum) of oxygen with the late Prof. Plücker's account of it, and concluding with his own confirmatory observations, Dr. Schuster shows that the low spectrum had always been found to consist of only four, single, wide-apart lines—viz. one in the orange, one in the citron, another in the green, and another still in the indigo-violet; and to that spectrum, containing only four such solitary, single, simple lines, the learned Doctor gave the second or *alias* name of the "compound-line" spectrum of oxygen.

The reason for such an apparent misnomer, or *lucus à non lucendo*, was, that at the very moderate temperature of electric illumination at which this trifling-lined spectrum of oxygen appears, most other gases do give forth very compound-line spectra indeed—spectra with hundreds or even thousands of

* From the Transactions of the Royal Society of Edinburgh, vol. xxx. part 1. Communicated by the Author.

lines, arranged in peculiar bands or parcels, and known generally as fluted spectra. Hence Dr. Schuster's name for what he saw in oxygen, while principally intended to show the physical circumstances under (or temperature-level on) which it is produced, might perhaps privately have been intended to indicate that he had somehow an idea that if the lines were not compound they ought to have been, and would one day be found so to be.

That, so far as I can ascertain, is the furthest point the subject has even yet reached elsewhere; and it was at that point that I had taken it up in 1879 and 1880, in the paper already printed by this Society. In the regular course of that paper, going through many gases, with single-prism power only, I abundantly confirmed Messrs. Plücker's and Schuster's four wide-apart lines, as constituting in themselves alone almost the whole and entire low-temperature spectrum of oxygen. I did, indeed, also find a strong line in the scarlet-red, besides two in the red or ultra-red, and two in the citron-green, of extreme faintness, and only probably belonging to the same spectrum. But they did not in any way alter the apparent anomaly of the Doctor's name; for each of these new lines was also solitary: and what are after all nine, or, to keep within more certain bounds, five, simple lines standing separate and along a length where 10,000 such could take their places without interfering with each other?

But when I looked last November with the improved apparatus, what a change was there! for of the five certain lines no less than four were found to be triple, after a fluted fashion too. These four truly compound lines then were—mine in the scarlet-red, and Plücker and Schuster's three lines in the orange, the citron, and green respectively; but their last line in the indigo-violet remained persistently and positively single. Still, with four fifths of this most scanty spectrum now proved to consist of triplets instead of single lines, Dr. Schuster's original and really most happy name for it, of "the compound-line spectrum" of oxygen, was fully justified, at the same time that the miniature scale of the triplets seemed to make the physical nature of oxygen more markedly different than ever from all other known gases; for they, at the same temperature-level, generally make their bands or compound-line arrangements on a comparatively enormous scale, and in multitudinous groupings.

To those who are engaged in chemical spectroscopy it will at once convey an idea of the small-sized triplicity of these oxygen lines, to be told that from the first to the second of each triplet the distance is one fifth that of the well-known salt-line double; and from the second to the third is between

one eighth and one ninth of the same space; while the salt-line double itself is only one eighth of the average distance apart of the stronger flutings of the citron-band of the carbo-hydrogen blowpipe-flame, which band has some six or seven of such flutings within its easily perceivable breadth.

Or, again, if we should on the black board represent the separation of the first and second of any oxygen triplet by a tenth of an inch, and from the second to the third by six hundredths of an inch (when it will require a good eye from the other side of the table to separate them), that would indicate a scale for the whole spectrum, or from red to violet, of 25 feet; and that is rather more than three times as long as the late Professor Ångström's grand and almost universally followed "Normal Solar Spectrum."

For bright-line chemical spectroscopy, and especially with the faint light of incandescent gas in a low-temperature electric spark, it is by no means usual or easy to separate lines so very close together as the members of one of the oxygen triplets. A few words of explanation may therefore be demanded of me in proof that the resolution was real and not an optical deception. The propriety of the demand, too, I am quite ready to allow, knowing only too well that there are prisms which will fringe every bright line with diffraction repetitions, or, when out of the best focus, will double or treble any line, and others, again, that make them so broad and hazy that clear separation of very close lines would be utterly impossible.

As my gas-vacuum-tube spectroscope admits only of a deviation range up to 45°, or that of a single white-flint prism of 52° refracting angle, I was compelled to have recourse to compound prisms to get up the necessary dispersion for crucial cases, say that of seven or eight such prisms. Now, although some beautiful compounds were made for me both in France and this country, they invariably failed in the item of perfect definition. I then tried a large fluid bisulphide-of-carbon prism made in Paris, but failed in several ways. So lately I entered into a contract with an exceedingly skilful as well as persevering optician in London, viz. Mr. Adam Hilger, to make two large bisulphide prisms, having a clear circular aperture of 2·1 inches diameter, a refracting angle of 104°, anti-prisms of crown glass square at the ends, and a central angular bored block of the same material to hold the fluid.

The troubles poor Mr. Hilger had to go through were almost overwhelming. He bored through block after block of crown glass, only to find after an hour, or a day, or a week

that it fell in pieces of its own accord, until he had a heap of tunnelled fragments large enough to make a cautionary photograph of. And when he had at last succeeded by his unconquerable perseverance in securing two blocks that stood, and the anti-prisms were to be fastened on their faces, he tried almost every patented and unpatented cement before he found one, or rather a particular method of using it, which could withstand the action of the bisulphide for more than a day or two.

At last, however, though long after the contracted time, he brought the two prisms here complete; and they passed successfully through the severest trial I had prepared for them, viz. that when a hydrogen-line was at its brightest, shining like a ray of sunlight in a dark field, its light must be entirely and sharply confined to the width of opening of the slit for the time being. Not, however, until some months after, when the telescope power was also improved, and a new class of difficulties with the prisms had been overcome, could the desired trial on oxygen be made. Now, whereas I had, as mentioned already, on previous occasions with the best compound glass prisms I could procure, seen only an uncertain idea that some of the oxygen-lines might be double, I now saw the real triplicity of four of the lines, and measured them micrometrically with a degree of certainty and satisfaction that I had never dreamt of with the older apparatus; and this triplicity of these lines never came out more remarkably than when the singularity of really single lines, such as those of hydrogen impurities in the same gas-tube, was rendered most distinctly. I will therefore now only seek to conclude with a few words on the bearing of this tripleness of the oxygen-lines, first, on the disputed question of the existence of oxygen in the atmosphere of the sun, and, secondly, on the absence of hitherto recognized oxygen-manifestations (though oxygen is so well known to exist as a large part of the earth's atmosphere) in the telluric rays that become visible in the solar spectrum at sunrise or sunset.

Oxygen in the Sun.

For many years it was a sort of crying wonder that the spectroscope showed no traces in the sun of so necessary a gas to combustion as oxygen—the expected test being, that we should see there as dark lines all the lines which are seen bright when oxygen gas, on being rendered incandescent in a *high* electric temperature, then shows what is called its "elemental-line spectrum." But not one of those lines could be detected by its dark counterpart in the sun. At length

Professor Henry Draper, of New York, from a series of experiments made with extraordinary skill and power, announced that oxygen appears in the solar spectrum not in its dark but in its bright lines, outshining where they come the brightness of the sun's continuous-spectrum background. Yet, though he gallantly made a voyage from New York to London especially to describe his experiments, and was honourably received and attentively heard, there are some persons there who are not convinced yet.

Now Dr. Schuster had already compared his four lines, though of the low-temperature oxygen-spectrum, with Ångström's normal solar-spectrum map, under the idea apparently that, though the solar oxygen might have been rendered incandescent in the very hottest central regions of the sun, it might have its Fraunhofer dark correspondences checked off by cooler oxygen vapour outside. But as his then knowledge of oxygen low-temperature lines made them only four single, simple, thin lines, the comparison was not attended with any very certain result; for there are so many of such lines, unclaimed for any element, strewed all along the solar spectrum. But now that I had found four out of five lines to be triplets of an accurate kind, could any thing further in the way of identification be ascertained?

In apparently the very place of the three fainter of the above-described triplets there is a close double of peculiarly thin Fraunhofer lines depicted by Professor Ångström in his normal solar-spectrum map; and in the place of the brightest of them, viz. Schuster's orange line, there is a triple* of the same kind of *ultra* thin lines; and not one member of all those four groups has been claimed for any known element by the great Swedish physicist. Yet I am by no means satisfied that the degree of correspondence is conclusive, and can only hope that those who have the means will positively confront the new oxygen triples with the sun itself and inform us what they find.

Oxygen of the Earth's Atmosphere in the Telluric Solar Spectrum.

If the long silence of the spectroscope touching oxygen in the sun was a wonder, and perhaps something the reverse of praise to those who used the supposed all-powerful instrument,

* That triplicity is indeed there broken in upon by a far stronger line, which Ångström traces to sodium (Na); but such cases of mere optical juxtaposition are frequent in the crowds of lines in much of the solar spectrum, without any physical connexion being supposed to be implied thereby.

what shall be said for its continued silence as to the presence of any free oxygen in the earth's atmosphere, when *some* gases that are therein do make themselves most signally conspicuous on the solar spectrum, in the shape of dark lines or bands, growing as the sun descends in altitude, until they become at last more grandly thick and black than all the truly solar markings put together?

Amongst these gigantic cases of Fraunhofer lines of some kind of telluric gaseous origin, we do indeed know, by a sort of inductive process rather than by any positive proof, that the band too hastily called "little *a*" and other bands and lines near C and near D are the spectroscopic proofs of *watery vapour*, as an invisible but potent gas in our atmosphere. But what makes the far greater A or great B and the α (alpha) band, no one pretends to know.

At first sight it might well be suggested that they must be formed by one or other, or both, of the two grand constituents of the earth's atmosphere as established by the chemists, viz. oxygen and nitrogen. But there we are instantly met by Ångström's inflexible law, promulgated by him in 1853, and repeated at p. 39 of the description of his "Normal Solar Spectrum" in 1868, viz. that "a gas in the state of incandescence emits luminous rays of the same refrangibility as those which it can absorb"—or, conversely, when it acts by absorption, it produces dark in all the spectrum places where, when in a state of incandescence, it produced bright lines.

Now we know by laboratory experiments what bright lines are given out by both oxygen and nitrogen in various states of incandescence; and not one of those lines is in the place, and at the same time endowed with the physiognomy, of great A or great B, or of the α (alpha) band of the sunset solar spectrum.

To get over this astonishing difficulty, Ångström—who held that all these three bands are of a similar visible constitution, viz. a thick line and then a band of thin lines stretching out towards the red end of the spectrum—suggested that they might be produced by "carbonic acid." But, over and above the difficulty that carbonic acid is an almost insensible impurity in the open air, and could hardly be expected to extinguish every sign of the existence of the atmosphere's two great constituents, we do know the spectrum given by incandescent carbonic acid in the laboratory, and it cannot claim to having a band in the place of either A or B or α (alpha), besides its series being turned in the opposite direction, or every band vanishing towards the violet, in place of the red, end of the spectrum.

Evidently, then, in despair, this sorely tried and now departed philosopher of Upsala, but who is still our chief authority in this line, on page 41 of his memoir already cited, after acknowledging that he had, in another place, suggested "carbonic acid," says further, perhaps it is ozone which produces these bands. No reason is given; but the prudent caution is inserted—if there is free ozone in the earth's atmosphere. Or, again, he adds as a third supposition, perhaps they are produced by "fluorescence of oxygen," a gas which he there states gives forth "a faint phosphorescence in a Geissler gas-vacuum-tube when an electric current is caused to pass through it." But this is just as far from presenting us with the very definite lines constituting the bands of great A, great B, and α (alpha) of the sunset telluric solar spectrum as before.

Now I have not, any more than all the rest of the world at this moment, any positive and proved means of raising Professor Ångström out of the difficulties he eventually sunk under. But the minute triplicity of the greater part of the low-temperature lines of the oxygen-spectrum, described in the beginning of this paper, may perhaps let in a chink of light upon the difficulty, when combined with the true constituent features of these grand telluric lines A, B, and α (alpha), as set forth in my Lisbon solar spectrum, so recently honoured by the Royal Society of Edinburgh with their Makdougall-Brisbane Prize.

Ångström, observing these three bands when they were thick and clumsy at sunset, pronounced their constitution to be exactly similar in every case. I, on the contrary, observing them in a high sun, when they were divested of the rotundity of flesh, and only their thin, linear bones appeared, found the ultimate constitution of the α (alpha) to be perfectly different from that of A and B; for in place of many regular and symmetrically arranged powerful lines, it was made up of little doublets and triplets of both inconceivable minuteness and very irregular occurrence (see the said Lisbon spectrum as printed in vol. xxix. of the Society's Transactions). These Lilliputian foundation-stones of the α (alpha) band appeared to me at the time almost ridiculous in their smallness; but now I recognize them as having a close family resemblance to the triplicity of the low-temperature oxygen-lines which I have been trying hereinbefore to describe. They are not indeed the very same; for they are in different spectrum places; but they do give the idea (suggested also by some points in both the aurora-spectrum and cometary spectra not yet reproduced in any laboratory elec-

trical experiments) that there is a temperature-level in nature for the incandescence of gases, much lower than that of the low-temperature oxygen-spectrum of these pages. Wherefore, if we could artificially produce that kind of ultra-low-temperature illumination, electric probably, we might find that the α (alpha) band in the solar sunset spectrum represented the oxygen, while the A and B bands showed us the nitrogen gas thereof—they two being the mighty gas constituents known so well to every one (*except telluric-line solar spectroscopists*) to exist in the earth's atmosphere, and in such overpowering quantity as to practically exclude every thing else except watery vapour.

XL. Regnault's *Determination of the Specific Heat of Steam.* *By* J. Macfarlane Gray*.

REGNAULT'S experiments on the specific heat of vapours have been interpreted by Regnault as giving results not at all in accordance with the deduction from the kinetic theory of gases, that, for matter travelling in single molecules, the product of the molecular weight by the specific heat is a constant for all substances. I have been led, by considering the order of temperature-pressures for steam, to conclude that the above deduction is true for steam; and I have no doubt, also for all matter travelling in single molecules. When, in 1880, I laid my conclusions before the Physical Society as being corroborated by Regnault's dynamical experiments, it was objected that Regnault's direct thermal experiments gave results widely different from my conclusions; and the report on my paper was that that difference proved that, in the corroborations I had pointed out, I had been led away by merely numerical coincidence.

I will now show that Regnault's thermal experiments have been misinterpreted by Regnault himself, and that he ought to have read the specific heat of steam, according to his experiments, to be exactly in accordance with the deduction of the kinetic theory.

The method of the experiments was to generate steam at 100° C., to superheat it under atmospheric pressure to (say) 125° in one set of experiments, condensing it in a calorimeter to ascertain what quantity of heat was given up, down to 0°. In a second set of experiments with the same apparatus, the temperature was raised to (say) 225°, while the steam was still at atmospheric pressure; this was also condensed in the

* Communicated by the Physical Society, having been read at the Meeting on February 25, 1882.

same calorimeter. It was found that the heat given up by the steam at 225° exceeded that given up by the steam at 125° by 48·051 units of heat; and, dividing by the difference of temperature (100), Regnault found 0·48051 for the specific heat of steam at constant pressure.

In these experiments the superheating to 125° was, no doubt, intended to thoroughly dry the steam, so as to get, in the quantity of heat abandoned in the first set of experiments, the entire heat of complete gasefication, because, if still some moisture remained in the steam, the latent heat of evaporation of that moisture would be included in the difference-quantity supposed to be due to the specific heat of temperature-raising, and, by that amount, the result would be too high.

It appears to me that the completion of the evaporation of suspended moisture cannot be accomplished between 100° and 125°; but it will be more likely to be carried on between 125° and 225°. Particles of liquid remain at the pressure-temperature whatever be the superheated temperature of the gas in which they are suspended. The rate of evaporation of those particles will therefore depend upon the temperature-difference; and in the lower range of temperatures but little of the moisture will be evaporated in its rapid passage through a worm heated to only 25° in excess of the pressure-temperature. In the second set of experiments the excess of temperature was 125°.

To test this, let similar experiments be made at 100°; and if the resulting apparent specific heats between 100° and 125° are higher than those obtained by Regnault between between 125° and 225°, then my argument is demolished; but if the results are smaller, then my suspicions have been justified, and a correction is required on the results as given by Regnault. Fortunately for my object, Regnault has left us (in vol. i., at page 695) the data of thirty-eight reliable experiments on the "total heat" of steam at 100° down to 0°, giving the mean = 636·70 units of heat. I have, with this "total heat" for 100°, compared the results of the experiments on the steam of 125°—in precisely the same way as Regnault worked for the interval between 125° and 225° (these experiments are recorded in vol. ii., pages 167–178). The results of my calculations are given below. The first two series were merely preparatory trials to arrive at the best form of apparatus; and only a few grammes of vapour were passed through the worm at each of those experiments. In the third and the fourth series ten times as much vapour was passed through on each occasion. Regnault says:—"J'ai pu opérer ainsi sur des quantités de vapeur beaucoup plus considérables, et diminuer l'importance

relative des corrections produites par les causes perturbatrices." This consideration is still more important when the range of temperature is limited to 25°—only one fourth of the range from which Regnault deduced his result. The large calorimeter was used only in the third and fourth series.

First series.	Second series.	Third series.	Fourth series.
·280	·546	·299	·386
·465	·591	·314	·381
·417	·567	·340	·346
·377	———	·375	·309
·463	Mean ·567	·400	·427
———		·411	·463
Mean ·400		·349	———
		·413	Mean ·3853
Preparatory trials.		·405	
		·415	
		———	
		Mean ·3721	

That the method of calculation may be perfectly clear, I give the particulars for the first experiment in the fourth series. Superheating to 124°·81, each unit of vapour gave up 646·28 units of heat down to 0°. Steam at 100° gave up 636·70 down to 0°.

$$\frac{646{\cdot}28 - 636{\cdot}70}{124{\cdot}81 - 100} = {\cdot}386.$$

The quantity of vapour in this experiment was 102·62 grms.; the quantity in the first of the first series was only 8·957 grms.

The fourth series was made with an apparatus which was an improvement on that used in the third series; and, taking that series only, adding the probable amount of moisture which would remain at 100° temperature (say 1 per cent. on the ·385), the result is ·389; this, on other grounds, I believe to be nearly correct. If, however, neglecting the other grounds for my opinion, we take the mean of the means of the third and the fourth series, we get

$$\frac{{\cdot}3721 + {\cdot}3853}{2} = {\cdot}3787.$$

This is what Regnault might fairly have done.

If we now calculate what Regnault's experiments would give as the kinetic-theory result,—we get the specific heat of hydrogen, at page 121, "mean = 3·4090;" and taking the molecular weight of steam =17·96, we find the specific heat

of steam

$$3{\cdot}409 \times \frac{2}{17{\cdot}96} = {\cdot}3796.$$

That is to say, the kinetic theory and the thermal determination give almost identically the same number.

This is, I believe, the first experimental proof that the law of "inversely as the molecular weights" applies to compound gases.

XLI. *Acoustical Observations.*—IV. *By* Lord Rayleigh, *F.R.S., Professor of Experimental Physics in the University of Cambridge*.*

On the Pitch of Organ-pipes.—Slow *versus* quick Beats for comparison of Frequencies of Vibration.—Estimation of the Direction of Sounds with one ear.—A Telephone-experiment.—Very high Notes.—Rapid Fatigue of the Ear.—Sensitive Flames.

On the Pitch of Organ-pipes.

In the Philosophical Magazine for June 1877 I described some observations which proved that the note of an open organ-pipe, when blown in the normal manner, was *higher* in pitch than the natural note of the pipe considered as a resonator. The note of maximum resonance was determined by putting the ear into communication with the interior of the pipe, and estimating the intensity of sounds of varying pitch produced externally.

A more accurate result may be obtained with the method used by Blaikley†, in which the external sound remains constant and the adjustment is effected by tuning the resonator to it. About two inches were cut off the upper end of a two-foot metal organ-pipe, and replaced by an adjustable paper slider. At a moderate distance from the lower end of the pipe a tuning-fork was mounted, and was maintained in regular vibration by the attraction of an electromagnet situated on the further side, into which intermittent currents from an interrupter were passed. Neither the fork nor the magnet were near enough to the end of the pipe to produce any sensible obstruction. By comparison with a standard, the pitch of the fork thus vibrating was found to be 255 of König's scale. The resonance of the pipe was observed from a position not far from the upper end, where but little of the sound of the fork could be heard independently; and the

* Communicated by the Author.

† Phil. Mag. May 1879.

paper slider was adjusted to the position of maximum effect. This observation was repeated many times, the distance between marks fixed on the pipe and on the slider respectively being recorded. The following numbers give the results, expressed in fiftieths of an inch:—

31	33	30	25	31
25	32	31	34	29
35	28	29	30	

The extreme range being only one fifth of an inch, shows that the observation is capable of considerable precision, corresponding as it does to only about 2 vibrations per second out of a total of 255. Finally, the slider was fixed at the mean of the above-determined positions, and the natural note of the pipe was then considered to be 255. The error in length was probably less than $\frac{1}{20}$ inch, and the error in pitch less than half a vibration per second.

The pipe was then blown from a well-regulated bellows; and the beats were counted between its note and that of the standard fork above referred to, the pressure being taken simultaneously with a water-manometer. Three observers were found to be necessary for accurate working—one to count the beats, rising to the rate of ten per second, one to keep the bellows uniformly supplied with wind, and one to observe the manometer. At pressures between 4·2 inches and 1·53 inch the pitch of the pipe was very well defined and considerably higher than the natural note. Below 1 inch the pitch became somewhat unsteady, and distinct fluctuations in the frequency of the beats were perceived, while no corresponding variation of pressure could be detected. At about ·8 inch the pitch of the pipe falls to unison with the natural note, and with further diminishing pressures becomes the graver of the two. Below ·7 inch the unsteadiness is such as to preclude accurate estimations of pitch.

The results are embodied in the accompanying table, which shows the correspondence of pitch and pressure. Instead of the actual number of beats counted, which involves a reference to the extraneous element of the pitch of the standard fork, the number (greater by unity) is given which expresses the excess in the frequency of vibration of the actual over that of the natural note of the pipe. It will be seen that at practical pressures the pitch is raised by the action of the wind, but that this rule is not universal.

Pressure, in inches.	Difference of frequencies.	Remarks.
4·2	+11·0	
2·72	9·3	
2·26	8·4	
1·86	7·1	
1·53	5·6	
1·32	4·2	
1·06	2·1	
·88	1·5	
·82	+ ·1	
·75	− ·5	
·68	1·2	
·64	2·3	
·57	3·9*	About this point a discordant high note comes in alongside of the normal note.
·53	3·7	
·48	3·1*	Here the discordant note ceases.
·46		
·40		
·39 } ·38 }	− 4·0	
·35	*	*About this point the octave of the normal note is heard, after which the normal note itself disappears.
·30	——	The normal note reappears, the octave continuing.
·26	——	The octave goes, and then the normal note, after which there is silence.
·20	+	Octave comes in again, and then the normal note, at a pitch which falls from considerably above to a little below the natural pitch. At the lowest pressures the normal note is unaccompanied by the octave.
·11	−	

Slow versus *quick Beats for comparison of Frequencies of Vibration.*

Most of those who have had experience in counting beats have expressed a preference for somewhat quick beats. Perhaps the favourite rapidity has been four beats per second. There is no doubt that in the case of insufficiently sustained sounds slow beats are embarrassing. The observer gets confused between the fall of sound which is periodic and that which is due to the dying away of the component vibrations, and loses his place, as it were, in the cycle. But it is also possible, I think, to trace an impression that, independently of the risk of confusion, quick beats can be counted with greater accuracy than slow ones. It is indeed true that the number of beats in a given time, such as a minute, can be determined with greater relative accuracy when there are many than when there are few; but it is also true, as a little consideration will show, that in the comparison of frequencies we are

concerned not with the *relative*, but with the *absolute* number of beats executed in the given time. If we miscount the beats in a minute by one, it makes just the same error in the result whether the whole number of beats is 60 or 240.

When the sounds are pure tones and are well maintained, it is advisable to use beats much slower than four per second. By choosing a suitable position we may make the intensities at the ear equal; and then the phase of silence, corresponding to antagonism of equal and opposite vibrations, is extremely well marked. Taking advantage of this, we may determine slow beats with very great accuracy by observing the time which elapses between recurrences of silence. In favourable cases, the whole number of beats in the period of observation may be fixed to within one tenth or one twentieth of a single beat, a degree of accuracy which is of course out of the question when the beats are quick.

In some experiments conducted by Dr. Schuster and myself*, to determine the absolute pitch of a König standard fork, I had occasion to observe some very slow beats. The beating sounds were of pitch 128. One of them was steady, proceeding from an electrically maintained fork; the other (from the standard fork) gradually died away. In order to be more independent of disturbing noises to which we were exposed, a resonator was used connected with the ear by an india-rubber tube. The standard fork was mounted at the end of a wooden stick, so that it might not be heated by the hand. As the vibrations became less powerful, the prongs of the fork were caused slightly to approach the mouth of the resonator, so as to maintain the equality of the two component sounds. In this way it was possible to obtain very definite silences, and to measure the interval of recurrence with accuracy. In one observation, extending over about two minutes, the beat occupied as much as twenty-four seconds, and there was no confusion. I have little doubt that even slower beats might be observed satisfactorily if both components were steadily maintained.

Estimation of the Direction of Sounds with one ear.

In my former experiments (Phil. Mag. June 1877) I found it difficult to obtain satisfactory observations with one ear closed, although it was not doubtful that the power of estimating directions was greatly curtailed. My desire to experiment upon an observer deaf on one side has since been gratified by the kind assistance of Mr. F. Galton. In January 1881 experiments were tried with him similar to those on normal

* Proc. Roy. Soc. May 5, 1881, p. 137.

hearers described in my former paper. It was found that Mr. Galton made mistakes which would be impossible for normal ears, confusing the situation of voices and of clapping of hands when to his right or left, as well as when in front or behind him. Thus, when addressed loudly and at length by a little boy standing a few yards in front of him, he was under the impression that the voice was behind. In other cases, however, there seemed to be some clue, whose nature we could not detect. Bad mistakes were made; but the estimates were more often right than mere chance would explain.

After this experience it seemed unlikely that there could be any success in distinguishing whether pure tones came from right or left, and from in front or behind. The experiment was tried, however, with in the main the expected result. But when the sounds were close, there appeared to be some slight power of distinguishing right and left, which may perhaps have been due to incomplete deafness of the defective ear.

A Telephone-experiment.

In Maxwell's 'Electricity and Magnetism,' vol. ii. § 655, it is shown that a perfectly conducting sheet acts as a barrier to the magnetic force:—"If the sheet forms a closed or infinite surface, no magnetic actions which may take place on one side of the sheet will produce any magnetic effect on the other side." In practice we cannot use a sheet of perfect conductivity; but the above-described state of things may be approximated to in the case of periodic magnetic changes, if the time-constants of the sheet circuits be large in comparison with the periods of the changes.

The experiment is made by connecting up into a primary circuit a battery, a microphone-clock, and a coil of insulated wire. The secondary circuit includes a parallel coil and a telephone. Under these circumstances the hissing sound is heard almost as well as if the telephone were inserted in the primary circuit itself. But if a large and stout plate of copper be interposed between the two coils, the sound is greatly enfeebled. By a proper choice of battery and of the distance between the coils, it is not difficult so to adjust the strength that the sound is conspicuous in the one case and inaudible in the other.

Very high Notes. Rapid Fatigue of the Ear.

In former experiments with bird-calls I had often been struck with what seemed to be the capricious behaviour of these sources of sound, but had omitted to follow up the ob-

servation. In the spring of last year the apparent caprice was traced to the ear, which very rapidly becomes deaf to sounds of high pitch and moderate intensity. A bird-call was mounted in connexion with a loaded gas-bag and a water-manometer, by which means the pressure could be maintained constant for a considerable time. When the ear is placed at a moderate distance from the instrument, a disagreeable sound is heard at first, but after a short interval, usually not exceeding three or four seconds, fades away and disappears altogether. A very short intermission suffices for at any rate a partial recovery of the power of hearing. A pretty rapid passage of the hand, screening the ear for a fraction of a second, allows the sound to be heard again. During his visit to Cambridge in March 1881, I had the pleasure of showing this experiment to Prof. Helmholtz.

The uniformity of the sound in the physical sense may be demonstrated with a sensitive flame, which remains uniformly affected so long as the pressure indicated by the manometer does not vary. The sensitive flame may also be employed to determine the wave-length of the sound, in the manner described in the Philosophical Magazine for March 1879, p. 154. In the case of two bird-calls blown with a pressure of about $2\frac{1}{2}''$ of water, the wave-lengths were found to be respectively $1''{\cdot}304$ and $1''{\cdot}28$. The method was found to work easily and with considerable accuracy, almost identical results being obtained from observations of the *loops*, where the flame is *most* affected, and from the *nodes*, where it is *least* affected.

By modifying the pressures with pinch-cocks, the two notes could be brought into unison. Although both bird-calls were blown from the same gas-bag, it was not possible to keep the beats slow for more than a few seconds at a time; but that period was quite sufficient for the effects of the beats to manifest themselves in a striking manner by the behaviour of the flame. In repeating these experiments, it may be necessary to bear in mind that many people cannot hear these high notes at all, even at first. With a shorter wave-length of about $\frac{1}{2}''$, as determined by the flame, I was myself quite unable to hear any sound from the situation of the flame. A slight hissing was perceived when the ear was brought up close to the source; but it is probable that this was not the part of the sound that agitated the flame.

Sensitive Flames.

In the chapter devoted to this subject in Tyndall's 'Sound' (third edition, p. 231) the accomplished author remarks:—
"An essential condition to entire success in these experiments

disclosed itself in the following manner. I was operating on two fishtail flames, one of which jumped to a whistle while the other did not. The gas of the non-sensitive flame was turned off, additional pressure being thereby thrown upon the other flame. It flared, and its cock was turned so as to lower the flame; but it now proved non-sensitive, however close it might be brought to the point of flaring. The narrow orifice of the half-turned cock interfered with the action of the sound. When the gas was fully turned on, the flame being lowered by opening the cock of the other burner, it became again sensitive. Up to this time a great number of burners had been tried, but with many of them the action was *nil*. Acting, however, upon the hint conveyed by this observation, the cocks which fed the flames were more widely opened, and our most refractory burners thus rendered sensitive." In the abstract of a Royal-Institution lecture (Phil. Mag. Feb. 1867) a rather more definite view is expressed:—"Those who wish to repeat these experiments would do well to bear in mind, as an essential condition of complete success, that a free way should be open for the transmission of the vibrations from the flame, *backwards*, through the gas-pipe which feeds it. The orifices of the stopcocks near the flame ought to be as wide as possible."

During the preparation of some lectures on Sound in the spring of last year, it occurred to me that light would probably be thrown upon these interesting effects by introducing a manometer on a lateral branch near the flame. In the path of the gas there were inserted two stopcocks, one only a little way behind the manometer-junction, the other separated from it by a long length of india-rubber tubing. When the first cock was fully open, and the flame was brought near the flaring-point by adjustment of the distant cock, the sensitiveness to external sounds was great, and the manometer indicated a pressure of ten inches of water. But when the distant cock stood fully open and the adjustment was effected at the other, high sensitiveness could not be attained; and the reason was obvious, because the flame flared without external excitation while the pressure was still an inch short of that which had been borne without flinching in the former arrangement. On opening again the neighbouring cock to its full extent, and adjusting the distant one until the pressure at the manometer measured nine inches, the flame was found comparatively insensitive.

It appears, therefore, that the cause of the prejudicial action of partially opened stopcocks in the neighbourhood of the flame is not so much that they render the flame insensitive as that they induce premature flaring. There are two ways in

which we may suppose this to happen. It may be that, as Prof. Barrett suggests (Phil. Mag. April 1867), the mischief is due to the irregular flow and consequent ricochetting of the current of gas from side to side of the pipe; or, again, the cause may lie in the actual production of sonorous disturbance of the kind to which the flame is sensitive, afterwards propagated forwards to the burner along the supply-pipe acting as a speaking-tube. The latter explanation was the one that suggested itself to my mind at the time, in consequence of the observation that a hissing sound was easily audible by the ear placed close to the half-open stopcock through which gas was passing; and it was confirmed when I found that a screw pinch-cock could be used for adjustment near the flame with impunity, in which case no sound was perceptible.

Subsequently further experiments were tried with various nozzles inserted in the supply-tube. These included holes in thin metal plates and drawn-out glass tubes. Even though the rubber tubes were so bent that the streams issuing from the nozzles were directed against the sides, no sound was heard, and no loss of sensitiveness was apparent. It would seem that mere irregularity of flow produced no marked effect, and that, provided no sound attended it, the full pressure could be borne without flaring.

These observations in no way impair the value of the practical rule laid down by Tyndall. In some cases I have found a flame flare without external excitation when a neighbouring stopcock was partially closed, and in spite of the increase of pressure recover itself when the stopcock was completely opened. When the object is to investigate the conditions of flaring, the use of a manometer near the flame is decidedly to be recommended.

March 24, 1882.

XLII. *The Effect of Temperature on the Electrical Resistance of Mixtures of Sulphur and Carbon.* By Shelford Bidwell, *M.A.*, *LL.B.**

SINCE December 1880 I have from time to time made a number of experiments with the object of ascertaining whether sulphur could, under any circumstances, be made to exhibit the remarkable property possessed by selenium, and in a smaller degree by tellurium, of having its electrical resistance diminished by the action of light.

* Communicated by the Physical Society, having been read at the Meeting on March 25, 1882.

Amongst other things, the effect was tried of mixing it with graphite. The sulphur was heated to a temperature a little above its melting-point (115° C.), and when quite liquid a small quantity of tolerably pure powdered plumbago was stirred into it. The liquid mixture was then poured into moulds and allowed to cool quickly. Sticks or plates were thus produced of a substance which in general appearance closely resembled crystalline selenium, its surface being dull and slate-coloured, and its fracture metallic, not unlike that of cast iron.

A short stick of this substance was fitted with platinum electrodes (platinum wires being made red hot and pressed into the two ends), and was joined up in circuit with a Leclanché cell and a reflecting galvanometer. The spot of light was brought to the zero-point at the middle of the scale by means of a magnet, and the sulphur rod exposed to the radiation of a gas-flame which was held within a few inches of it. The spot at once began to move, showing a considerable change in the resistance of the rod. The gas-flame was extinguished; and the spot slowly returned to zero. Upon examination, however, it appeared that the behaviour of this sulphur rod differed from that of crystalline selenium in two important particulars. In the first place, the change of resistance was clearly an effect not of light, but of temperature. Burning magnesium produced no greater change than the gas-flame when held at the same distance. Sunlight was found to have a powerful effect, which was scarcely diminished when the light was caused to pass through red glass; but blue glass or a cell containing water formed an effectual screen. A black hot poker, or even the warmth of the finger, caused a greater deflection than a strong light which was too far removed to heat the rod sensibly.

Now there can be little doubt that the decrease in the resistance of selenium which occurs under the influence of radiation is totally distinct from any effect produced by temperature. It is of course true that absorption of radiations is followed by a rise of temperature, particularly when such radiations belong to the red and infra-red part of the spectrum; but it is not, I think, the fact that such rise of temperature is in any way connected with the remarkable variation of resistance under the influence of light, which, owing to the invention of the photophone, has lately attracted so much attention. The electrical effects of radiation are, in this case at least, no more due to rise of temperature than are its chemical effects. The evidence in favour of this assertion seems to me to be overwhelming; and I hope to return to it

upon another occasion, with special reference to the paper in support of the opposite view read last year by Dr. Moser.

The second point in which the sulphur rod appeared to differ from selenium, was in the *direction* of the change which the radiation of the gas-flame produced, the galvanometer showing that its resistance was increased, instead of being diminished as might have been expected. Here, however, upon the supposition that the effect is due to heating, the behaviour of the sulphur is really similar to that of selenium; for my experiments (agreeing with those of Prof. Adams) show that at ordinary temperatures slight heating is always accompanied by considerably increased resistance.

After numerous trials, in order to ascertain what proportions of sulphur and graphite yielded the greatest sensitiveness to heat, it appeared that a mixture containing 20 parts by weight of sulphur to 9 of graphite was the most suitable. It was also found to be more sensitive when cooled rather quickly than when cooled slowly, though its specific resistance (which is always high) was generally lower in the latter case.

In order that the peculiar property of the substance may be exhibited in the most effective manner, it is necessary to arrange it in thin films, so that a large surface relatively to the bulk may be exposed to the action of radiation. This was at first done by spreading it as thinly and evenly as possible upon plates of mica having tinfoil electrodes at each end. An objection to this method, however, was the enormously high resistance which it involved, amounting sometimes to several hundred thousand ohms. I therefore adopted the device which I generally use in the construction of selenium cells, and which is a simple modification of a plan originally proposed by Dr. Werner Siemens.

Two wires, preferably of platinum, are wound parallel to each other, and very close together, around a slip of mica, care being taken that the wires do not touch each other at any point. A film of the melted mixture is spread evenly over one surface of the mica; and the wire electrodes are thus connected with each other through half their entire length by a series of very narrow strips of the sulphur mixture.

The resistance of a sulphur "cell" constructed in this manner was 9100 ohms at a temperature of 14° C. The cell was slowly heated in an air-bath, and the resistance measured as accurately as possible at almost every degree. The following table sufficiently indicates the rate at which the resistance increased.

Temperature.	Resistance.
14° C.	9,100 ohms.
19°	11,400 ,,
25°	13,700 ,,
30°	16,700 ,,
35°	20,600 ,,
40°	26,900 ,,
45°	34,000 ,,
50°	42,900 ,,
55°	57,000 ,,

Thus at 55° the resistance of the cell was more than six times as great as at 14°.

Upon another occasion the cell was raised to a much higher temperature. No measurements were made with the bridge, however, the cell being simply connected with a battery and a galvanometer, and the deflections noted from time to time. After passing 55°, the resistance increased with great rapidity, until at a temperature of 100° it was sensibly infinite, there being no movement of the spot of light when the circuit was opened with a key. Immediately after passing 100°, the resistance began to fall even more quickly than it had risen. The deflection at 105° was the same as that at 85°; at 110° the same as at 65°; at 114° the same as at 50°; and at 115° the same as at 35°. The spot of light now became so unsteady (probably in consequence of the melting of the cell) that it was not possible to carry the experiment further.

A mixture has also been prepared in which lampblack was used instead of graphite; but very few experiments have yet been made with it, and the results obtained have not been uniform. At ordinary temperatures it generally behaves like the graphite mixtures; but its temperature of maximum resistance is probably lower than 100°. In one case indeed, in which the proportions were 8 parts of sulphur to 1 of lampblack, the resistance was found to be greatest at 15° (530,000 ohms), any change in the temperature, whether in the direction of heat or cold, producing a decrease. But since no such effect has been produced with other specimens of the same compound, I am inclined to think that it is due to some unnoticed peculiarity in the construction of the cell.

With this single exception, every specimen of the mixtures, whether made with graphite or lampblack, has at ordinary temperatures been found to have its resistance increased by heat.

At first sight this appears to be a very paradoxical phenomenon. It is now generally admitted that the resistance of

graphite and other forms of carbon is diminished by heat; and it is also commonly believed that a rise of temperature invariably causes a decrease in the resistance of insulators such as sulphur. The compound of sulphur and carbon with which we have to do is certainly only a mechanical mixture (for no chemical combination could be formed at a temperature of 150° C., which is never exceeded in making the preparation); yet the effect which heat produced upon it is exactly opposite to that produced upon each of its constituents.

Some experiments were made in order to determine the effect of heat upon the resistance of carbon and sulphur separately; but, although in the case of sulphur some unexpected results were obtained, nothing whatever was observed which would *per se* account for the variations of resistance in the sulphur and carbon mixtures.

I believe the true explanation to be this:—The mixture does not consist of a uniform structureless mass of sulphur having particles of carbon imbedded in and completely surrounded by it. It is in fact an aggregation of little crystals of sulphur, with carbon packed between them like mortar between bricks. The conduction thus takes place entirely through the carbon particles, which may be considered as extending in a series of chains from end to end of the mass. Under the influence of heat, both the sulphur and the carbon expand; but the expansion of the sulphur is nearly ten times as great as that of the carbon, the net result being that the carbon particles are drawn apart, and have fewer points of contact with each other. The number of complete chains is thus diminished, and the resistance of the mass consequently increased.

It is stated in Balfour Stewart's 'Treatise on Heat,' upon the authority of Kopp, that sulphur, after being heated to a certain temperature, contracts instead of expanding. This would account for the fact that, after attaining a certain temperature, the resistance of the mixture begins once more to decrease; for the carbon particles would, when the sulphur contracts, be brought together again.

Several experiments corroborate this explanation of the variation of the resistance. Thus a mixture was made containing 3 parts of shell-lac to 2 of graphite. Though the proportion of carbon was larger than in the sulphur experiments, its resistance was found, as was expected, to be infinite; for the structureless shell-lac penetrated between and completely surrounded the carbon particles. A mixture of paraffin and graphite gave the same result.

A short rod of the usual sulphur-and-graphite compound (20 parts to 9) was fitted with platinum-wire electrodes; and its

resistance in the air at 17° was found to be 3170 ohms. The rod was immersed in a beaker of turpentine at the same temperature, and its resistance almost immediately went up about 800 ohms; and in five minutes it was somewhat higher. Though this result did not at first appear to be easily intelligible, it is fully explained by supposing that the liquid penetrated between the sulphur crystals and surrounded the carbon particles. In order to increase the effect, the beaker of turpentine containing the rod was placed under the receiver of an air-pump and the air exhausted. After remaining for a quarter of an hour in a vacuum, air was admitted, and another quarter of an hour was allowed for the action of the atmospheric pressure. At the expiration of this time its resistance was found to have increased to 15,600 ohms, about five times as great as it was originally. The rod was then placed upon blotting-paper; and three days afterwards its resistance was 2970 ohms, the temperature of the air being 16°. This was 200 ohms lower than when it was first made; but the temperature was one degree lower.

Thinking that the oil of turpentine might possibly have had some action on the sulphur, I repeated the experiment with olive-oil. On first immersion, the resistance went up from 2970 to 3150 ohms, and in six minutes to 3770 ohms. It was then left in a vacuum for forty-five minutes, after which the air was admitted, though the effervescence had not even then quite ceased; and when measured, after sufficient interval to allow the oil to penetrate, its resistance had increased to 8140 ohms. The comparative smallness of the effect produced in this latter experiment was probably due to the greater viscidity of the oil.

Lastly, since the behaviour of the sulphur-and-graphite mixture under the influence of heat seemed, like that of the microphone, to depend upon the interaction of contiguous particles of carbon, it was expected that one of the sulphur-cells before described might be used as a telephone-transmitter. This, upon trial, was found to be the case. With a single Leclanché the sounds reproduced in the receiving-telephone were feeble; but well-known nursery-rhymes were easily recognized. When, however, twenty Leclanchés were used, the sounds were much louder and the articulation perfectly distinct. No experiments whatever have yet been made with the view of developing this property; and there can be little doubt that it is capable of extension. It is not impossible that a transmitter constructed upon this principle might be found to be of practical value; and it is entirely different, at least in appearance, from any thing which has been made before.

Fig. 1.

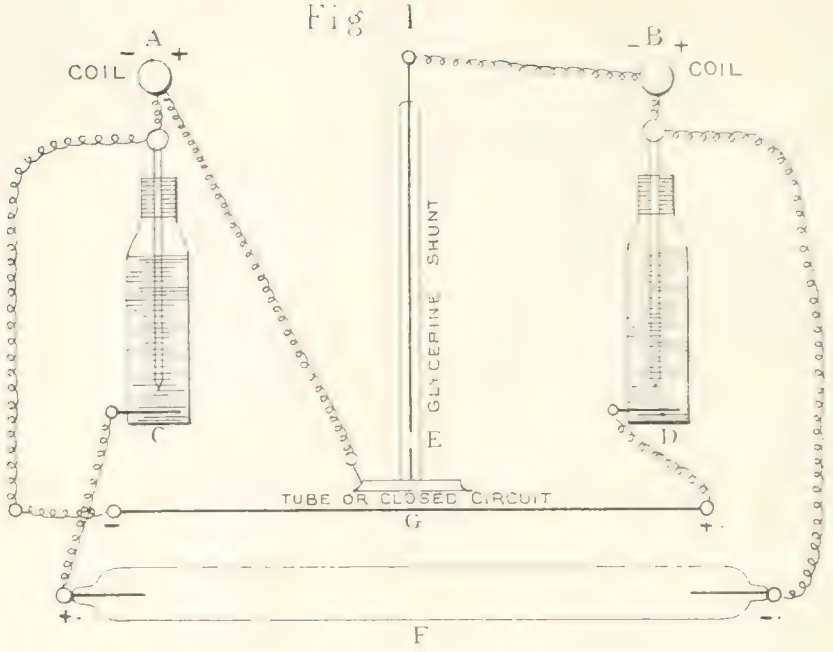
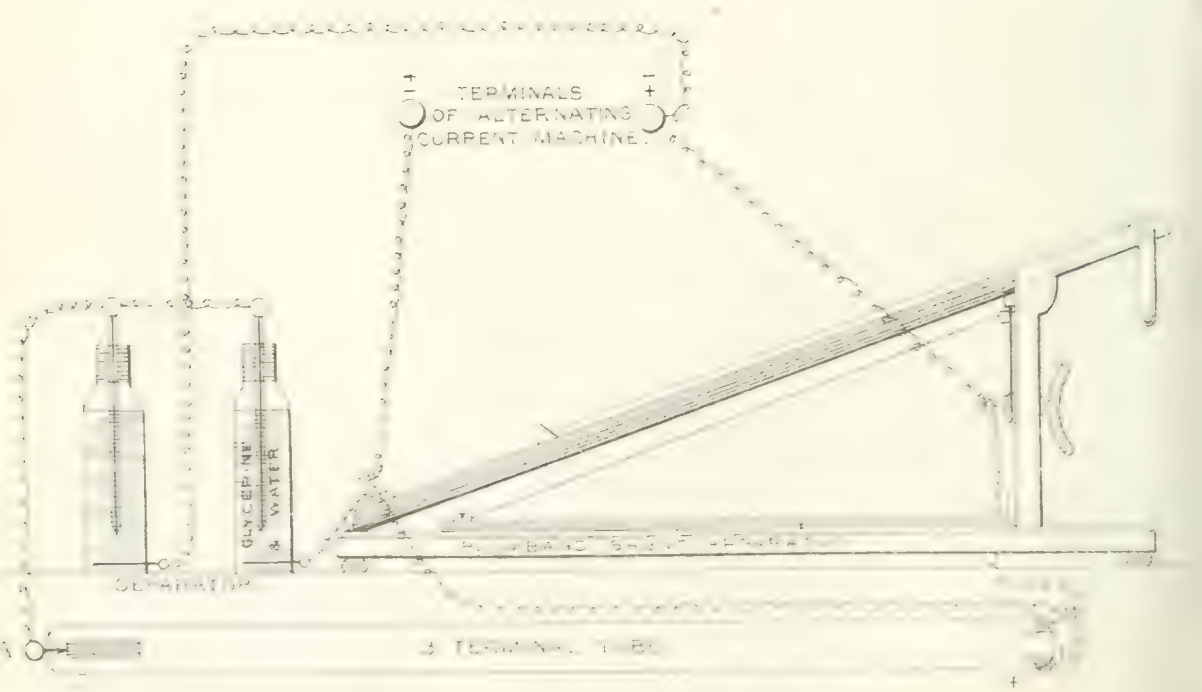

A
B
COIL
COIL
GLYCERINE SHUNT
E
C
D
TUBE OR CLOSED CIRCUIT
G
F

Fig. 2.


TERMINALS
OF ALTERNATING
CURRENT MACHINE.
GLYCERINE
WATER

The sulphur-cell might also perhaps be used as a thermoscope, being simpler and more easy to construct than an ordinary thermopile.

It has recently been proposed to use mixtures of sulphur and graphite for making cheap resistance-boxes. It is needless to point out that the great sensitiveness of such compounds to small changes of temperature renders them very unsuitable for such a purpose.

XLIII. *On a Separator and a Shunt for alternate Currents of High Tension.* *By* WILLIAM SPOTTISWOODE, *M.A., LL.D., D.C.L., President of the Royal Society**.

[Plate IX.]

IN a former communication (Phil. Mag. 1879, vol. viii. p. 390; see also Proceedings of the Royal Institution, March 1882) I described a mode of exciting an induction-coil by means of an alternate magneto-electric machine. The machine then, and at present, used for the purpose was one by De Meritens. It answers its purpose perfectly; but it is not improbable that other alternate machines might also succeed. The method offers various advantages, several of which are described in the second paper to which reference is made above; but it also presents one considerable disadvantage—namely, that the currents produced in the secondary circuit are alternate in direction as well as in time. For some purposes this is of no consequence; but for others, such as experiments with vacuum-tubes, it gives rise to inconvenience by the production of two opposite discharges of the same strength through the tube. As these succeed one another with great rapidity, both sets are visible together, and by their presence they mask the very effects which are the subject of study.

With a view to remedy this defect many contrivances were devised; and among them it is difficult to decide how much is due to my assistant, Mr. P. Ward, and how much to myself. But it is right to state that much credit is due to him for many valuable suggestions in principle, as well as for the instrumental construction.

The first object proposed was to get rid of one set of discharges, either by preventing its passage by means of resistances, or by diverting it from the tube into a different channel. The main principle of which we availed ourselves for this purpose was found in the known property of vacuum-tubes, that a discharge is more easily effected when the negative terminal

* Communicated by the Author.

is large than when it is small. This property is doubtless well known to most persons who have experimented in this field; but I am not aware of any special experiments made for the purpose of examining the subject. De La Rue and others have noticed that with a large negative terminal the amount of surface covered by the glow depends upon the strength of the current; and Goldstein has mentioned that the dimensions of the dark space immediately round the negative terminal depend upon the size of that terminal. Pursuing the question of the connexion between facility of discharge and the size of the terminals, we found not only that the facility depended less on the relative than on the absolute magnitudes, but also that the size of the positive may be reduced indefinitely without interfering with the discharge. Indeed, when we reflect on the appearance of the discharge under ordinary circumstances, and consider that, whatever be the size or shape of the positive terminal, the discharge at that end always proceeds as from a point, then we are almost brought *à priori* to the conclusion that the positive terminal itself may be reduced to a point without affecting the discharge. The reverse, however, is the case with the negative terminal; for, without entering more deeply into the question, it is clear that, if we reduce the surface of that terminal below the extent which would have been covered by the glow when unrestricted, we thereby interfere with the natural development of the discharge. In accordance with this consideration, we found that, when one terminal was small, in that one the discharge which was positive passed freely, while the opposite discharge failed altogether to pass; and thus in this case the problem, as it were, solved itself. The same results were obtained even when one terminal was reduced to a mere point. Acting on this principle, we inserted a tube, having one very small and one large terminal, in series with the tube which was the subject of experiment; and by means of it as an auxiliary tube we succeeded in shutting off one set of currents from the main tube.

Another, although not more successful, accessory for the same purpose was found in a tube furnished with a series of diaphragms each having a small aperture, with a tubular process leading from it and coincident with the axis of the tube. This form of tube is known as a Holtz tube; and it possesses the property of allowing the passage of discharges directed towards the tubular processes, but of impeding the reverse discharges. Speaking more accurately, each of these apertures offers resistance to the passage of the electricity in either direction, but more so to that in which the end of the

tubular process is positive than to the reverse. In some cases this is sufficient to extinguish the discharge. Advantage was taken of this fact, by using a tube furnished with several diaphragms, behind each of which was a terminal which exposed a minute point of metal only. With a stronger current a greater number of diaphragms was used in circuit; with a weaker current, a smaller number; and we were thus enabled to adjust the resistance to the strength of current employed. When several diaphragms were used the resistance became very great.

The Holtz tube, although in some ways very convenient for use, is not without its drawback; for the narrow tubular processes offer some (and not inconsiderable resistance) even to the discharges which they allow to pass. This inconvenience was obviated by substituting for the Holtz tube a column of glycerine and water in which were inserted two terminals, one consisting of a fine point and the other of a plate. The rod which terminated in the point could be raised or lowered according as it was required to increase or to diminish the length of the column, and thereby the resistance employed. By mixing the water and the glycerine in various proportions, the resistance of the column could be varied within very wide limits; and by making the mixture as weak as possible compatibly with the requirements of the case, the instrument, or electrical valve as it may be termed, could be made one of extreme delicacy.

The next step consisted in turning to account the set of currents which had hitherto been merely shut off and not utilized. The arrangement whereby this was effected will be best understood by the diagram (Plate IX. fig. 1). A and B are the two terminals of the coil; C and D, two glycerine-and-water valves; E, a glycerine shunt; F and G, two tubes, or other passages for the currents. When the current, positive at A, enters C, it passes through the valve to the tube F, and thence to the opposite terminal B of the coil. When the current is negative at A, it is unable to pass through the valve C, and it is diverted to the tube G. The same current, positive at B, passes through the valve D, also to the tube G. The complete instrument may be called a separator.

Thus far we have succeeded in separating the two sets of currents, and in diverting one set, so as to make use of them in a second tube or other channel. We have in fact disentangled the two sets; but we have done this at the expence of either making use of only one set, or at best of making use of both in two separate tubes. It is, however, but seldom that more than one tube is required at the same time for experi-

mental purposes; so that, although the last-mentioned arrangement might serve for a display, it can hardly be said to give much promise of practical utility.

We next devised another plan whereby both sets of currents can be turned to account in the same tube with much advantage. If instead of two tubes we take a single tube and furnish it with two sets of terminals, then, if we connect one pair of terminals of the separator with one pair of terminals of the tube, and the other pair of the separator with the other pair of tube, in such a way that both sets of currents enter the tube at the same end, we shall have a double set of discharges in the same direction through the tube.

It was, however, further found that it was not necessary to have four terminals in the tube; for, taking advantage of the difficulty in effecting a discharge when the negative terminal is small, we fixed two very small terminals near one end of the tube and one large one at the other. We then connected the two small terminals directly with the two sides of the machine respectively (that is, before the currents entered the separator), and the one large terminal with the two sides of the machine after the currents had been led through the two branches of the separator respectively. This effected the object desired; for, in the first place, there was no tendency in the discharge to pass from one small terminal to the other on account of the principle above quoted; secondly, there was no tendency in the current to become short-circuited at the single large terminal, on account of the difficulty in the passage of the current backward through either branch of the separator. Consequently every current issuing from the machine was obliged to pass to one or other of the small terminals, and thence through the tube to the large terminal at the other end. The scheme of the connexions to the various parts of the apparatus will, however, be best understood by reference to the diagram (Plate IX. fig. 2).

The arrangement may therefore be said to have completely solved the problem of producing unidirectional discharges through a tube from an alternating machine, and at the same time utilizing all the currents from the machine. The method has also the advantage of making the number of currents discharged through the tube in a given time double that which would have been discharged if one set of currents had been merely diverted as in the arrangement first described.

But beside shunting off, or diverting, one set of currents, it was often desirable to reduce the strength of the whole to suit the condition of the tube or the circumstances of the experiment. The readiest method of effecting this appeared to be

to introduce a resistance in the primary circuit. But this proved to be wanting in the delicacy required; and it was found necessary to operate directly on the secondary. Mr. De La Rue had, it is true, already constructed a platinum-wire resistance of 1,000,000 ohms; but even this amount was insufficient for our present purposes, while its cost was almost prohibitory. Columns of acidulated water proved very effective; but they were open to the objections, first, of heating during use, and, secondly, of evaporation of the water at all times. Both of these actions tended to diminish the resistance of the column.

Mr. De La Rue had also constructed a resistance, consisting of a thin film of plumbago spread upon ebonite; but the current heated the plumbago to such an extent that the ebonite was burnt. The instrument in question was therefore never used. Mr. Ward suggested the employment of slate instead of ebonite; and accordingly he constructed the instrument actually in use, and represented in fig. 2. The slab of slate is about two feet in length and two inches in breadth. It is fixed at an angle of about 30° to the horizon, the inclination being so adjusted that a block of lead, which serves as a contact piece, will slide freely down the inclined plane. A fine wire attached to the block, and passing over a pulley at the upper end, regulates the length of the plumbago actually in circuit. It is found most convenient to use this instrument (which proves to be one of great delicacy) as a shunt.

A more convenient form of this instrument consists of a vertical rod of ground glass, porcelain, or other incombustible nonconductor, having its surface slightly roughened so as to hold the plumbago. One terminal consists of a double slider capable of being elevated or depressed, and making good contact, either by a spring or by gravity, with the plumbago film.

XLIV. *Note on Transformation.*

By Sir James Cockle, *M.A., F.R.S., F.R.A.S., &c.**

54. IN my recent papers (see the Numbers for September 1881 and for January last) the details of calculation are kept within narrow limits. There are, however, various independent sources of verification, viz. substitution, reducibility of radicals, comparison of formulæ with other results and with each other, and, finally, an actual transformation.

55. In art. 2 put $e=a$; the formulæ of arts. 8, 9 hold for all values of n. Put $a, f, h=c, g, k$; the like is true, and the

* Communicated by the Author.

transformed equation is free from x^n. Put $a, e=1$ and $h, k=0$; the factors are in arithmetical progression, as, by art. 13, they should be. Put $A, E=1, 2$; then art. 19 gives

$$P_2=0, \quad Q_2=4+9M-12L, \quad R_2=32+18N-3M,$$
$$S_2=6N+12.$$

Hence

$$2B_2=16+12(L-M+N)=16+12b,$$

and

$$6C_2=12L-6M \text{ or } C_2=b+L-N;$$

and the first two conditions of art. 20 are satisfied, as is the last; for, by art. 18, $b_2=b+2$.

56. Since η and η_2 are to be taken as one-valued when the quantities in terms of which they are expressed are so taken, we infer that the radical in arts. 10 and 17 means the appropriate root, not either root indifferently, and that the root can be rationally expressed. This is the case; irrationality has disappeared from all our results.

57. Multiply the products $\alpha_1\alpha_2$ and $\beta_1\beta_2$, wherein the factors are in order of magnitude, into γ. If we preserve the order, we get six configurations, viz. $\gamma\alpha_1\alpha_2$, $\alpha_1\gamma\alpha_2$, $\alpha_1\alpha_2\gamma$, and $\gamma\beta_1\beta_2$, $\beta_1\gamma\beta_2$, $\beta_1\beta_2\gamma$. Assume that there is a value of γ which will make one of the α set and also one of the β set an arithmetical progression. We get nine mutually exclusive but severally possible pairs of relations, of which one, for example, is

$$\alpha_2+\gamma=2\alpha_1, \quad \beta_2+\gamma=2\beta_1;$$

whence

$$4\alpha_1-2\alpha_2=4\beta_1-2\beta_2 \text{ or } 3I=3J+U. \quad . \quad . \quad (14)$$

Let α be taken to represent $D-\alpha$, and we thus obtain by Boole's algorithm a result included in those of art. 50. The same thing holds for the remaining eight cases.

58. This last verification indicates that a factor always disappears from (13), and that the solution $\eta_2=-1$ must be expunged from art. 53; for, by art. 36, $\omega=0$. In the numerator of (13) of art. 50 change D into $D-3$, or, rather, omit the transformation by which D has replaced the $D-3$ which would otherwise have remained there. A factor will disappear if any one of the nine expressions $4\omega\pm3(J\pm I)$, $4\omega\pm3I$, $4\omega\pm3J$, ω vanishes. But these conditions coincide with those of arts. 33, 34, and with those implied in art. 57.

59. The introduction of the arithmetical progression, while it affords a good verification, restricts generality. Putting

$$B=A-\tfrac{1}{2}, \quad \Delta=D+a_2-1, \quad z=x^{a_2}X^{-\frac{\gamma_2}{3}}\zeta,$$
$$f(\Delta, B, I)=(\Delta+2B)(\Delta-B+\tfrac{3}{4}I)(\Delta-B-\tfrac{3}{4}I),$$

I find that the binomial terordinal deducible from that in z (arts. 16, 17) is

$$f(\Delta, \mathrm{B}, \mathrm{I})\zeta + f(\Delta+2\omega, \mathrm{B}-\omega, \mathrm{J})x^3\zeta = 0, \quad . \quad . \quad (15)$$

from which a factor, viz. $\Delta+2\mathrm{B}$, may be made to disappear.

60. Getting rid of this factor, putting $\mathrm{Z}=x^{-\mathrm{B}}\zeta$, and a_2 (which is arbitrary) $=1$, and changing x^3 into x^2, we are led to

$$(\mathrm{D}+\tfrac{1}{2}\mathrm{I})(\mathrm{D}-\tfrac{1}{2}\mathrm{I})\mathrm{Z} + (\mathrm{D}+2\omega+\tfrac{1}{2}\mathrm{J})(\mathrm{D}+2\omega-\tfrac{1}{2}\mathrm{J})x^2\mathrm{Z} = 0.$$

61. But, the restriction entailed by the progression being removed, we can employ both the values of ω given in art. 42, and so obtain two equations either of which is a transformation of the other.

62. In verification I add that, by a further application of Boole's process, these two equations may be put under the respective forms

$$(\mathrm{D}-\beta_1)(\mathrm{D}-\beta_2)u + (\mathrm{D}-\alpha_1)(\mathrm{D}-\alpha_2)x^2u = 0,$$

$$(\mathrm{D}-\beta_1)(\mathrm{D}-\beta_2)u_2 + (\mathrm{D}+\mathrm{U}-\alpha_1-2)(\mathrm{D}+\mathrm{U}-\alpha_2-2)x^2u_2 = 0,$$

and that one of these forms is a transformation of the other, viz. $u_2=(1+x^2)^{1-\frac{\mathrm{U}}{2}}u$.

63. If in $b_2=-\eta_2(\eta_2+3)$ we for η_2 substitute $3-b_2^{-1}\mathrm{B}_2$ and reduce, we get

$$b\{b+3\omega(\omega+1)\}\{b+3(\omega+2)(\omega+3)\}=0; \quad . \quad . \quad (16)$$

and from $(\mathrm{N}_2-\mathrm{L}_2)^{-1}(\mathrm{C}_2+b_2)=3-b_2^{-1}\mathrm{B}_2$ we get

$$\{b+3\omega(\omega+1)\}\{b(\Omega-1)+(\omega+3)(\mathrm{N}-\mathrm{L})\}=0. \quad . \quad (17)$$

64. Both (16) and (17) are satisfied if $b+3\omega(\omega+1)=0$. This is the solution which I have discussed. They are also both satisfied if $b=0$ and $\omega+3=0$. This solution is inadmissible; for it would make η_2 a vanishing fraction. There remains the system

$$\left.\begin{aligned} b+3(\omega+2)(\omega+3)&=0, \\ b(\Omega-1)+(\omega+3)(\mathrm{N}-\mathrm{L})&=0; \end{aligned}\right\} \quad . \quad . \quad (18)$$

which is important, but which I shall not discuss in these pages. The latter solution of art. 26 is not irrelevant to the system (18).

2 Sandringham Gardens, Ealing,
March 14, 1882.

Erratum.

Vol. XII. p. 196, art. 36, lines 10, 11, transpose "one is" and "two are" and in line 12 *dele* integers.

XLV. *On the Fluid Density of certain Metals.* *By* W. Chandler Roberts, *F.R.S.* *and* T. Wrightson, *Memb. Inst. C.E.**

[Plate VII.]

At a Meeting of the Physical Society, on February 12, 1881†, we communicated the results of certain experiments, made with the aid of the oncosimeter, on the fluid density of bismuth, in which we showed that the densities in the fluid and cold solid states are respectively 10·055 and 9·82, the fluid density thus being 2·4 per cent. higher than the cold solid. These determinations correspond closely with results that had been obtained by one of us, using the "cone" method devised by Mr. Mallet‡. For the sake of convenience we here reproduce our results on bismuth.

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Floating effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
31.	2	23·33	9·72	1·0	10·13	Bismuth ball in fluid bismuth.
32.	2·25	22·184	6·99 (iron)	9·3	9·92	Iron ball in fluid bismuth.
33.	do.	33·46	9·755	1·3	10·11	Bismuth ball in fluid bismuth.
34.	do.	33·37	9·757	·6	9·94	do.
35.	do.	33·53	9·774	·7	9·98	do.
36.	do.	22·184	7·02 (iron)	10·2	10·25	Iron ball in fluid bismuth.
					Mean ..... 10·055	
					Specific gravity of solid bismuth ..... 9·82	

Since these results were published we have extended the experiments to the following metals—copper, lead, tin, zinc, silver. The results are now submitted to the Society.

* Communicated by the Physical Society, having been read January 28, 1882.

† Proc. Phys. Soc. vol. iv. p. 195; and Phil. Mag. [5] vol. xi. 1881, p. 295.

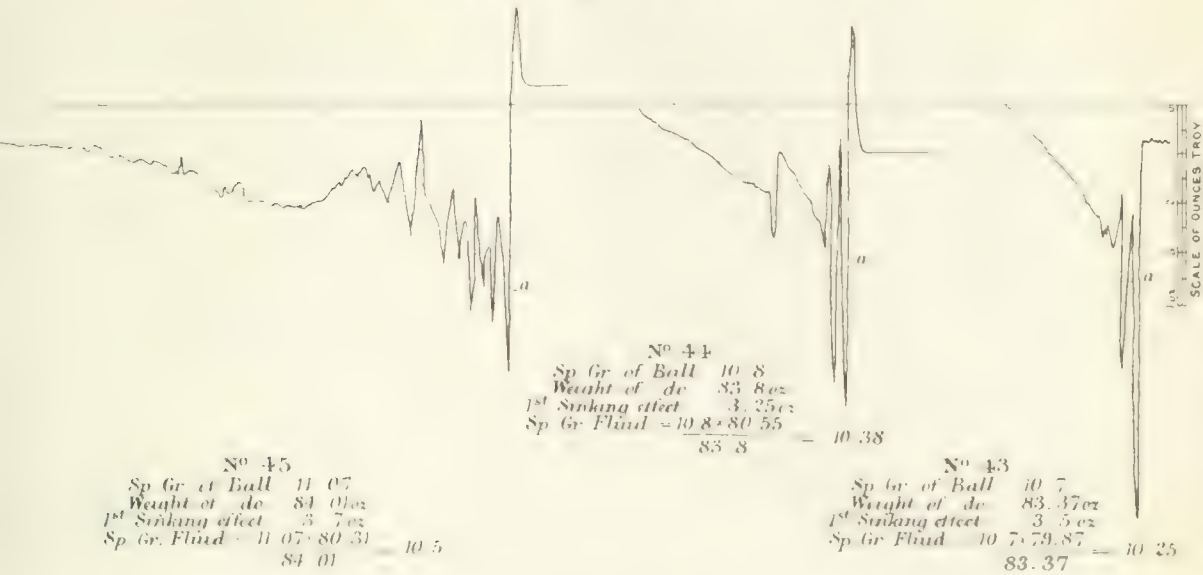
‡ Proc. Roy. Soc. vol. xxii. p. 366, and vol. xxiii. p. 209.

DIAGRAM N° 1

May 6th 1881

Experiment on 3˝ Lead Balls melted in Fluid Lead

Note The lead was very hot during Exp^{t} 43 and gradually cooled during Exp^{ts} 44 & 46



N° 44
Sp Gr of Ball 10 8
Weight of do 83 8 oz
1st Sinking effect 3 25 oz
Sp Gr Fluid = $\frac{10\ 8 \times 80\ 55}{83\ 8} = 10\ 38$

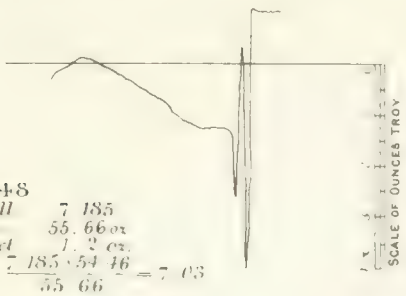
N° 45
Sp Gr of Ball 11 07
Weight of do 84 01 oz
1st Sinking effect 3 7 oz
Sp Gr Fluid = $\frac{11\ 07 \times 80\ 31}{84\ 01} = 10\ 5$

N° 43
Sp Gr of Ball 10 7
Weight of do 83 37 oz
1st Sinking effect 3 5 oz
Sp Gr Fluid = $\frac{10\ 7 \times 79\ 87}{83\ 37} = 10\ 25$

DIAGRAM N° 2

May 18th 1881

Experiment on 3˝ Tin Ball melted in Fluid Tin

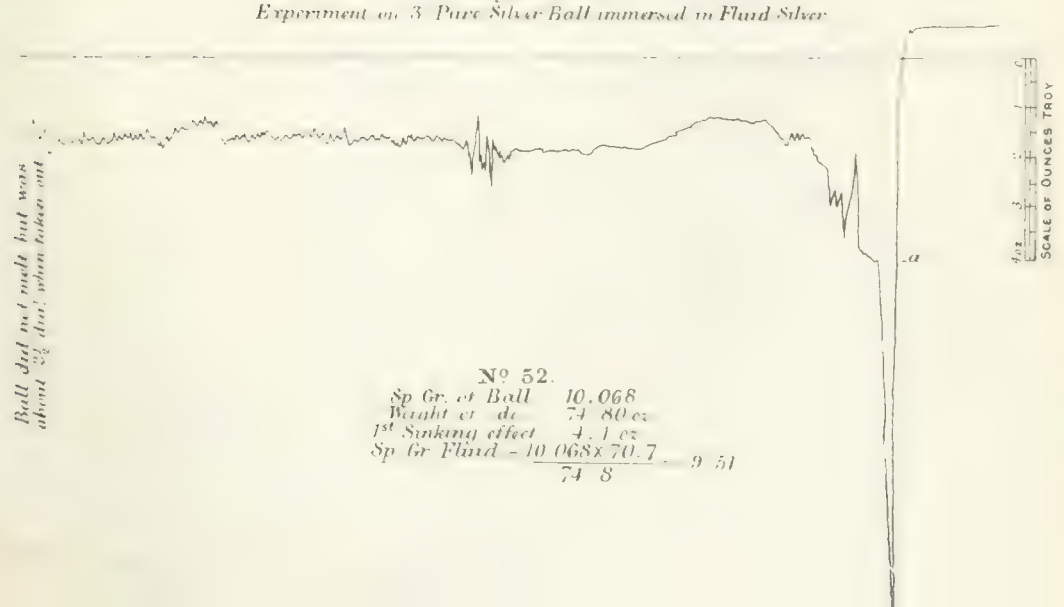


N° 48
Sp Gr of Ball 7 185
Weight of do 55 66 oz
1st Sinking effect 1 2 oz
Sp Gr Fluid = $\frac{7\ 185 \times 54\ 46}{55\ 66} = 7\ 03$

DIAGRAM N° 3.

Aug 18th 1881

Experiment on 3˝ Pure Silver Ball immersed in Fluid Silver



N° 52.
Sp Gr. of Ball 10.068
Weight of do 74 80 oz
1st Sinking effect 4 1 oz
Sp Gr Fluid = $\frac{10\ 068 \times 70\ 7}{74\ 8} = 9\ 51$

Copper.

Six experiments were made with the oncosimeter* on copper; four of these were made with cast copper balls $2\frac{1}{4}''$ diameter, one with a cast-iron turned ball $2\frac{1}{4}''$ in diameter, and one with a copper ball $3''$ in diameter. The first four were made with a spring that lengthened $\frac{1}{4}$ inch per ounce troy; the last two with a more sensitive spring, giving $\frac{1}{2}$ inch elongation per ounce troy. It will be seen by the accompanying table that the fluid density does not vary much, although, as has just been stated, the conditions were varied.

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
37.	2·25	26·357	8·23	nil.	8·23	Copper ball in fluid copper.
38.	do.	27·005	8·463	·2	8·4	do.
39.	do.	26·25	8·288	·5	8·13	do.
40.	do.	25·923	8·281	·2	8·217	do.
41.	do.	22·186	7·02 (iron)	Floating effect 3·5	8·127	Iron ball in fluid copper.
42.	3	64·237	8·61	Sinking effect 3·0	8·2	Copper ball in fluid copper.
					Mean ..... 8·217	
					Specific gravity of solid copper ..... 8·8	

The mean of these results gives 8·217 as the density of fluid copper. This metal melts at a temperature which exceeds 1000° C., and, when melted in air, absorbs oxygen. We found, for this reason, that it was not possible to obtain trustworthy results by Mallet's method, even though the cone was filled with an atmosphere of coal-gas.

* A description of this instrument was published in the Journal of the Iron and Steel Institute, part 2, 1879, p. 418.

Lead.

Three experiments were made with the oncosimeter, the results being as follows:—

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
43.	3	83·37	10·7	3·5	10·25	Lead ball in fluid lead.
44.	3	83·8	10·8	3·25	10·38	do.
45.	3	84·01	11·07	3·7	10·5	do. Fluid lead getting cold.
					Mean	10·37
					Specific gravity of solid lead	11·4

A mean of two experiments on lead, made on Mallet's system, gave 10·65 as the liquid density, the results being 10·63 and 10·66 respectively.

The diagram No. 1 (Plate VII.), gives the results of the experiments on lead; and the calculations of fluid density are in each case attached, the point *a* indicating in each the position taken as giving the true initial sinking effect at the moment of immersion.

Tin.

Two experiments were made with the oncosimeter, with the following results:—

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
46.	3	55·66	7·165	1·1	7·02	Tin ball in fluid tin.
47.	Failure.					
48.	3	55·66	7·185	1·2	7·03	do.
					Mean	7·025
					Specific gravity of solid tin	7·5

The diagram No. 2 is reduced from the oncosimeter diagram, experiment 48.

A mean of three experiments on tin, made on Mallet's method, gave 6·974 as the liquid density—the results being 6·903, 6·982, and 6·976 respectively.

Zinc.

Three experiments were made with the oncosimeter on this metal, the results being tabulated below:—

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
49.	3	52·52	6·893	2·4	6·57	Zinc ball in fluid zinc.
50.	3	52·58	6·901	3·3	6·47	do.
51.	3	52·32	6·903	3·75	6·4	do.
				Mean	6·48	
Specific gravity of solid zinc 6·8 to 7·2 (Rankine).						

The crucible was kept in the furnace during the three experiments, and maintained as nearly as possible at the same temperature throughout.

Only one determination of this metal has been made by Mallet's method, giving a liquid density of 6·55.

Silver.

We looked with much interest to the experiments on the density of molten silver for several reasons—mainly from the fact that it melts at a high temperature, which has been determined with great care by the late Henri Ste Claire Deville, who in his later experiments on the subject fixed the temperature at 940° C.* We operated on a thousand ounces of silver, which proved on assay to contain 998 parts of pure silver in 1000. We have as yet only secured one trustworthy result, as we found it very difficult so to adjust the temperature of the molten silver as to prevent the silver balls from melting either too rapidly or too slowly. In the experiment No. 52, in which we did secure a result, the metal must have been just about its melting-point, as at the end of four

* *Comptes Rendus*, t. xc. (1880) p. 773.

minutes the silver ball had not entirely melted; the experiment was stopped, however, as the silver began to solidify on the iron stem of the ball.

The diagram No. 3 is reduced from the original; and we have appended the calculation for fluid density, assuming the position of equilibrium at first immersion of the ball to be at point *a*.

The particulars of the experiment are as follows:—

No. of expt.	Diameter of ball. in inches.	Weight, in troy ounces. including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
52.	3	74·8	10·068	4·1	9·51	Pure silver ball in pure silver.

The liquid density of pure silver, as determined by one of us*, on Mallet's method, from a mean of two experiments, giving respectively 9·447 and 9·476, was 9·46. This confirms the single result obtained by the oncosimeter.

If we now tabulate the mean results of the foregoing experiments, as well as those previously obtained, we shall be able to compare the densities according to the two methods of investigation:—

Fluid density of	By Mallet's method.	By oncosimeter.
Bismuth	10·039	10·055
Copper	.....	8·217
Lead	10·65	10·37
Tin.....	6·974	7·025
Zinc	6·55	6·48
Silver.....	9·46	9·51

The difficulties connected with the working of the oncosimeter are less than those which arise in Mallet's method; and the sources of error are less, the correctness of the result in the latter depending on the metal in the cone being free from air-holes.

It will be seen from the above table that, in the case of

* Roberts, Proc. Roy. Soc. vol. xxiii. p. 493.

the five metals in which both systems were tried, the results correspond as closely as could be expected, taking into consideration the great difficulties of observation at the high temperatures employed. We therefore venture to think that the oncosimeter can be depended upon generally for the determination of the fluid densities of metals, if the experiments are conducted with the necessary care.

Taking therefore the oncosimeter-results as approximately accurate, we find the change of volume of these metals in passing from the cold solid to the liquid state to be as follows :—

Metal.	Specific gravity of solid.	Specific gravity of liquid.	Percentage of change in volume from cold solid to liquid.
Bismuth	9·82	10·055	Decrease of vol. 2·3
Copper	8·8	8·217	Increase of vol. 7·1
Lead	11·4	10·37	do. do. 9·93
Tin	7·5	7·025	do. do. 6·76
Zinc	7·2	6·48	do. do. 11·1
Silver	10·57	9·51	do. do. 11·2
*Iron (No. 4, foundry, Cleveland)	6·95	6·88	do. do. 1·02

The experiments just described lead us to believe that, although the conditions under which they are made may render it impossible to obtain results that are rigidly accurate, still the errors are inconsiderable. It has been urged that the unsoundness or porosity in the casting would disturb the accuracy of the results ; but this is provided against in each case by taking the specific gravity of the ball operated on, its iron stem being submerged in water to a point which is afterwards just reached by the molten metal. It has also been suggested that the expansion of the ball, when it enters the fluid metal, causes tension on the metal in the interior of the ball, and that therefore the density of the ball (from which the fluid density is determined) can never be trusted. It must be remembered, however, that the determination of the fluid density is made and automatically registered at the moment of immersion, before the volume of the ball can be practically affected.

Since our last paper was read, we have seen that MM. F. Nies and A. Winkelmann† have been investigating the changes in volume of certain metals when melted. Their method will

* Wrightson, Journal of the Iron and Steel Institute, No. 1 (1880), p. 20.

† *Sitzungsberichte der Akademie der Wissenschaften zu München*, 1881, part 1, p. 63.

be best understood by a short description of the way in which the metal tin was treated by them.

Tin was melted in a suitable vessel, the melting-point (viz. 226°·5 C.) being carefully maintained. Pieces of tin were then dropped in, and observed first to sink, then to rise before melting—showing that solid tin has a higher specific gravity than the liquid mass, but that when its temperature has been raised to the melting-point, or rather to a certain temperature just below, then it is specifically lighter. In order to determine this relation, they enclosed pieces of copper (having a higher specific gravity than tin) in pieces of tin, and were thus able to increase the specific gravity of the solid piece at will; and although not able to determine directly whether the compound piece was of the same density as the liquid, the latter not being transparent, yet by varying the amount of copper enclosed in the tin, thus forming pieces of a different density, they were enabled to say that it lay between the two limiting values. Compound pieces with varying and known amounts of copper, thus formed, were placed on the surface of the liquid tin for some little time, then dipped under the surface; and it was noted whether they rose again or not. The pieces were each then carefully taken out again and weighed, to ascertain that no part had been melted. This was continued until they found two compound pieces such that, at a temperature just below the melting-point, one would just rise and the other would just sink.

If, then, the relative densities of the liquid tin and the copper at the temperature of liquid tin could be ascertained by experiment, the relative density of the solid tin just below melting-point and the liquid tin can be calculated.

A copper ball was heated to the temperature of the melting tin, and weighed, first in air, then in the liquid tin. The relative densities being thus ascertained, the calculations, which we need not give here, are of a simpler character.

In the case of tin, they found that the liquid is of ·7 per cent. greater density than the hot solid at a temperature just below melting-point.

This, so far as it goes, is confirmatory of the results obtained by us. If diagram No 2. be examined, it will be seen that the line of volume rises gradually to the line of equilibrium, indicating expansion; and just before the ball melts rises above the line, indicating a floating effect, when the temperature approximates to the melting-point.

Tin therefore appears to be similar to iron ; viz. it is at its maximum density when cold, and at its minimum density when at a temperature just below melting, and that the fluid-density is between the two.

The results obtained by MM. Nies and Winkelmann on bismuth will serve to illustrate the degree of approximation of these limiting values.

A piece of bismuth, when allied with a platinum block weighing 21·76 grammes, sank; and another piece, weighing 35·4 grammes, floated; which is equivalent to stating that the specific gravity of the melting bismuth is between 10·28 and 10·12.

They arrive at the general result that not one of the eight metals they examined will justify the assertion that "bodies contract on becoming solid;" but the experiments rather favour the view that metals when solid, *at a temperature close upon their melting-points*, are less dense than when molten.

Without accepting MM. Nies and Winkelmann's results as final, we do not consider them to be opposed to our own, as theirs relate solely to the ratio of the densities of the solid and liquid metals at as nearly as possible the same temperature, while our experiments were undertaken with a view to determine the actual density of a metal at the *lowest* temperature at which it is perfectly liquid.

XLVI. *Notices respecting New Books.*

Geology of the Environs of Tokio.

By Prof. David Brauns, *Ph.D.*, *M.D.* Tokio: 1881.

THE present memoir is one (No. 4) of a series published by the University of Tokio, and contains a description of the Geology of the Environs of Tokio, which are part of an extensive plain, the largest in the Japanese Empire, everywhere constructed on the same plan, and showing essentially the same formations, which consist of alluvial, diluvial, and Tertiary deposits; the bounding hills and mountains, which form a vast quadrant, are composed of Palæozoic and various crystalline rocks.

The plain or plateau of Tokio, although not much elevated, is intersected by deep cuttings of river-valleys and ravines with much alluvial matter; from which it occurs that all the diluvial parts of the plain appear as isles or peninsulas, divided from one another by those river-valleys and side valleys down to the most minute undulations of the ground. This division of the surface of the plain is of the highest importance for agriculture—the rice-fields being in most instances confined to the lower or alluvial tracts, whilst on the higher level we find the cereals, many kinds of beans, the plantations of tea, of the mulberry-tree, and most of the small forests. According to the author, this widely spread plateau must have been deposited by the sea and under its surface, and therefore must have risen above the level of the sea since the diluvial epoch in which it was formed; for it is to be noticed that in no part of it is there any trace of glacial deposits or of glacial action.

The alluvial and so-called diluvial deposits are described in the second and third chapters; in the latter, organic remains are not very numerous in the environs of Tokio: there are plant-remains of surviving species, and land, freshwater, and marine Mollusca of existing Japanese forms. The remains of mammals are rather limited: bones of Deer and Cetacea occur; also two species of Elephants, which are referred to *Elephas meridionalis* and *E. antiquus*, forms belonging to the Glacial and Preglacial fauna of Europe.

The succeeding chapters (4–7) treat of the Tertiary deposits of Oji, Tokio, Yokohama, and other parts of Japan, and of the Mollusca found in them, which are all living species; many, however, are not now found in the neighbourhood of Japan, nor even in the Pacific. In many respects, Dr. Brauns says, the Japanese shell-layers have the greatest resemblance to the *Crag*, and, next to it, to the younger Subapennine deposits, while the rocks resemble very closely the European *Faluns*.

With the memoir are given some sections and a sketch map of the environs of Tokio, and six plates of fossil shells, which are highly creditable to the Japanese artist, Mr. H. Hirauchi, who designed the illustrations.

The Theory of Equations; with an Introduction to the Theory of Binary Algebraic Forms. *By* W. S. Burnside, *M.A.*, *and* A. W. Panton, *M.A.* Longmans, Green, and Co.: London. Pp. 387.

Students of Mathematics are greatly indebted to the authors of this work for having combined in one volume the substance of the best standard books on the subject, with the improvements and extensions it has received at the hands of modern investigators, whose memoirs have hitherto been chiefly contributions to various Mathematical Journals. The first ten chapters comprise all the essential propositions included in the best treatises on the Theory of Equations. In the more advanced portions of the subject, Chapter XI., on Determinants, and Chapters XIV. and XV., on Covariants and Invariants, are distinguishing features of the work. To beginners these chapters are of the greatest value, not only on account of the simple and lucid manner in which they are written, but the care which has been taken to place before the student the most recent improvements in this Calculus.

The book is unusually well got up in every respect. The type, which is of various kinds, is remarkably clear: and the book altogether a triumph of the printer's art.

Celestial Objects for Common Telescopes. *By the* Rev. T. W. Webb, *M.A.*, *F.R.A.S.* Fourth Edition, 493 pp., with numerous figures. London: Longmans, Green & Co. 1881.

This excellent book, every page of which teems with information, is unquestionably not only the best and handiest companion for the observatory, but also for the amateur who studies astronomy for mere amusement. The questions, What is to be looked for? and How is it to be looked for? are abundantly answered in its pages.

And we know that in the compilation of the work this was the main object of its author. On opening it for the first time, the student cannot do better than turn to Appendix I. and make the corrections indicated there. Marked and important features of the manual are a map of the Moon, and a clear and not overcrowded index to it, Double Stars, Clusters, and Nebulæ, from Struve's famous 'Catalogue,' Smyth's 'Cycle of Celestial Objects,' and the more recent accurate observations of Burnham and others; while an Appendix contains the same objects placed, for the sake of convenience, in the order of their R.A.

The labour bestowed on Part III. and the Appendices must have been enormous; and great praise is due to the author for the way in which it has been accomplished. Some at least of the materials used in compilation must themselves have caused great trouble by the careful sifting they needed. Smyth's 'Cycle of Celestial Objects,' for instance, is a work not to be relied on for general accuracy.

With regard to Part II., we may note, *en passant*, what appear peculiar faults of omission and commission when we consider what a surprising amount of detail is given elsewhere. No mention is made of Mercury's distance (35,500,000 miles) from the Sun. Neither the dimensions of Venus nor her distance from the Sun are given. On page 172 a very curious and puzzling description of Saturn is given. Part of it runs thus:—"The Globe. Though about 69,200 m. in equatorial diameter, second only to Jupiter, and about nine times larger than the earth," &c. We are afraid that young students, not knowing that spheres are to one another as the cubes of their diameters, would take this sentence to mean that Saturn is only nine times the size of the Earth. The rest of the sentence is simply beyond human comprehension. However, the book amply fulfils its object, and ought to find a place wherever an astronomical telescope is in use, whatever its range may be.

XLVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 147.]

January 25, 1882.—R. Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Fossil Fish-remains from the Armagh Limestone in the Collection of the Earl of Enniskillen." By James W. Davis, Esq., F.G.S., F.L.S.

2. "On an extinct Chelonian Reptile (*Notochelys costata*, Owen) from Australia." By Prof. Owen, C.B., F.R.S., F.G.S.

3. "On the Upper Beds of the Fifeshire Coal-Measures." By the late E. W. Binney, Esq., F.R.S., F.G.S., and James W. Kirkby, Esq.

These beds are best exposed in Fife, between the mouth of the

Leven and East Wemyss. The coal-field of the latter district is the most northerly piece of true Coal-measures in Britain; they dip 10° E., or even less. The highest member (which may possibly be even later than the Carboniferous Period) is an unfossiliferous purplish sandstone, full 200 feet thick; next comes soft red marl, 12 feet, followed by sands (290 feet), marls, clays, &c. (100 feet), and sandstones, clays, &c., with thin coals and limestones (about 80 feet). Other sections were also described. No unconformity has been noticed in Fifeshire between these and the underlying measures. The argillaceous limestones and their coals are fossiliferous; there are a few fish and crustacea, and many plants, with some curious markings, which may be rootlets or perhaps algæ. The last occur only in a limited part of the rock, not with other plants, and are very delicate and membranous. The affinities of these were fully discussed, the author inclining to the view that they are algæ.

February 8.—R. Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Description of some Iguanodon Remains discovered at Brook, Isle of Wight, indicating a New Species, *Iguanodon Seelyi*." By J. W. Hulke, Esq., F.R.S.

2. "On a peculiar Bed of Angular Drift on the high Lower-Chalk Plain between Didcot and Chilton." By Prof. J. Prestwich, M.A., F.R.S., F.G.S.

February 22.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Additional Discoveries of High-level Marine Drifts in North Wales, with Remarks on Driftless Areas." By D. Mackintosh, Esq., F.G.S.

The author begins with remarks on the importance of the marine drift-area (part of which he briefly described in his last paper), especially as regards its great extent, and the absence, so far as yet known, of similar high-level drifts (between 1000 and 1350 feet above the sea) in continental Europe, Asia, or North America. He lately traced the drift-area two miles further south than he had done during former explorations, its entire length being little short of 5 miles. In this paper he gives a detailed description of the numerous exposures of rounded gravel and stratified sand between the north end of Minera Mountain and Llangollen Vale, which in some places spread out into large flat expanses, but more frequently assume the form of knolls (frequently in perched positions), which rise up from beneath a covering of clay or peat. He dwells on the probable origin of the knoll-shaped configuration, including the theory of the precipitation of the drift from the stranding of floating ice, and the forcing-up of previously deposited drift by the same agency, but inclines to the idea of the knolls having been chiefly

accumulated by sea-currents. The author then describes several large areas in North Wales in which he could find no trace of rounded gravel, enters into a consideration of the causes of these driftless areas, and discusses the relative merits of the theory of their having been temporarily occupied by land-ice, and of the theory of non-exposure to tempestuous seas, or seas capable of rounding stones. He then gives an account of the discovery of granite boulders, associated with partially rounded drift, on the summit of Moel Wnion, 1900 feet above the sea (near Aber, North Wales); and endeavours to show that, while they could have been readily transported by floating ice (probably from Scotland, certainly not from Cumberland), the flow of land-ice from Snowdon, according to Ramsay, along the north face of Moel Wnion, must have prevented the access of northern land-ice to the summit of the latter mountain, while land-ice flowing from Cumberland to Anglesey (according to Ramsay) could not have been crossed by land-ice flowing south from Scotland to Moel Wnion. After referring to the outward direction of striæ on the north coast of North Wales, he concludes by giving a summary of facts and inferences.

2. "On some Sections of Lincolnshire Neocomian." By H. Keeping, Esq., of the Woodwardian Museum, Cambridge.

The construction of the Louth and Lincoln railway, in the year 1872, led to the exposure of some fine sections of the Neocomian strata forming the base of the Lincolnshire Wold. From these beds the author had obtained a very large number of fossils, in many cases in an excellent state of preservation. As the sections are now becoming obscure, the author gives an account of the observations made by him at the time when they were best exposed, with lists of the fossils he obtained from them. He agrees with Professor Judd, who in 1867 first described these strata and determined their age, in his conclusions concerning the classification and correlation of the beds, but is disposed to regard the sands above Market Rasen as being a drift deposit, and not as belonging to the Lower Sand and Sandstone.

3. "Notes on the Geology of the Cheviot Hills (English side)." By C. T. Clough, Esq., M.A., F.G.S.

After a brief description of the physical structure of the district, the author passes on to its igneous rocks. These he groups under three heads—(1) the granite, (2) the lava-flows and ash beds, (3) the intrusive dykes. The first occupies an area of about 24 square miles, it varies much in texture, is generally not conspicuously rich in quartz, and often contains hornblende as well as mica. It exhibits in places a curious streaked or banded structure due to local crushing. This is fully described. The lava-flows (with ashes) are porphyrite; this is generally compact, sometimes glassy in structure, with scattered crystals of plagioclase, biotite, hornblende (sometimes augite), a little free quartz, and occasional olivine and apatite. Some peculiarities in these rocks are described. The intrusive dykes

(3) consist of (*a*) coarse red porphyrite, (*b*) quartz porphyry, (*c*) felsite, (*d*) granite. The author considers that the porphyrite and granite (which are alternately intrusive one in another) belong approximately to the same geological epoch. This is the Lower Old Red Sandstone: and as pebbles of the granite as well as of the porphyrite occur in the Lower Carboniferous measures of the region, great denudation must have taken place prior to the latter epoch. The author also describes some basalt dykes which he considers to be of Miocene age.

March 8.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Additional Note on certain Inclusions in Granite." By J. Arthur Phillips, Esq., F.R.S., F.G.S.

The author referred to certain rounded inclusions in granite which were rich in mica. These he had described in his paper published in vol. xxxvi. of the 'Quarterly Journal,' and had considered to be contemporaneous segregations from the molten rock. He had, up to that time, not found a case where one of the larger crystals of felspar in a porphyritic granite occurred partly in the one, partly in the other. Of late he had seen several, one of which he described minutely, thus proving the correctness of his supposition.

2. "The Geology of Madeira." By J. S. Gardner, Esq., F.G.S.

Madeira consists almost wholly of sheets of basaltic lava of variable thickness interstratified with tuff scoriæ and red bole, cut by innumerable dykes. In the central part of the island is a horse-shoe-shaped valley, more than 4 miles in diameter, its bed 2500 feet above the sea, its precipitous walls full 3000 feet high, rising here and there to yet greater elevations, and forming a central point in the mountain-system of the island. This the author regards as the basal wreck of a volcanic mountain, blown into the air by an explosion of exceptional violence. Fragments of the slopes of scoriæ which once composed the inner shell remain on the peaks surrounding this amphitheatre. The dykes here are trachyte. The author describes a limestone exposed in one place beneath the basalts and referred to the Upper Miocene, and a plant-bearing bed associated with them, containing fossils of species still living in the island, some of which have been wrongly referred to extinct forms. In conclusion the author remarked upon the almost infinite variability of the genus *Rubus* and the difficulty of distinguishing its species.

3. "On the Crag Shells of Aberdeenshire and the Gravel Beds containing them." By Thomas F. Jamieson, Esq., F.G.S.

4. "On the Red Clay of the Aberdeenshire Coast, and the Direction of Ice-movement in that quarter." By Thomas F. Jamieson, Esq., F.G.S.

The author describes a red clay occurring in the eastern part of Aberdeenshire, which differs in many important respects from the

ordinary grey Boulder-clay of the district. Its contents appear to show that it has not been derived from the Jurassic and granitic rocks of Aberdeenshire, but from masses of Old Red Sandstone, now perhaps removed by denudation. Only few and imperfect molluscan remains, with bones of fish, aquatic birds, and seals have been found in this red clay. He regards it as having been formed during a period of great submergence which followed the period of maximum glaciation. He finds at Cove proof of a remarkable change in direction of the flow of the glacial ice. Additional proofs of the submergence are found in the old beaches with shells, extending to heights of from 380 to 470 and even up to 560 feet above the sea-level. The Mollusca occurring in these raised beaches are all of living species, but now occur in arctic seas. He differs from Mr. J. Geikie, who regards these deposits as of post-Glacial age and of estuarine character. He concludes by describing in detail the remarkable section exhibited in the Bay of Nigg.

March 22.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On a Fossil Species of *Camptoceras*, a Freshwater Mollusk, from the Eocene of Sheerness." By Lieut.-Colonel H. H. Godwin-Austen, F.R.S., F.G.S.

2. "Note on the Os Pubis and Ischium of *Ornithopsis eucamerotus* (synonyms—*Eucamerotus*, Hulke; *Bothriospondylus* (in part), R. Owen; *Chondrosteatosaurus*, R. Owen)." By J. W. Hulke, Esq., F.R.S., Pres.G.S.

3. "On *Neusticosaurus pusillus* (Fraas), an Amphibious Reptile having affinities with the terrestrial Nothosauria and with the marine Plesiosauria." Py Prof. H. G. Seeley, F.R.S., F.G.S.

April 5.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Geological Age of the Taconic System." By Prof. J. D. Dana, F.M.G.S.

The author takes exception to some remarks made before the Geological Society by Dr. T. Sterry Hunt on the 16th November last. Dr. Sterry Hunt has thrown doubt on the results arrived at by the geologists who have studied the relations of the so-called Taconic strata, not in consequence of any observations of his own, but on the general ground that "where newer strata are in unconformable contact with older ones, the effect of lateral movements of compression, involving the two series, is generally to cause the newer and more yielding strata to dip towards, and even beneath the edge of the older rock—a result due to folds, often with inversion, sometimes passing into faults."

It was pointed out in opposition to these views, that the observations of Emmons, H. D. and W. B. Rogers, Mather, Sir W. Logan, James Hall, E. Hitchcock, C. H. Hitchcock, Hager, and Wing prove that the Taconic schists and limestones are in conformable succession and of Silurian age. The stratigraphical structure of the Taconic range is, indeed, so simple that all observers who have studied it have described the schists and limestones as conformable; and numerous characteristic Silurian fossils have been found in both. This view had been maintained by Dr. Sterry Hunt himself till 1878, when he first propounded his new interpretation of the strata in question; but the latter was not based on any fresh facts or observations.

The author's own observations on the subject, carried on during many years, were detailed and illustrated by a map of the whole of the Taconic range. In conclusion he pointed out that, even if Dr. Sterry Hunt's general principle were conceded (and he was not by any means himself prepared to make such a concession), it would have no bearing on the point at issue; for the supposed younger strata do not dip against the Taconic schists. In opposition to the view that the geological age of strata can be inferred from their mineral characters, he pointed out what remarkably different rocks have been produced by the metamorphism, in different degrees, of the strata of the Taconic range.

2. "On some Nodular Felsites in the Bala Group of North Wales." By Prof. T. G. Bonney, M.A., F.R.S., Sec.G.S.

These peculiar felsites are chiefly found in one or two localities in the neighbourhood of Bettws-y-coed and of Conway. In the former case, in the same mass are found ordinary compact felsite exhibiting under the microscope fluidal structure, a peculiar rock resembling a greenish schist, and various nodular felsites (with cleavage) showing nodules sometimes 3 or 4 inches in diameter, some being solid, others hollow or more or less filled with chalcedony, chlorite, chalybite, &c. The nodules never exhibit a radial structure; and there is no difference of importance between their structure and that of the ordinary rock. The author showed that the schisty rock is only an amygdaloidal felsite, in which, after cleavage, a secondary (micaceous?) mineral has been deposited along the cleavage-planes. As regards the nodular structure, he showed that it is analogous to the spheroidal structure seen on a larger scale in many igneous rocks, and on a smaller scale in perlite, and gave reasons for its being frequently associated with vesicular cavities. The infiltration of these cavities and consequent solidification was almost certainly prior to the production of the cleavage-surfaces, which are distorted by them. The Conway felsites exhibit similar phenomena, except that they are practically uncleaved.

3. "On the Cambrian (Sedgw.) and Silurian Rocks of Scandinavia." By J. E. Marr, Esq., B.A., F.G.S.

The author has examined the following areas of Cambrian and

Silurian rocks in Scandinavia :—(1) Dalecarlia, (2) Ostrogothia and Westrogothia, (3) Christiania, (4) Scania, (5) Baltic Isles. A sketch of the stratigraphy of each of these regions was given, and the author drew the following conclusions :—

Cambrian. Silurian.	Mudstones of Ramsåsa and Bjersjölagård . .	= Ludlow.
	Cardiola beds ; *Cyrtograptus* and *Retiolites* Shales	= Wenlock.
	Lobiferus Shales ; Upper part of Brachiopod beds	= May Hill.
	Lower part of Brachiopod beds	= Upper Bala.
	Trinucleus Shales ; *Beyrichii* Limestone . .	= Middle Bala.
	Kärgårde Shales ; Cystidean Limestone . .	= Lower Bala.
	&c. &c.	= &c.

A correlation with the beds of Bohemia was also given. The author pointed out that there is evidence of a physical break (varying in amount), as well as of a palæontological one, between the Cambrian and Silurian of Scandinavia. Several of the beds of Scandinavia admit of a very exact parallel with strata in the English Lake district.

The author considered that the fauna of these Scandinavian deposits affords evidence of migrations. This can be shown by observing that the same forms occur in two beds of different age, but are absent from an intermediate one ; or by tracing beds laterally, and showing that the forms occur in an earlier deposit in one locality than in another.

The author considered the black shales deep-water deposits, and accounted for their wide extent by supposing the material derived directly from the decomposition of the felspar in metamorphic rocks, and so in a very fine state of division. The deep-water fauna in the Cambrian appears to have migrated from the south-west ; the shallow-water forms, as might be expected, were more variable in their direction of migration : examples were given in support of this view. In Silurian times the direction of migration appears to have changed, the dispersal taking place from Britain, owing probably to greater local upheaval there. The coast-line also, instead of running in a W.N.W. and E.S.E. direction, seems to have run more W.S.W. to E.N.E., as shallow-water forms are common in Britain, but deep-water forms in the central Swedish area. The result of the author's investigations, as bearing on classification, is that there is a break in Scandinavia at the base of the equivalents of the May-Hill series, but no other break in the Cambrian series of Sedgwick of equal importance—no break, physical or palæontological, existing at the base of the *Ceratopyge* limestone (Tremadoc), where some authors have drawn a boundary.

XLVIII. *Intelligence and Miscellaneous Articles.*

ON THE DIMENSIONS OF A MAGNETIC POLE IN THE ELECTROSTATIC SYSTEM OF UNITS. BY PROF. J. D. EVERETT.

A PAPER* by Clausius, dealing with Electrical and Magnetic Units has just reached me; and I think I shall be doing good service in laying before the readers of the Philosophical Magazine the principal novelty which it contains.

Electricians are familiar with the fact that a circuit with a current flowing in it is a species of magnet, and that the moment of the magnet thus constituted is the product of the current by a certain area.

Hence the moment of a magnet must have the same dimensions as a current multiplied by an area. That is, we must have

$$\text{Pole} \times \text{Length} = \text{Current} \times (\text{Length})^2;$$

or, more simply,

$$\text{Pole} = \text{Current} \times \text{Length}.$$

This equation must be true in any consistent system of units.

In the electrostatic system, the dimensions of Current (or quantity of electricity divided by time) are $M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2}$ (M denoting Mass, L Length, and T Time). Hence, by the above equation, the dimensions of a Pole are $M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-2}$.

This is substantially Clausius's reasoning, though I have for brevity somewhat altered its form; and it appears to me unimpeachable. Nevertheless the result differs (as the author points out) from Maxwell's formula for the dimensions of a pole in the electrostatic system, namely $M^{\frac{1}{2}} L^{\frac{1}{2}}$. This formula will be found in Maxwell's 'Electricity and Magnetism,' § 626, where the name "quantity of magnetism" is used instead of "strength of pole," or the briefer name "pole." It will also be found in the Reprint of Reports of the B.A. Committee on Electrical Standards, page 90; but in neither work have I been able to find an explicit statement of the reasoning by which the result is obtained. Perhaps some reader of the Philosophical Magazine can give the necessary explanation.

In my own treatise on Units† the question at issue is not raised, as practically the dimensions of a magnetic pole do not enter into electrostatic discussions.

The dimensions of a pole, calculated in the ordinary way from the mutual repulsion of two poles, are $M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}$; and from this, by considering the force exerted by a current upon a pole, the dimensions of Current in the electromagnetic system are found to be $M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-1}$. The relation between these two, it will be observed,

* "Ueber die verschiedenen Maasssysteme zur Messung electrischer und magnetischer Grössen," von R. Clausius. Vorgetragen 6. März 1882. Separat-Abdruck aus den *Verhandlungen des naturhist. Vereins der preuss. Rheinlande*, Bd. xxxix. [A full translation of this paper will appear in an early Number of this Magazine.—Ed.]

† 'Units and Physical Constants' (Macmillan: 1879).

fulfils the foregoing condition,

$$\text{Pole} = \text{Current} \times \text{Length}.$$

It appears to me that Clausius's result must be accepted as the correct one.

Belfast, April 24, 1882.

ON THE ELECTROLYSIS OF DISTILLED WATER.

BY D. TOMMASI.

Several physicists have questioned the possibility of decomposing chemically pure water by means of a galvanic current. According to M. Bourgoin*, water is not an electrolyte, the substance mixed with it to render it conductive is alone decomposed.

The few experiments which I have made in regard to this subject have proved to me the contrary—that is to say, that water can be electrolyzed even by the current of a very feeble battery, provided that the calories liberated by the battery are at least equal to those absorbed by water in decomposing into its elements (about 69 calories). Those experiments are as follows :—

(1) Into a U-tube filled with distilled water I introduce two platinum electrodes connected with two Daniell elements. The distance between the electrodes is about 2 centim. No visible effect is produced, even after some time. The calories liberated by the battery are nevertheless more than sufficient to effect the decomposition of the water—in fact, $98 > 69$. If, then, the water in this case undergoes no decomposition, that is owing solely to the resistance opposed by it to the passage of the current, and not to insufficiency of energy produced by the battery.

(2) If in the preceding experiment the positive electrode be replaced by a silver wire, this is what is observed :—After 18 hours no appreciable alteration is noticed in the liquid ; but if, after taking out the silver wire, a drop of hydrochloric acid be poured into the branch in which it dipped, a very manifest white turbidity makes its appearance, having all the characters of chloride of silver. The hydrochloric acid does nothing more than precipitate the minute quantity of oxide of silver which was dissolved in the distilled water. Now silver does not decompose water at the ordinary temperature ; but, in order to produce oxidation, the water must have been decomposed ; and consequently the distilled water must have been passed through by the current. The electrodes in this experiment were distant from each other about 7 centim. It is evident, then, that, if the positive electrode is of silver, we can, with the aid of two small Daniell elements, overcome the resistance of a column of distilled water of 7 centim. length.

(3) With three Daniell elements the effect is much more marked. After 15 minutes it can already be ascertained, with the aid of hydrochloric acid, that the silver has begun to dissolve. At the end of 18 hours all the curved portion of the tube is found covered with a coat of oxide of silver, partly reduced by a secondary action.

* *Ann. de Chem. et de Phys.* [4] xv. p. 47, and xxviii. p. 119.

(4) If, in the experiment I have just spoken of, six Bunsen elements be substituted for the Daniell battery, the decomposition, as might have been expected, takes place with much more intensity. After 18 hours the quantity of oxide of silver, in part reduced, found at the bottom of the tube is relatively considerable.

(5) Gold employed as the positive electrode is not oxidized in the presence of distilled water, even by the action of the current of eight Bunsen elements.

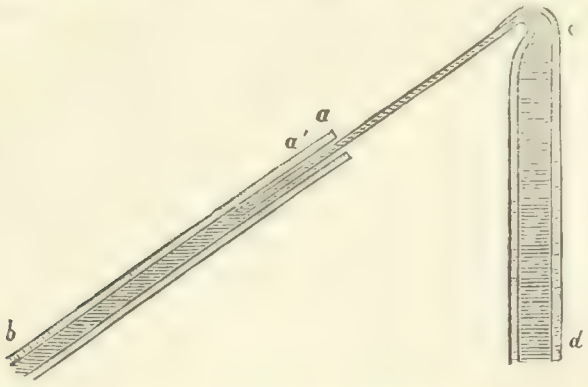
Is silver, then, the only metal which possesses the property of decomposing distilled water under the action of a galvanic current? No, fortunately; for it might be objected to me that, if water is decomposed when silver serves for the positive electrode, it may be due to this—that the oxide of silver, being soluble in water*, always suffices to render the water sufficiently conductive to be electrolyzed. This objection, however, would not be quite relevant; for it would not account for the current passing at the commencement of the experiment, since at that moment the water contains no foreign substance, and it is only after some time that the silver begins to dissolve. Therefore it is not the oxidation of the silver that permits the current to pass; but it is the current that, by decomposing the water, oxidizes the silver. It is true that, once the water contains a little oxide of silver, its conductivity is increased, and consequently electrolysis can take place with more facility; but, I repeat, the oxidation of the silver is not the determining cause of the decomposition of the water, but only the effect of that decomposition. Be that as it may, here is an experiment which will show positively that the solubility of the silver oxide plays only a secondary part in the electrolysis of distilled water:—

(6) The oxides and the hydrates of copper are completely insoluble in distilled water; and hence they cannot in any way augment the conductivity of the water. Now I have found that copper possesses, like silver, the property of decomposing distilled water when it is connected with the positive pole of a battery. The experiment is made as before; that is to say, a platinum wire and a copper wire are immersed in the two branches of a U-tube filled with distilled water, the former joined to the negative, and the latter to the positive pole of a battery composed of three Daniell elements. The distance separating the two electrodes is about 4 centimetres. At the end of 18 hours, upon a length of about 2 centim. at the lower part a coat of reduced copper is found adhering to the sides of the tube. A portion of the copper is deposited on the platinum wire. The decomposition of the water in this case can be explained only on the hypothesis that the copper employed as the positive electrode tends to diminish the resistance of the water, and consequently to render it more apt to be electrolyzed. It is the same with silver employed as positive electrode.—*Comptes Rendus de l'Académie des Sciences*, April 5, 1882, pp. 948-951.

* Oxide of silver dissolves in 3000 times its weight of water.

A LITTLE ILLUSION. BY J. PLATEAU.

Permit me to describe an experiment which, at the first glance, seems as if it must realize perpetual motion. Into a capsule of sufficient dimensions let us pour distilled water till its depth is about 1 centimetre. In this let us immerse one end of a glass tube of small internal diameter, 1·5 millim. for instance, and from 8 to 9 centim. in length; let us keep this tube in a suitably inclined position, so that the water which will be caused to rise in it by capillary action shall occupy a sufficiently large portion of its length. Let us procure a second tube, having, say, an internal diameter of 3 millim., and a length of some centimetres. Let us draw out one end of this second tube, and bend back the drawn-out thin portion so that it makes a more or less acute angle with the wide part. Let us introduce the thin portion into the upper orifice of the inclined tube till it reaches the water contained in this, and give to the wide portion a vertically descending position; this last condition will be obtained by modifying either the above-mentioned angle or the inclination of the other tube. Let us suppose that the orifice of the vertical portion does not reach the liquid of the capsule; lastly, let us imagine the thin portion and the vertical tube both full of water. For greater clearness, I represent in section, in the annexed figure, the upper part of the system.



a b is the inclined tube, and *c d* the vertical. In consequence of the narrowness of the space comprised between the slender portion and the inner surface of the tube *a b*, capillarity will maintain the water near the orifice *a* of the latter; suppose that it stops at *a'*. The lower extremity of the tube *c d* not reaching the liquid of the capsule, that tube with its slender portion will constitute a siphon of which the short branch dips in a liquid in equilibrium, while the long branch descends several centimetres lower than the surface of that liquid. Does it not seem, then, that the water must incessantly run through this siphon, to rejoin that in the capsule? Now, if that took place, it would evidently be perpetual motion.

But, the impossibility of perpetual motion being demonstrated, I asked myself what would in reality take place under the above conditions. I therefore requested M. Van der Mensbrugghe to

perform the experiment, which he did according to my directions, employing tubes having the above-assumed dimensions. I pass over in silence the construction of the supports of the tubes, as well as the little manipulations of the experiment: the reader who has the curiosity to repeat it will find out those details for himself. Now we verified that, instead of the water running away through the tube $c\,d$, it reascends in this, and continues to be aspirated until its free surface reaches a certain point in the slender portion, after which every thing stops. Only, in order that the aspiration may take place, the orifice of the tube $c\,d$ must be several millimetres above the liquid of the capsule; if it is lower, the tube $c\,d$ continues full, the water remains suspended in it.

A little reflection soon enabled me to understand the cause of these apparently singular phenomena. The small surface which terminates at a' the liquid between the two tubes is concave in the transverse direction; it therefore, in virtue of its curvature, exerts suction upon the whole mass of liquid to which it belongs—that is to say, upon the liquid of the tube $a\,b$ and that of the tube $c\,d$, the latter liquid communicating with the former by the slender tube. In the tube $a\,b$ this suction has the effect of keeping the water raised up to a'; and it is balanced by the action of gravity, which action is measured by the difference of level between the surface at a' and the water of the capsule. In order that it may be in like manner balanced in the tube $c\,d$, it seems at first necessary that the difference of level between the surface at a' and the orifice of the tube $c\,d$ be equal to the preceding, which would require that orifice to reach the water of the capsule; and yet we have seen that equilibrium subsists even when the orifice in question is several millimetres higher. But it is because, in consequence of a tendency to absorption, the water presents at this orifice a slightly concave surface, which exerts a slight suction downwards; and this, added to the incomplete action of gravity, compensates the suction emanating from a'. For a greater distance of the orifice from the water of the capsule, the suction at a' is in excess, the liquid is drawn upward in the tube $c\,d$; and this movement stops when the free surface of the liquid has arrived, in the slender portion, at a point where the suction due to its strong concave curvature makes equilibrium with that of a'*. Such is the simple explanation of the observed effects; and this little experiment offers a curious example of the suction exerted by concave liquid surfaces; it shows us, besides, an equally curious exception to the usual action of siphons. —*Extrait des Bulletins de l'Acad. roy. de Belgique*, Jan. 1882.

* In our experiment the vertical distance from a' to the liquid of the capsule was nearly 4 centim., and the least distance from the orifice of the tube $c\,d$ to the same liquid, for which absorption was produced, was about 5 millim.

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[FIFTH SERIES.]

JUNE 1882.

XLIX. *On the different Systems of Measures for Electric and Magnetic Quantities.* *By* Prof. R. CLAUSIUS*.

FOR the measurement of electric and magnetic quantities, it is well known that two essentially different systems, usually called the *electrostatic* and the *electromagnetic*, are employed. Both reduce the determination of electric and magnetic quantities to the measurement of *mass*, *length*, and *time*; but in the manner of the reduction they differ from each other more considerably than is usually the case with other different systems of measures. While in the ordinary mechanical quantities (such as velocities, forces, and mechanical work) the different measuring-systems employed are distinguished from one another only by the so-called fundamental units, viz. the units of mass, length, and time, having different values, in the two above-mentioned systems applied to electricity and magnetism the *formulæ* which serve for the determination of one and the same quantity are also different, since they contain different powers of the fundamental units.

The deduction of these formulæ is systematically carried out with singular completeness in Clerk Maxwell's splendid work, 'A Treatise on Electricity and Magnetism,' Oxford, 1873; and the high estimation in which Maxwell is justly held could not fail to cause his formulæ not only to be accepted in

* Translated from a separate impression, communicated by the Author, from the *Verhandl. des naturhist. Vereins der preuss. Rheinlande und Westfalens*, vol. xxxix. (1882). Read at the meeting of the Niederrheinische Gesellschaft für Natur- und Heilkunde, on the 6th March, 1882.

England as correct throughout, but also to be adopted unaltered in the works of writers belonging to other nations. In regard to the latter, I will mention only Mascart and Joubert's valuable *Leçons sur l'Electricité et le Magnétisme* (Paris, 1882), and Herwig's useful book, *Physikalische Begriffe und absolute Maasse* (Leipzig, 1880).

Nevertheless I think I can show that Maxwell has committed an error in his development, by which several of his formulæ have been rendered incorrect. The correction of this mistake appears to me to be especially necessary just now, since at the present time the attention of wider circles than formerly is directed to this subject by the decisions of the Congress of Electricians at Paris upon the electrical units of measurement, and hence an error remaining uncorrected might be mischievously disseminated and fixed. It also appears to me that Maxwell's exposition of the subject (which is not all collected together in one passage of his book, but appears in portions in different parts of it) presents some difficulty to the understanding. Hence I think that a connected presentation of the thing, as simple and distinct as possible, will not be unwelcome.

§ 1. *The Fundamental Units and the first General Equation of each System.*

As already mentioned above, the units of mass, length, and time are employed as fundamental units. After Maxwell, it is usual to denote them by enclosing in square brackets the letters which are generally employed to represent those three kinds of quantity. Thus the unit of mass is [M], the unit of length [L], and the unit of time [T].

From these are immediately obtained the units of various other quantities. That velocity with which a unit of length is passed through in the unit of time is employed as the unit of *velocity*; hence, for its representation, we have to divide the unit of length by the unit of time, and we get the formula $[LT^{-1}]$. The unit of *force* is that force which imparts the unit of velocity to the unit of mass in the unit of time; hence we obtain the formula which serves to represent it, if we divide the product of the mass-unit and the velocity-unit by the time-unit, whence arises $[MLT^{-2}]$. The unit of *mechanical work* is that work which is done by the force-unit along a path equal to the length-unit; and therefore, in order to represent it, we need only to multiply the force-unit by the length-unit, by which we get $[ML^2T^{-2}]$.

Now, for the measurement of electricity we must employ

the forces exerted by it. These are of two essentially different kinds:—first, the forces independent of its motion, which amounts of electricity always exert upon one another whether they are at rest or in motion; secondly, the forces arising only through the motion. The former are named the *electrostatic*, and the latter the *electrodynamic* forces. To the electrodynamic we must reckon the magnetic forces, if we with Ampère derive the explanation of magnetism from small electric currents taking place in the interior of the magnet. Now, of these two forces, we can apply either the one or the other to the measurement of electricity; and thence arise those two systems of measures, of which the former is called the *electrostatic*, while the latter (as already said) is ordinarily named the *electromagnetic*, but would be more rationally called the *electrodynamic*. In those cases in which it is self-evident that *electrical* systems of measures are meant, one can, for convenience, omit the prefix "electro," and, briefly, speak of *static* and *dynamic* measuring-systems.

In the *electrostatic* system the most important unit, which serves as the basis of all the others, is the *unit of electricity*. This is determined by the following definition:—*The unit of electricity is that amount of electricity which exerts the unit of force upon an equal amount of electricity at the unit of distance.* We can therefore put the unit of force equal to a fraction having the square of the electricity-unit for its numerator and the square of the length-unit for its denominator. We will at the same time denote, with Maxwell, the electricity-unit by enclosing the letter e in square brackets; but, in order to express that the *static* electricity-unit is intended, we will provide the e with the index s, so that the symbol will have the form $[e_s]$. The equation in question will then read:—

$$\frac{[e_s^2]}{[\mathrm{L}^2]} = [\mathrm{MLT}^{-2}];$$

and from this follows

$$[e_s] = [\mathrm{M}^{\frac{1}{2}}\mathrm{L}^{\frac{3}{2}}\mathrm{T}^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In the *electrodynamic* system of measures, it would first suggest itself, from what has been said above, to take as the standard that force which, in addition to the static force, two moved quantities of electricity exert upon each other in consequence of their motion. Respecting this force, however, differences of opinion prevail, which make the choice of it as the starting-point of the determinations inexpedient. On the other hand, the forces exerted upon each other by *closed* electric currents can be regarded as indubitably known. As,

further, the little electric currents which, according to Ampère, must be assumed to flow in the interior of a magnet are likewise closed, we have in magnetism to do with forces of the same kind, and hence we can also select as the standard the force exerted upon one another by two quantities of magnetism. This latter is the most convenient, because magnetic forces can be more simply expressed than the forces exerted between larger closed currents. Accordingly, in the electrodynamic system the unit of magnetism is determined, in precise correspondence with the unit of electricity in the electrostatic system, by the following definition:—*The unit of magnetism is that amount of magnetism which exerts upon an equal amount of magnetism at the unit of distance the unit of force.*

For the mathematical representation, we, again with Maxwell, denote the unit of magnetism by a square-bracketed m; but, in order to indicate that the unit in question is the *dynamic* unit, we annex a d as index, so that the symbol has the form $[m_d]$; and with this we form the equation

$$\frac{[m_d^2]}{[\mathrm{L}^2]} = [\mathrm{MLT}^{-2}],$$

whence results

$$[m_d] = [\mathrm{M}^{\frac{1}{2}}\mathrm{L}^{\frac{3}{2}}\mathrm{T}^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

§ 2. *The Relation between Electricity and Magnetism.*

By equations (1) and (2), for the static system the unit of electricity, and for the dynamic system the unit of magnetism, are determined. There is now the further question, How is the unit of magnetism to be determined for the static system, and the unit of electricity for the dynamic? For this the well-known proposition of Ampère respecting the substitution for a closed galvanic current of two magnetic surfaces, which has also been adopted by Maxwell in its utmost generality and irrespective of any particular system of measurement*, is available.

For the sake of simplicity, the current-curve may be supposed plane, and the area of the plane figure inclosed by it assumed as the surface-unit. Besides the plane containing this figure, let us now imagine a parallel plane placed at an infinitesimal distance, and upon this a figure delimited congruent with the first figure and lying perpendicularly opposite to it. Now let these two plane figures be uniformly covered with equal quantities of north and south magnetism—that

* See Maxwell's 'Treatise on Electricity and Magnetism,' vol. ii. part 3, chapter iii.

which, if we imagine ourselves going with the current round the figures, we have on the left hand with north, and the other with south magnetism. The quantity of the magnetisms is determined by the intensity of the current and the mutual distance of the planes. Let the latter be denoted by $\epsilon[\mathrm{L}]$, in which [L], as always, signifies unit length, and ϵ an infinitesimal numerical value. Then, if a current-unit be assumed as the current-intensity, each of the two quantities of magnetism, apart from the sign, is to be supposed equal to a unit of magnetism divided by ϵ. The pair of magnetic surfaces thus formed can replace the current in regard to all the forces exerted by it.

To express this mathematically, we have to multiply the intensity of the current by the area round which it flows, to multiply the quantity of magnetism present on one of the surfaces by the distance between the surfaces, and then to equate the two products. Now the current-intensity is a current-unit, which is a unit of electricity flowing through the cross section in unit time, and which is therefore represented by $[e\mathrm{T}^{-1}]$; and the area round which it flows is a unit of surface, therefore $[\mathrm{L}^2]$. Accordingly the first product is $[e\mathrm{L}^2\mathrm{T}^{-1}]$. Further, the quantity of magnetism coming into consideration is $\frac{[m]}{\epsilon}$, and the distance between the surfaces $\epsilon[\mathrm{L}]$; so that the second product reads $\frac{[m]}{\epsilon}\epsilon[\mathrm{L}]$ or $[m\mathrm{L}]$. We have consequently to form the following equation:—

$$[m\mathrm{L}]=[e\mathrm{L}^2\mathrm{T}^{-1}],$$

from which results

$$\frac{[m]}{[e]}=[\mathrm{L}\mathrm{T}^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This equation, which is only an expression of the relation established by Ampère between magnetism and electric currents, must hold good for every system of measurement; and hence we can form from it two special equations referable to the static and the dynamic systems respectively, namely:—

$$\frac{[m_s]}{[e_s]}=[\mathrm{L}\mathrm{T}^{-1}], \quad . \quad . \quad . \quad . \quad . \quad . \quad (3\mathrm{A})$$

$$\frac{[m_d]}{[e_d]}=[\mathrm{L}\mathrm{T}^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3\mathrm{B})$$

If we bring these two equations into connexion with the equations (1) and (2), for $[e_s]$ and $[m_d]$, we thereby arrive

at the expressions for $[m_s]$ and $[e_d]$. From (1) and (3 A) we get by multiplication, on which $[e_s]$ vanishes,

$$[m_s]=[M^{\frac{1}{2}}L^{\frac{5}{2}}T^{-2}]; \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and from (2) and (3B) is obtained by division, $[m_d]$ vanishing,

$$[e_d]=[M^{\frac{1}{2}}L^{\frac{1}{2}}]. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

§ 3. Maxwell's *Incorrect Equations.*

Instead of the relation between electricity and magnetism deduced in the foregoing section from Ampère's proposition, and expressed in equation (3), in the equations constructed by Maxwell another relation between electricity and magnetism is implicitly expressed; for at page 240 of the second volume of his work, under (1) and (3) are found the equations

$$[pC]=\frac{[L^2M]}{[T^2]} \text{ and } \frac{[e]}{[C]}=[T].$$

Here $[e]$ signifies, as with us, the unit of electricity, and $[C]$ represents the unit of current. By $[p]$ is understood the unit of a quantity which he calls the electrokinetic moment of a current, into the meaning of which, however, we need not here enter, since he himself, further on, puts $[p]=[m]$, and therefore we can in the first of the above equations substitute $[m]$ for $[p]$. If we then multiply the two equations together, we obtain the equation

$$[em]=[ML^2T^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This, according to Maxwell, holds good for every system of measurement; hence in his developments it plays the same part as equation (3) does in ours.

The way in which he arrives at his equation rests upon his bringing into calculation the force exerted by a current upon a magnetic pole, in a similar manner to that in which we, in deducing equations (1) and (2), have brought in the force acting between two units of electricity, and between two units of magnetism, respectively. The force, however, which a current exerts upon a magnetic pole is *electrodynamic*; and from this it follows that an equation of which the deduction is based upon this force can be regarded as valid only in the dynamic system founded upon the electrodynamic forces, and not in the static system based on the electrostatic forces.

Indeed it is also manifest that Maxwell's equation (16) leads to the same result in the dynamic system of measurement as

our equation (3), while in the static system it gives a different result. If, namely, we put, first,

$$[e_d m_d] = [\mathrm{ML^2T^{-1}}];$$

and divide this equation by (2), there comes

$$[e_d] = [\mathrm{M^{\frac{1}{2}} L^{\frac{1}{2}}}],$$

which equation agrees with equation (5) above given. But if we put

$$[e_s m_s] = [\mathrm{ML^2T^{-1}}],$$

and divide this by (1), we get

$$[m_s] = [\mathrm{M\ L}\ ], \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

which is different from our equation (4), at which we arrived by employing equation (3).

By Maxwell's incorrect formula for the static magnetism-unit the formulæ of other units in the static system of measures, dependent on the unit of magnetism, have been rendered also incorrect, in a not inexplicable manner.

§ 4. *The Determination of other Units.*

The units of electricity and magnetism in both systems of measures having been determined, the units for the other quantities also which occur in electric and magnetic investigations can now be easily deduced. Of these the most important only may here be mentioned.

The unit of the *intensity of a current* results very simply from understanding by current-intensity the quantity of electricity flowing in unit time through a cross section. Hence we only need here to divide the unit of electricity by the unit of time; and we thereby obtain, if i denotes intensity, the following equations relating to the two systems :—

$$[i_s] = \frac{[e_s]}{[\mathrm{T}]} = [\mathrm{M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2}}], \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$[i_d] = \frac{[e_d]}{[\mathrm{T}]} = [\mathrm{M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-1}}]. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The unit of *electromotive force* is most simply determined from the condition (which holds universally) that the product from the units of electromotive force, current-intensity, and time, or (what is the same) the product from the units of electromotive force and quantity of electricity, must be equal to the unit of mechanical work. From this, namely, we obtain,

if E denotes electromotive force, the equation

$$[Ee]=[ML^2T^{-2}]; \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and if this equation be brought into the form

$$[E]=\frac{[ML^2T^{-2}]}{[e]},$$

and then applied successively to both systems, the following equations are arrived at:—

$$[E_s]=\frac{[ML^2T^{-2}]}{[e_s]}=[M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}], \quad . \quad . \quad (11)$$

$$[E_d]=\frac{[ML^2T^{-2}]}{[e_d]}=[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-2}]. \quad . \quad . \quad (12)$$

The unit of *resistance* of conductors is defined as the resistance of a conductor in which the unit of electromotive force generates a current of unit intensity. We have, therefore, in order to deduce the formula for it, only to divide the unit of electromotive force by the unit of current-intensity; and then we get, if R denotes conductors' resistance:—

$$[R_s]=\frac{[E_s]}{[i_s]}=[L^{-1}T], \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$[R_d]=\frac{[E_d]}{[i_d]}=[LT^{-1}]. \quad . \quad . \quad . \quad . \quad (14)$$

Finally, the *capacity* of a conducting body may also be considered, by which is to be understood the quantity of electricity which the body can receive through the action of a unit of electromotive force. Since according to this definition the capacity of a body to which the unit of electromotive force can convey a unit of electricity is to be considered the unit of capacity, in order to construct the formula for the capacity-unit we have to divide the electricity-unit by the unit of electromotive force; and from this we obtain, if C denotes capacity:—

$$[C_s]=\frac{[e_s]}{[E_s]}=[L], \quad . \quad . \quad . \quad . \quad (15)$$

$$[C_d]=\frac{[e_d]}{[E_d]}=[L^{-1}T^2]. \quad . \quad . \quad . \quad (16)$$

For the sake of clearness, the units which have been successively determined in the foregoing may here be tabulated:—

Static Measure.	Dynamic Measure.
$[e_s]=[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1}]$,	$[e_d]=[M^{\frac{1}{2}}L^{\frac{1}{2}}]$,
$[m_s]=[M^{\frac{1}{2}}L^{\frac{5}{2}}T^{-2}]$,	$[m_d]=[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1}]$,
$[i_s]=[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-2}]$,	$[i_d]=[M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}]$,
$[E_s]=[M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}]$,	$[E_d]=[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-2}]$,
$[R_s]=[L^{-1}T]$,	$[R_d]=[LT^{-1}]$,
$[C_s]=[L]$.	$[C_d]=[L^{-1}T^2]$.

§ 5. *Comparison of the Units of the two Systems.*

In the foregoing, in determining the static units the measurement based on the electrostatic force, and in determining the dynamic units the measurement based on the electrodynamic force, were employed. The formulæ thus formed can therefore only serve to express the relation in which the units of each system stand to one another, but not to compare, as to their quantity, a unit of one of the systems with the corresponding unit of the other. For this latter purpose the ratio between the electrodynamic and the electrostatic force must also be taken into account.

In the static system the force between two quantities of electricity is expressed simply by the product of the quantities of electricity divided by the square of the distance; while the force between two quantities of magnetism has for its expression in the static system the product of the quantities of magnetism divided by the square of the distance and then multiplied by a constant factor k, which determines the ratio between the electrodynamic and the electrostatic force. In the case of the quantities of electricity and magnetism considered being assumed as units, the expressions of the two forces are $[e_s^2L^{-2}]$ and $k[m_s^2L^{-2}]$.

Now, in order, first, to learn more precisely the nature of the factor k, we will in the last expression, in accordance with (3A), put for $[m_s]$ the product $[LT^{-1}].[e_s]$, by which it is changed into $k[L^2T^{-2}].[e_s^2L^{-2}]$. Now, as in this expression the last factor $[e_s^2L^{-2}]$ represents a force (viz. the unit of force), and the whole expression is also to represent a force, the product $k[L^2T^{-2}]$ must be a pure numerical value, whence it follows that k must be the reciprocal value of the square of a velocity. Consequently, if we choose the symbol K for the latter, we can write $k=\frac{1}{K^2}$, by which, if at the same time we put the formula $[MLT^{-2}]$, representing the force-unit, for

$[e_s^2 L^{-2}]$, we get the following expression for the force exerted by two static units of magnetism upon one another at unit distance:—

$$\frac{[L^2T^{-2}]}{K^2}[MLT^{-2}].$$

The velocity denoted by K, which, so far as the measurements hitherto effected permit a judgment to be formed, agrees with the velocity of light *in vacuo*, is a quantity so important for electricity that it seems convenient to introduce a special name for it; and I propose to call it, analogously to a name introduced by Andrews into the theory of heat, the *critical* velocity.

Returning now to the consideration of the magnetic forces, according to the above the force exerted between two static units of magnetism at the unit of distance is equal to the fraction $\frac{[L^2T^{-2}]}{K^2}$ of a unit of force. The force between two dynamic units of magnetism at unit distance is, on the contrary, according to § 1, equal to a unit of force. Consequently the latter force is to the former as 1 to $\frac{[L^2T^{-2}]}{K_2}$, or as K^2 to $[L^2T^{-2}]$. Since, then, the forces must at equal distances be as the products of the quantities of magnetism acting upon each other, and consequently in the present case as the square of the dynamic unit of magnetism to the square of the static unit of magnetism, these two squares must also be as K^2 to $[L^2T^{-2}]$, and the two units of magnetism themselves must therefore be as K to $[LT^{-1}]$.

In representing this result mathematically, we must not denote the units of magnetism simply by $[m_s]$ and $[m_d]$; for these symbols represent the quantities of magnetism under consideration (taken as units) on the hypothesis that the one is measured with the aid of the electrostatic and the other with the aid of the electrodynamic force. For a comparison of the magnitudes of the two units, however, it is necessary that both be measured by the same measure—and consequently that either the dynamic unit be reduced to static measure or the static unit to dynamic measure. For these reduced values we will introduce a special symbol. To indicate that a quantity whose former symbol supposes dynamic measurement is measured according to static measure we will prefix to that symbol v. s., the initials of *valor staticus*; and to indicate that a quantity whose former symbol supposes static measurement is measured according to dynamic measure, we will prefix to that

symbol v. d., the initials of *valor dynamicus*. Thus, v. s. $[m_d]$ denotes the value measured according to static measure of the dynamic unit of magnetism, and v. d. $[m_s]$ the value measured according to dynamic measure of the static unit of magnetism.

With the aid of these symbols we can now express the above result as follows:—

$$\frac{\text{v. s. } [m_d]}{[m_s]} = \frac{[m_d]}{\text{v. d. } [m_s]} = \frac{\text{K}}{[\text{LT}^{-1}]}. \quad . \quad . \quad (17)$$

In accordance with this, and employing the before-given formulæ for $[m_s]$ and $[m_d]$, the following two pairs of equations for the two systems of measurement can be formed:—

$$[m_s] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{5}{2}}\text{T}^{-2}]; \quad \text{v. s. } [m_d] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{3}{2}}\text{T}^{-1}]\text{K}; \quad . \quad (18)$$

$$\text{v. d. } [m_s] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{5}{2}}\text{T}^{-2}]\text{K}^{-1}; \quad [m_d] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{3}{2}}\text{T}^{-1}]. \quad . \quad . \quad (19)$$

From these equations corresponding equations for the units of the other quantities can also be derived.

As to the quantity e, we can put, in accordance with (3A) and (3B),

$$[e_s] = [m_s][\text{L}^{-1}\text{T}], \qquad [e_d] = [m_d][\text{L}^{-1}\text{T}],$$

and hence also

$$\text{v. d. } [e_s] = \text{v. d. } [m_s][\text{L}^{-1}\text{T}], \quad \text{v. s. } [e_d] = \text{v. s. } [m_d][\text{L}^{-1}\text{T}].$$

By employing these values we obtain from the preceding equations:—

$$\frac{\text{v. s. } [e_d]}{[e_s]} = \frac{[e_d]}{\text{v. d. } [e_s]} = \frac{\text{K}}{[\text{LT}^{-1}]}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

$$[e_s] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{3}{2}}\text{T}^{-1}]; \quad \text{v. s. } [e_d] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{1}{2}}]\text{K}, \quad . \quad (21)$$

$$\text{v. d. } [e_s] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{3}{2}}\text{T}^{-1}]\text{K}^{-1}; \quad [e_d] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{1}{2}}]. \quad . \quad (22)$$

From these equations we get for i, conformably with (8) and (9):—

$$\frac{\text{v. s. } [i_d]}{[i_s]} = \frac{[i_d]}{\text{v. d. } [i_s]} = \frac{\text{K}}{[\text{LT}^{-1}]}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$[i_s] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{3}{2}}\text{T}^{-2}]; \quad \text{v. s. } [i_d] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{1}{2}}\text{T}^{-1}]\text{K}, \quad . \quad (24)$$

$$\text{v. d. } [i_s] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{3}{2}}\text{T}^{-2}]\text{K}^{-1}; \quad [i_d] = [\text{M}^{\frac{1}{2}}\text{L}^{\frac{1}{2}}\text{T}^{-1}]. \quad . \quad (25)$$

For E we get, from the equations for e in conjunction with equations (11) and (12):—

$$\frac{\text{v. s. } [\text{E}_d]}{[\text{E}_s]} = \frac{[\text{E}_d]}{\text{v. d. } [\text{E}_s]} = \frac{[\text{LT}^{-1}]}{\text{K}}, \quad . \quad . \quad . \quad (26)$$

$$[E_s]=[M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}];\quad \text{v.s.}\,[E_d]=[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-2}]K^{-1}, \quad (27)$$

$$\text{v.d.}\,[E_s]=[M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}]K;\quad [E_d]=[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-2}]. \quad (28)$$

For R there result from the equations for E and i in conjunction with equations (13) and (14):—

$$\frac{\text{v.s.}\,[R_d]}{[R^s]}=\frac{[R_d]}{\text{v.d.}\,[R_s]}=\frac{[L^2T^{-2}]}{K^2}, \quad (29)$$

$$[R_s]=[L^{-1}T];\quad \text{v.s.}\,[R_d]=[LT^{-1}]K^{-2}, \quad (30)$$

$$\text{v.d.}\,[R_s]=[L^{-1}T]K^2;\quad [R_d]=[LT^{-1}]. \quad (31)$$

Lastly, for C we get from the equations for e and E in conjunction with equations (15) and (16):—

$$\frac{\text{v.s.}\,[C_d]}{[C_s]}=\frac{[C_d]}{\text{v.d.}\,[C_d]}=\frac{K^2}{[L^2T^{-2}]}, \quad (32)$$

$$[C_s]=[L];\quad \text{v.s.}\,[C_d]=[L^{-1}T^2]K^2, \quad (33)$$

$$\text{v.d.}\,[C_s]=[L]K^{-2};\quad [C_d]=[L^{-1}T^2]. \quad (34)$$

§ 6. *Maxwell's Statements respecting the Ratios between the Static and Dynamic Units.*

On the ratios between the static and dynamic units, discussed in the foregoing section, Maxwell expresses himself differently. He gives, in his work on Electricity (vol. ii. p. 243), the following table, in which I have merely taken leave to alter the letters employed for the notation so as to make them agree with our notation:—

Number of Electrostatic Units in one Electromagnetic Unit.

For e and i	K,
„ m and E,	$\frac{1}{K}$,
„ C	K^2,
„ R	$\frac{1}{K^2}$.

Among these statements, that which refers to the magnetism m directly contradicts our equation (17), since with Maxwell K stands in the denominator, while in equation (17) K stands in the numerator. The mistake made by Maxwell in this statement is a consequence of the already mentioned oversight made by him in the construction of the expression for the electrostatic unit of magnetism.

With respect to the rest of the statements, I think I must make one remark upon their form. I cannot admit it to be mathematically correct to say that the number of the electrostatic units in an electromagnetic (or electrodynamic) unit is equal to a *velocity* or to any power of a *velocity*. It is true that Maxwell himself has, in another passage, somewhat modified his manner of expression by adding the word "numerically" to the word "equal;" but other authors, who have followed him in treating of the units, have paid no particular regard to this addition, but have simply retained the above form, to which they have thereby given wide diffusion.

A manner of expression of this sort permits it to be inferred that formulæ of different dimensions in respect of their quantity are compared with one another, which is inadmissible. If one wishes to compare a static unit with the corresponding dynamic unit with respect to quantity, one must, as we have above done, express both in one and the same system of measures, and consequently either reduce the dynamic unit to static measure or the static unit to dynamic measure. Thereby are obtained formulæ of equal dimensions; and when these are compared with one another, it is found, not that the number of static units is equal to a *velocity* or to a power of a *velocity*, but that it is equal to the *ratio of two velocities*, or equal to a power of that *ratio*.

I believe that in investigations like that with which we are here occupied, in which it is precisely the determination of the dimensions of the different kinds of quantities that is chiefly the question, it is essentially important that even the manner of expression in regard to the dimensions be absolutely correct.

§ 7. *The Practical System of Measures.*

In all that has been said hitherto the question has been only, how can the units relating to electricity and magnetism be represented by the fundamental units, namely the units of mass, length, and time? The quantity which has been given to the fundamental units must now be discussed.

Gauss and Weber, who introduced the electrodynamic system of measures, selected as the units of mass, length, and time the milligram, millimetre, and second; while the British Association (who in the rest have adopted the system of Gauss and Weber) have, at the suggestion of Sir William Thomson, chosen the gram, centimetre, and second as units of mass, length, and time.

Both systems of fundamental units, however, furnish electrical units the magnitude of which is very different from the

magnitudes to be practically measured ; so that the latter can only with the aid of very large or very small numerical values be represented by those units. To avoid this inconvenience, the British Association, and in connexion with it the Congress of Electricians at Paris, have decided to multiply or divide the electrical units resulting from Thomson's system of fundamental units by certain (some of them very high) powers of 10, and thus to form units of practically suitable magnitude.

For these latter, names borrowed from celebrated men specially meritorious with respect to this branch of physics were then chosen. Among the units we have above discussed, only one is left without a name, and, indeed, just that which forms the base of the dynamic system, namely the unit of magnetism. Hence I would take leave to propose that for it the name "weber" be introduced ; for to Weber we are indebted for singularly great advances in regard to electrical measurements, and he, in conjunction with Gauss, was the founder of the electrodynamic system. Hence also, formerly, one of the units, the unit of current-intensity, was designated by his name. In the practical system now introduced, however, it turned out that the unit of current-intensity which fits into this system differs from that designated by the name of Weber in the proportion of 1 to 10; and as it was feared that confusion might be produced by employing the same name for the new unit, another name, ampère, was given to it. Accordingly, if the name of Weber were not introduced for the unit of another kind of quantity, it would be missing in the system of names, which would not be in keeping with justice. I therefore think I may reckon upon my proposal meeting with general assent.

The practical system of measures established by the Congress of Electricians, with the inclusion of the above-discussed unit of magnetism, if the gram and the centimetre be, as is usual, denoted by *gr* and *cm* and the second by *s*, can be written as follows:—

Weber ..... $[m_d] = gr^{\frac{1}{2}} cm^{\frac{3}{2}} s^{-1} . 10^{8}$,

Coulomb ... $[e_d] = gr^{\frac{1}{2}} cm^{\frac{1}{2}} . 10^{-1}$,

Ampère..... $[i_d] = gr^{\frac{1}{2}} cm^{\frac{1}{2}} s^{-1} . 10^{-1}$,

Volt ..... $[\mathrm{E}_d] = gr^{\frac{1}{2}} cm^{\frac{3}{2}} s^{-2} . 10^{8}$,

Ohm ..... $[\mathrm{R}_d] = cm\, s^{-1} . 10^{9}$,

Farad ..... $[\mathrm{C}_d] = cm^{-1} s^{2} . 10^{-9}$.

§ 8. *The Practical System of Measures as a Simple System.*

In the foregoing form the practical system has the inconvenience that with every unit one must bear in mind the power of 10 by which the general formula given in § 4 is to be multiplied. Nevertheless, as the Committee of the British Association has already remarked, by a suitable selection of the fundamental units the practical can be made a simple system, in which all the units can be represented merely by the formulæ given in § 4. For this purpose $1 . 10^{-11} gr$ must be taken as the unit of mass, and $1 . 10^{9} cm$ or $1 . 10^{7} m$ as the unit of length, while the time-unit remains 1 second.

If the length $1 . 10^{7} m$ be denoted by q to indicate that it is equal to a quadrant of the meridian, and the mass $1 . 10^{-11} gr$ by p, the practical units can be written as follows:—

Weber	$[m_d] = p^{\frac{1}{2}} q^{\frac{3}{2}} s^{-1}$,
Coulomb	$[e_d] = p^{\frac{1}{2}} q^{\frac{1}{2}}$,
Ampère.....	$[i_d] = p^{\frac{1}{2}} q^{\frac{1}{2}} s^{-1}$,
Volt	$[\mathrm{E}_d] = p^{\frac{1}{2}} q^{\frac{3}{2}} s^{-2}$,
Ohm	$[\mathrm{R}_d] = q s^{-1}$,
Farad	$[\mathrm{C}_d] = q^{-1} s^{2}$.

This manner of representing them has manifestly the advantage of greater simplicity; and at the same time it is further to be remarked that the units of other kinds of electric and magnetic quantity, not contained in the preceding table, assume a similar simple form when these fundamental units are employed; while if *gr*, *cm*, *s* be used, the formula of every new unit must be provided with a specially to be determined power of 10 as a factor, by which the distinctness and ready intelligibility of the formulæ are impaired.

The circumstance that, in order to obtain at once for electric units values agreeing to some extent with the quantities we have at other times to measure, and hence convenient for us, so very small a unit of mass and so very large a unit of length must be employed, is to be regarded as a characteristic peculiarity of electricity, and hence cannot be avoided. In order, then, to facilitate the representation in investigations in which such very small and very large units occur, it is desirable to be able to state them briefly, not only by mathematical symbols, but also in words; and I would make a proposal for that purpose.

In the French system of weights and measures, as is well known, for each kind of quantity, starting from the funda-

mental unit, the first three lower units resulting from divisions by ten are named by prefixing the words *deci*, *centi*, and *milli*, borrowed from the Latin, and the first four higher units, resulting from multiplications by ten, by prefixing the words *deka*, *hekto*, *kilo*, and *myria*, borrowed from the Greek. I propose, then, to employ for naming the lower units arising from further divisions by 10, and the higher units arising from multiplications by ten, likewise Latin and Greek numerals, but *ordinal* numerals, which will specify the negative or positive exponents of the powers of ten by which the fundamental unit is to be multiplied; so that, for instance, with the metre the fifth lower unit $1 . 10^{-5} m$ will be called a *quintometre*, and the fifth higher unit $1 . 10^{5} m$ a *pemptometre*, and correspondently with the gram and other fundamental units*.

According to this, we have to call the mass $1 . 10^{-11} gr$, occurring in the above system, an *undecimogram*, and the length $1 . 10^{7} m$ a *hebdometre*. The practical system of measures is consequently characterized in its entirety by the name *the electrodynamic system of measures undecimogram, hebdometre, second*. This name easily impresses itself in the memory; and by it the aim of a more precise definition of the system is more perfectly attained than by saying that it is the *electrodynamic system of measures gram, centimetre, second*, in which, however, each unit must be multipied by a specially to be stated power of ten.

§ 9. *The Critical System.*

We have seen, in section 5, that, in the different kinds of electric and magnetic quantity, the ratio of the dynamic unit to the value of the static unit measured according to dynamic measure, or, what comes to the same, the ratio of the value of the dynamic unit measured according to static measure to the static unit, is always represented by a power of the ratio of the critical velocity to the velocity-unit. It thence follows that, if the fundamental units be chosen so that the velocity-unit becomes equal to the critical velocity, then those ratios become each equal to 1. A system of measures in which this

* Only the higher unit resulting from multiplication by 10^{6} makes a little difficulty. As the sixth is ἕκτος in Greek, in accordance with the above we should have, properly, to call the quantity $1 . 10^{6} m$ a hectometre. But in the French system of measures the prefix *hekto* is already employed in another sense, namely as a contraction of ἑκατόν, so that *hektomètre* signifies $100 m$: and we must therefore seek help from some other source for the naming of $1 . 10^{6}$. To that end we might, *e. g.*, deviating somewhat from Greek usage, say *hexometre*, or even employ the prefix *mega*, proposed for this order of quantity by the British Association, and form the name *megametre*.

takes place will, as has been insisted by other authors, be distinguished above the rest by simplicity. Such a one may here, in conclusion, be discussed.

First, as to the time-unit to be employed in the new system, this can be selected at pleasure; but, since all the systems hitherto considered agree in having a second as the unit of time, there is no reason for choosing any other time-unit here, and we therefore retain the second.

By this determination of the time-unit the unit of length is also determined, because as such that length must be taken which a point endowed with the critical velocity would travel in one second. This amounts to approximately 30 meridian-quadrants, and is consequently about 30 times the unit of length of the practical system of measures. We will denote it by λ.

There still remains to be determined the unit of mass. This has no influence upon the unit of velocity; so that any value whatever can be given to it without altering the unit of velocity; hence it appears to me most expedient so to choose this value that two of the most important electrical units, viz. those of quantity of electricity and intensity of current, take the same values in the new system of measures as in the practical system. In the latter we have, according to the preceding section, to put

$$[e_d]=p^{\frac{1}{2}}q^{\frac{1}{2}} \text{ and } [i_d]=p^{\frac{1}{2}}q^{\frac{1}{2}}s^{-1};$$

and in the new system, if its unit of mass be denoted by μ, the following equations must hold:—

$$[e_d]=\mu^{\frac{1}{2}}\lambda^{\frac{1}{2}} \text{ ; and } [i_d]=\mu^{\frac{1}{2}}\lambda^{\frac{1}{2}}s^{-1}.$$

Now, if the latter values of $[e_d]$ and $[i_d]$ are to agree with the former, the products $\mu^{\frac{1}{2}}\lambda^{\frac{1}{2}}$ and $p^{\frac{1}{2}}q^{\frac{1}{2}}$ must be equal, and hence also

$$\mu\lambda=pq, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

from which follows

$$\mu=p\frac{q}{\lambda}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

Hereby the determination of μ is reduced to the determination of λ, and consequently to the determination of the critical velocity; and from what has been said above respecting the approximate value of λ, it results that μ is approximately $\frac{1}{30}$ undecimogram or $\frac{1}{3}$ duodecimogram—that is, a three billionth of a gram.

This system, in which the velocity-unit is equal to the critical velocity, may be called the *critical system of measures*; and here the values corresponding to it of the electric and magnetic units may also be collected:—

$$[m_d] = \text{v. d. } [m_s] = \mu^{\frac{1}{2}} \lambda^{\frac{3}{2}} s^{-1} = \frac{\lambda}{q} \text{ weber},$$

$$[e_d] = \text{v. d. } [e_s] = \mu^{\frac{1}{2}} \lambda^{\frac{1}{2}} = 1 \text{ coulomb},$$

$$[i_d] = \text{v. d. } [i_s] = \mu^{\frac{1}{2}} \lambda^{\frac{1}{2}} s^{-1} = 1 \text{ ampère},$$

$$[\mathrm{E}_d] = \text{v. d. } [\mathrm{E}_s] = \mu^{\frac{1}{2}} \lambda^{\frac{3}{2}} s^{-2} = \frac{\lambda}{q} \text{ volt},$$

$$[\mathrm{R}_d] = \text{v. d. } [\mathrm{R}_s] = \lambda s^{-1} = \frac{\lambda}{q} \text{ ohm},$$

$$[\mathrm{C}_d] = \text{v. d. } [\mathrm{C}_s] = \lambda^{-1} s^2 = \frac{q}{\lambda} \text{ farad}.$$

Although this critical system of measures is not suited for practical measurements, and especially for the preparation of standards, because the critical velocity is not yet known with sufficient precision, yet it may offer great conveniences in theoretic investigations, on account of the accordance between the values of the static and dynamic units.

L. *On the Question of Electrification by Evaporation.* *By* S. H. Freeman, *Fellow in Physics, Johns Hopkins University, Baltimore, Md.**

IT has been very commonly believed that evaporation is an important source of atmospheric electricity. So far as this belief has an experimental basis, it is to be found in the researches of Pouillet† and of Tait and Wanklyn‡. In comparison with their work the experiments of Volta, Saussure, and others are of little value. As the result of an elaborate series of experiments, Pouillet came to the following conclusions:—

1. Simple changes of state never give the least sign of electrification.

* Communicated by the Author.

† *Annales de Chimie et de Physique*, ser. 2. tome xxxv. p. 401. and xxxvi. p. 4.

‡ Phil. Mag. ser. 4. vol. xxiii. p. 494.

2. From solutions of alkaline solids the water evaporated carries away a negative charge.

3. From solutions of gases, acids, or salts the water carries off a positive charge.

Tait and Wanklyn repeated Pouillet's experiments with a more sensitive electrometer. They detected electrification where Pouillet had failed to do so, *e. g.* in the evaporation of distilled water; and in some other cases the effects obtained were totally different in kind and degree from those obtained by Pouillet and earlier experimenters.

In these experiments a few drops of liquid were dashed on a red-hot platinum dish insulated and connected with an electrometer. So long as the spheroidal state continued, little or no electrification was observed; but as soon as violent vaporization began, decided deflections were obtained. This fact and other circumstances about the experiments indicate that friction was the chief source of electrification, and make it doubtful whether evaporation had any part in producing the observed electrification. Tait and Wanklyn recognized the fact that to friction was due a large part of the electrification which they obtained. Still, in his lectures on Thunderstorms*, Professor Tait, speaking of atmospheric electricity, says:—"In calm clear weather the atmospheric charge is usually positive. This is very commonly attributed to evaporation of water: and I see no reason to doubt that the phenomena are closely connected." After reading these lectures, the writer of this article decided to investigate whether evaporation alone and under normal conditions causes electrification, and whether such electrification, if any, is sufficient to account for the phenomena of atmospheric electricity. In this connexion it will be remembered that Faraday traced the electricity, produced by the escape of steam or water from orifices, to friction, and concluded it was not due to evaporation, nor did it have any bearing on atmospheric electricity.

The investigation was begun with the expectation, on the part of the writer, of finding electrification; Prof. Rowland, to whom the author is much indebted, believed that no electrification would be found. In any case the subject seemed important enough to warrant experiments, which were free from the objections which hold against those already described. At the very beginning of the investigation it was found that at most the electrification due to evaporation would be extremely small, and very great caution would be necessary to eliminate various sources of error. The apparatus finally used

* 'Nature,' vol. xxiii. p. 340.

consisted of an evaporating-dish, 19 centim. in diameter and 1 centim. deep, insulated, and connected with one pair of quadrants of a Thomson's quadrant-electrometer. The other pair of quadrants and the case of the electrometer were connected with earth. The evaporating-dish was supported on a wire frame suspended by silk threads. To prevent disturbances due to electricity on neighbouring objects, particularly the clothes of the observer, it was found necessary to enclose the whole apparatus in a metallic case. Openings were made in this case, to facilitate evaporation and to permit the mirror of the electrometer to be seen. The commutator was governed by a silk thread passing through an opening in the case. In the experiments the results of which are given in Table II., the evaporating-dish was of copper; in the other cases it was of tin. The liquids examined were alcohol, sulphuric ether, water, and solutions of NaCl and of $CuSO_4$. The last three were most carefully studied—water and NaCl because of their importance in evaporation in nature, and $CuSO_4$ because of the very large deflections which Tait and Wanklyn obtained with it.

The electrometer was about thirteen times as sensitive as Prof. Tait's. Its deflections were reduced to absolute measure by comparison with a Daniell's cell, the deflections produced by which were observed before and after each series. The capacity of the evaporating-dish and its connexions was found, by comparison with Prof. Rowland's standard condenser, to be 65 centim. nearly. The rate of evaporation was determined by weighing the evaporating-dish and liquid before and after each series. It was found impossible to eliminate every source of electricity except evaporation, since deflections were always obtained even with the evaporating-dish dry. In order to correct for this, each series began and ended with two or more observations of the deflections obtained by insulating for a known time, usually five minutes, the dry evaporating-dish. Between these observations, several similar observations with the liquid evaporating were made.

As the result of a large number of observations, in which the evaporating-dish was insulated for five minutes,

Alcohol gave a potential from	−0·04 to −0·09	that of a Daniell's cell.
Cotton wet with alcohol gave a potential from	+0·02 to −0·06	,, ,,
Sulphuric ether gave a potential from . .	−0·02 to −0·10	,, ,,

(The sign indicates the charge apparently carried away by the vapour.)

No correction has been made in the above figures for the deflections obtained with the dry dish. These were more variable, but usually of the same sign as when a liquid was evaporating, and of a magnitude not very different. Making this necessary correction,

Alcohol gave a potential from	$-0{\cdot}024$ to $-0{\cdot}065$	that of a Daniell's cell.
Cotton wet with alcohol gave a potential from	$-0{\cdot}028$ to $-0{\cdot}050$	,, ,,
Sulphuric ether gave a potential from .	$-0{\cdot}004$ to $-0{\cdot}023$	,, ,,

In the above observations the rate of evaporation was not measured.

The earlier observations on water and salt water gave deflections about 0·1 that given by a Daniell's cell, the deflections with the dry dish being nearly the same (see Table I.). Though the dish was insulated for five minutes or more, the position of the spot of light was read every thirty seconds. Far the greater part of the deflection was attained in the first minute or two; and after five minutes scarcely any further deflection could be discovered.

The observations thus indicated that the small deflections obtained were mainly due to leakage from electrified parts of the apparatus. The sources of this leakage were carefully investigated; and three principal ones were discovered, viz. the charged needle of the electrometer, the vulcanite posts supporting the electrodes of the electrometer, and the glass cup of the commutator. In the later experiments care was taken to eliminate these sources of leakage as far as possible. The connexions were so made that the electrodes and commutator were not touched for days before the experiments, and the charge on the needle was not replenished within about twenty-fours of the experiment. When these precautions were taken the deflections became less than 0·01, and generally less than 0·005 of a Daniell's cell (see Table II.).

In contrast with these deflections, Tait and Wanklyn obtained, by dashing liquids on red-hot platinum, for

solution $CuSO_4$	a potential	$+300$	times	that of a Daniell's cell,		
solution NaCl	,, ,,	$+120$	,,	,,	,,	,,
distilled water	,, ,,	$+\ 24$	,,	,,	,,	,,
alcohol	,, ,,	$+\ 3$	,,	,,	,,	,.

In order to compare the deflections obtained by evaporation with those obtained by friction, the experiments of Tait and Wanklyn were repeated. It was impossible, however, to measure the deflections obtained, since in almost every case, as soon as the spheroidal state ceased, the spot of light went beyond the limits of the scale.

To show the sensitiveness of the apparatus to very slight friction, it was so arranged that drops of liquid could fall upon the evaporating-dish from an insulated conductor in metallic connexion with it. A few drops of liquid falling three or four centimetres were sufficient to give deflections of nearly the size obtained from a Daniell's cell, and much larger than any obtained in evaporation.

In order to compare the results obtained with the phenomena of atmospheric electricity, the writer computed the virtual depth of water which would be necessary to produce one flash of lightning if the vapour in a thunder-cloud had been electrified as indicated by the experiments. By virtual depth is meant the depth of the layer of water over the whole area of the cloud, were all its vapour condensed to the liquid state.

Let Δ = this depth in centimetres for a cloud of area A and height a,

V and C = respectively the potential and capacity of the cloud,

q = the charge per gramme of water;

then

$$\Delta = \frac{VC}{Aq}.$$

But, from Sir Wm. Thomson's experiments, $V = 130a$*; and by the ordinary formula for a condenser of horizontal plates,

$$C = \frac{A}{4\pi a}.$$

Now, in the experiments,

let d = the deflection obtained by insulating for t minutes, and apparently due to evaporation,

D = the deflection obtained from a Daniell's cell,

W = weight, in grammes, of liquid evaporated in T minutes,

v and c = potential and capacity of evaporating-dish and its connexions;

* Papers on Electricity and Magnetism, p. 259.

then

$$q = \frac{vc\mathrm{T}}{t\mathrm{W}}.$$

But $c = 65$ centimetres, as stated above; and $v = 0{\cdot}00374\,\frac{d}{\mathrm{D}}$ in electrostatic units, according to Sir William Thomson's measurements*. Then

$$\Delta = 43\,\frac{t\mathrm{W}\mathrm{D}}{\mathrm{T}d},$$

in terms of the original measurements in the experiments.

TABLE I.

Showing Results of earlier Series.

Liquid.	Deflection with evaporation.	Deflection with dry dish.	*d*, in scale-divisions.	D, in scale-divisions.	*t*, in minutes.
Distilled water	4·12	4·60	+0·48	42·02	5
	4·82	4·33	−0·49	42·05	5
	3·95	4·50	+0·55	41·55	5
	2·48	2·33	−0·15	42·00	5
Solution NaCl	6·70†	4·68	−2·02	41·75	5
	3·47	4·40	+0·93	41·50	5
	3·10	3·94	+0·84	42·20	5

Liquid.	T, in minutes.	W, in grammes.	Δ, in centimetres.	Sign of apparent charge of vapour.
Distilled water	43	1·44	630	+
	43	0·49	210	−
	37	0·54	237	+
	41	0·80	1170	−
Solution Na Cl	55	0·29	24	−
	54	0·48	85	+
	39	0·70	194	+

* Papers on Electricity and Magnetism, p. 245.

† Throughout this series there was an instantaneous deflection at breaking contact, which was several times larger than the total deflection obtained during the five minutes' insulation. The series is therefore of much less value than any of the others, in which the instantaneous deflection was never more than 0·1 of a division.

TABLE II.

Showing Results of later Series.

Liquid.	Deflection with evaporation.	Deflection with dry dish.	*d*, in scale-divisions.	D, in scale-divisions.
Saturated solution $CuSO_4$	0·3	0·0	+0·30	37·6
	0·12	0·6	+0·60	37·6
	0·8	0·6	+0·20	37·6
	0·6	0·42	+0·18	39·6
	0·5	0·21	+0·29	39·6
	0·1	0·21	−0·11	39·6
	0·3	0·21	+0·09	39·6
Saturated solution NaCl	0·0	0·35	−0·35	39·6
	0·2	0·12	+0·08	39·6
	0·4	0·35	+0·05	39·6
Hydrant water	1·6	0·6	+1·00	37·6
	1·45	0·6	+0·85	37·6
	1·3	0·6	+0·70	37·6
Distilled water	0·5	0·6	−0·10	39·6
Cotton wet with hydrant water	1·0	0·66	+0·34	37·6

Liquid.	*t*, in minutes.	T, in minutes.	W, in grammes.	Δ, in centi-metres.	Sign of apparent charge of vapour.
Saturated solution $CuSO_4$	2	174	3·19	198	+
	2	174	3·19	99	+
	10	174	3·19	1482	+
	10	38	0·9	2240	+
	5	38	0·9	695	+
	5	38	0·9	1833	−
	5	38	0·9	2240	+
Saturated solution NaCl	6	30	0·54	525	−
	2	30	0·54	766	+
	6	30	0·54	3678	+
Hydrant water	10	30	0·59	318	+
	10	30	0·59	373	+
	10	30	0·59	454	+
Distilled water	7	35	0·89	3031	−
Cotton wet with hydrant water	11	30*	0·59*	1029	+

The actual value of Δ (the virtual depth of water) for a cloud 1500 metres thick, if condensation began at

35° C., is about 5·9 centim.
30 „ „ 4·5 „
25 „ „ 3·4 „
20 „ „ 2·6 „
15 „ „ 1·9 „

* In this case the rate of evaporation was not measured, but was assumed in computing Δ to be the same as for hydrant water at the same time. The rate was probably greater, which would make Δ still greater.

Hence, even if the deflections d of the tables be really due to evaporation, and if evaporation be the principal source of the atmospheric electricity, the quantity of water which would be required to produce a single flash of lightning is very much greater than the actual quantity ever found in a thunder-cloud, while the thunder-cloud usually gives, not one, but many flashes.

It may be objected to this reasoning that a thunder-cloud in some way collects the electricity obtained by the evaporation of a much larger quantity of water than that contained in itself. On this account it is of interest to consider the total evaporation on the globe. If 100 centim. be taken as the average annual rainfall for the whole surface of the earth, and 400 centim. be taken as the value of Δ, the total annual evaporation on the earth is sufficient to produce only about 14,000,000 flashes of lightning from clouds 9 square kilometres in extent. Hence, if one were able to observe every flash of lightning which occurs within 18 kilometres of himself, the average number of lightning-flashes seen by such observers at different points of the earth's surface in one year would be only twenty-eight. Calculation from more exact data would give a less number. This calculation assumes that all the electricity is discharged through lightning-flashes, whereas a very large portion of the atmospheric electricity is discharged in other ways—*e. g.* in the aurora borealis and other atmospheric phenomena which are referred to electricity, and in quiet discharges which take place without any noticeable accompanying phenomena.

Evaporation is then, at most, a very insignificant source of the atmospheric electricity. But, further, the following facts are to be observed :—

1. The deflections obtained in the experiments were always very small. (In the original readings it was easy to make an error of 0·1 of a division ; and though the numbers in the tables are the means of several observations, they are still affected by this error of reading.)

2. The sign of the apparent charge is not always the same for the same liquid. This is particularly noticeable in the important case of water.

3. The deflections were very much diminished when care was taken to eliminate very small sources of electricity. Still the deflections obtained with the evaporating-dish dry, are usually much larger than the difference between these and the deflections obtained when a liquid was evaporating.

Evidently, then, most of the electrification was due to other causes than evaporation; and the experiments do not certainly

trace any electricity whatever to this source. The problem of the source of the electricity of the atmosphere is still unsolved. Evaporation, first proposed by Volta, whose theory until now has been better supported by experiment than any other, fails to account even for a small portion of it; and no other source has been proposed which can yet be considered sufficient.

LI. *On the Function of the two Ears in the Perception of Space.* *By* Professor SILVANUS P. THOMPSON, *D.Sc., B.A.*

1. THE conceptions formed in our minds of the extension of space may be resolved into two parts:—(1) the conception of distance independent of direction: (2) the conception of direction independent of distance.

These conceptions are founded upon the perceptions of three separate senses, each of which, though distinct from the others, enables us to form conceptions both of linear magnitude (or distance) and of angular magnitude (or direction). These three senses are:—

(*a*) The muscular sense;
(*b*) The optical sense;
(*c*) The auditory sense.

(*a*) The *muscular* sense gives us direct perceptions of linear magnitude and of angular magnitude through the sensations produced by extending the limbs and by twisting the frame and head.

(*b*) The *optical* sense gives us perceptions of angular direction of two kinds:—the first of them depending upon the optical formation of images of external objects upon the differentiated nerve-structures of the retina of the eye; the second of them being partly optical, partly muscular, and derived from the sense of muscular effort required to move the eye-balls, or the whole head, into the position in which light from an object situated in any given direction shall be most easily observed. The optical perception of distance is of three kinds—one kind being indirect and associative, the other two muscular. The *first* of these depends upon the possession of two eyes, giving, in consequence, two retinal pictures with slight differences, which we gradually learn to associate with varying conditions of distance. The *second*, and principal, of them depends also upon the possession of two eyes, the greater or lesser amount of muscular effort required to converge their optic axes in viewing an object affording a basis for a mental estimate of

* Communicated by the Author, having been read before Section D of the British Association at York, September 1881.

distance. The *third* of them does not depend upon the possession of two eyes, or upon "binocular parallax," but consists in the perception by the muscular sense of the amount of effort which the ciliary muscle must exert to focus the eye for rays proceeding from an object at a given distance. The author has elsewhere * shown how this last mode of perception is complicated by the question of the colour of the rays in consequence of the imperfect achromatization of the eye. To these three a fourth, and purely associative, kind of perception of distance must be added, in the apparent magnitude of objects of known size. Of these modes of perception, the two depending on binocular parallax are of greatest importance; without them our optical conceptions of solidity and distance would be extremely imperfect. The theory of binocular vision was practically complete when the invention of the stereoscope, and of its *reductio ad absurdum* the pseudoscope, proved the general correctness of Wheatstone's views, though several corollaries to the theory have since been elaborated by younger workers.

(*c*) The *auditory* sense may in like manner afford us perceptions of linear and of angular magnitude—that is to say, of distance and of direction. But the theory of the acoustical perception of space is strangely incomplete. Just as the possession of two eyes gives us by binocular parallax the most important of the optical means of perception of space, so the possession of two ears is found, from the researches of Mach, Luca, Steinhauser, Graham Bell, and of the present writer, to give us, by binaural parallax, important means for receiving acoustical perceptions of space. Besides these binaural perceptions, it is undeniable that imperfect monaural perceptions of space exist. These will be noticed in passing.

2. Any comparison of the *ear* and *eye* in respect of their power of affording perceptions of space would be incomplete if three points of essential difference in the arrangements of these organs were not adverted to:—

(i) The ear has no lens, and nothing equivalent to a lens by which a sound-wave proceeding in any given direction could be focussed on the receptive mechanism. The pinna or auricle of the ear and its auditory meatus do not even serve, as a hollow parabolic reflector might serve for light, to focus rays coming in different directions on different points of the receptive membrane.

(ii) The receptive nerve-structure of the ear is differentiated in a wholly different manner from that of the eye, so that its different parts are not sensitive to different *directions* in the

* Phil. Mag. July 1877, [5] vol. iv. p. 48.

movement of acoustic waves, but only to their difference in *frequency*.

(iii) The ears cannot be turned toward different directions as the eyes can be, independently of the movements of the head, at least in man. There seems no reason, however, to doubt that the faculty of moving the pinnæ of the ears is useful to animals in their perception of the direction of a sound. It is a matter of common knowledge that horses, asses, cats, and dogs do thus use their pinnæ in ascertaining the direction of a sound. In these cases, however, it is doubtful whether there is any muscular sense of convergence to give (as with the eye) a perception of distance.

3. There are four physical characteristics of waves of sound by which one sound is discriminated from another, viz.:—

(i) *Intensity*, or loudness, depending upon extent or energy of the vibratory motions.

(ii) *Pitch*, or frequency, depending upon the rapidity of the vibratory motions.

(iii) *Phase* of the vibratory motions, as to whether moving backward or forward or at any other state.

(iv) *Quality*, or timbre, depending upon the degree of complexity of the vibratory motion.

The third of these physical characteristics is one for which the single ear possesses no direct means of perception. The author of this paper discovered in 1877 *, however, that in binaural audition a perception of difference of phase did exist; and the same discovery was made independently in the succeeding December by Prof. Graham Bell and Sir William Thomson. The existing theories of the acoustic perception of space, which will now be examined, are founded upon the perceptions of these four physical characteristics.

4. *Theories of* Steinhauser *and of* Graham Bell.—A very complete and careful theory of binaural audition was published in 1877 by Prof. Anton Steinhauser †, who worked out on geometrical principles the laws which determine the *relative intensity* with which a sound will reach the two ears when starting from any given point of space. The intensities are equal in the two ears when the source of sound is in the median plane, and is a maximum when in front, a minimum when behind the head, since the ears are set angularly (so as to catch sounds from the front of the head) in planes which determine, according to Steinhauser, the conditions of best hearing. The formula deduced by Steinhauser is the follow-

* Rep. Brit. Assoc. 1877, p. 37; Phil. Mag. [5] vol. iv. Oct. 1877, p. 274.

† Vienna, 1877: Phil. Mag. [5] vol. vii. pp. 181, 261.

ing:—Let β be the angle between the plane of the ear and the line of sight, and α the angle which the line of sight makes with the direction of the sound; and let i_1 and i_2 represent the relative intensity with which the sound is heard in the two ears; then

$$\tan \alpha : \tan \beta :: i_1 - i_2 : i_1 + i_2.$$

The operation, therefore, of finding the direction of a sound —say a lark singing high up in the air—will, according to Steinhauser, be as follows:—First, the head is rotated on its axis horizontally until the sound is equally loud in both ears: then the head is moved up and down until a maximum of loudness is discovered, when the lark will be found in the line of sight. Steinhauser's theory, it will be noted, takes into account no differences of phase, pitch, or quality, but of intensity only; and it fails, as will be seen, to account for the fact that we have, *without moving the head at all*, a very fair perception of the direction of sounds. It also presupposes (what the experiments of Crum Brown and M'Kendrick have pretty definitely shown to exist) that we possess a rotation-sense, a means of perception of angular movement in horizontal and meridional planes. Steinhauser devised an instrument, which he named the *homophone*, wherewith to test his theory. It consisted of a system of wooden tubes for bringing to the ears the sounds of two organ-pipes tuned to unison, whose respective intensities could be regulated by stopcocks. It was held by its inventor to confirm his theory.

A similar theory has been implicitly adopted by Prof. Graham Bell, in the paper on Experiments relating to Binaural Audition which he read before the American Association for the Advancement of Science in 1879*. Graham Bell investigated the matter with telephones connected to the ears, with some curious and valuable results. He also experimented on the degree of accuracy with which the ear can determine the direction of a sound, and found it to be most accurately defined for those sounds the direction of which approximates to the axial line of the ears.

5. *Theories of* A. M. Mayer *and* (*provisionally*) *of* S. P. Thompson.—A modification of the preceding theory was provisionally adopted by the present writer in the earlier part of his researches. He had discovered that when two sounds agreeing in every respect, save in phase, are led to the ears, a singular phenomenon occurs, complete difference of phase producing an acoustical illusion as if the sound were localized in the back of the skull. He therefore suggested that differ-

* American Journal of Otology, July 1880, p. 169.

ence of phase, being thus recognizable, might assist the observer in finding the true direction of a sound; for when his gaze was directed in the precise direction, not only would the relative intensities be equal, but the phase would be identical, and the sensation would be free from complication with the illusion.

Prof. Alfred M. Mayer of the Stevens Institute, in the course of his acoustic researches, suggested an instrument which he called the *topophone*, the object of which is to ascertain the direction of a source of sound, and which depends upon differences of phase and of intensity. An experiment of Zoch (see Radau, *Die Lehre vom Schall*, p. 246) for comparing the relative phases of two simple unison-tones by means of König's manometric flames, suggested to Mayer an instrument composed of two resonators fixed at the ends of a T-shaped frame, and connected with manometric flames, whose objective vibrations, as seen in a rotating mirror, would be synchronous when the phases and intensities of the sound-waves received in the two resonators were alike, or when the cross-arm on which they were fixed stood at right angles to the direction of the sound. In another form of the instrument, the frame carrying the resonators was fixed by a kind of yoke to the shoulders, the resonators being respectively connected by tubes to the two ears. Comparatively small angular movements of the head would then produce considerable differences of *phase*, rendering the perception of direction more narrowly accurate.

6. *Theories of* Mach *and of* Lord Rayleigh.—Prof. Mach, of Prague, advanced the theory (*Archiv für Ohrenheilkunde*, 1874) that the perception of direction of sound arose from the operation of the pinnæ of the ears as resonators for the higher tones to be found in the compound sounds to which the ear is usually accustomed; their action as resonators should be more or less effective according to the position of the pinnæ with respect to the direction of the sound-waves; and by thus reinforcing in different positions with unequal intensity, some one or more of the higher tones of a compound sound should affect the *quality* or timbre of the perceived sound, producing a difference between the sounds heard in the two ears in all positions, save when the source of sound lay in the median plane of the head. There should also, according to this theory, be a difference in the quality of the perceived sound (owing to the dissymmetry of the upper and lower parts of the pinna) for sounds in the median plane according as they were above, below, or in the line of sight. It will be convenient to consider along with the theory of Mach the somewhat similar theory of Lord Rayleigh*, propounded in 1876 before the

* Trans. Mus. Assoc. 1876.

Musical Association. Lord Rayleigh pointed out that, as the size of the head bears a finite proportion to the size of sound-waves, there will be no sharp shadows of sounds occurring at the opposite side of the head to that at which a sound arrives, but that *diffraction* of the sound-waves will take place, which, in the case of complex sounds, will produce the result that the partial tones of different pitch will arrive at the side of the head opposite to that nearest the source of sound with very different intensities. Thus, without making any assumption as to the functions of the pinnæ as resonators, Lord Rayleigh's theory agrees with that of Mach in attributing the acoustic perception of direction to differences of *quality* or timbre between the two ears, the brain drawing, from the slight differences of the tones received in the two ears, an unconscious judgment based on empirical observation.

7. One other theory only will be mentioned, and this only to be at once dismissed. It is the theory advanced by Küpper *, that sound-waves proceeding in different directions affect different parts of the tympanum, and so give rise to different sensations. This is physically untenable, because the tympanum is not the true receptive organ with differentiated nerve-structures, but only a part of a mechanical device by which the alternating compressions and rarefactions taking place at the bottom of the tube of the ear are conveyed to the true receptive organ in the labyrinth of the internal ear. It is also untenable because, as Mach and Fischer have shown, the outer ear cannot act as a true reflector except for waves whose length is a small fraction of its own dimensions, which is only true for sounds of such excessive shrillness as to be out of the range of ordinary sounds. [The shrillest sound audible in human ears is of wave-length 0·4 of an inch.] It is, finally, untenable because, in fact, nothing is more difficult than to tell the direction of sounds whose source is in the median plane of the head—front, back, zenith, and nadir being usually undistinguishable except for very well-known sounds.

8. In order to test the rival theories of binaural hearing, the author of this paper devised a little instrument, described before the British Association (Section A) in 1879 under the name of the *pseudophone* †, which is for the ears what Wheatstone's pseudoscope was for the eyes, an instrument for verifying the laws of perception by means of the illusions which it produces. The *pseudophone* consists merely of a pair of adjustable flaps or reflectors which can be fitted to the ears, and which can be set at any desired angle to catch sounds

* *Archiv für Ohrenheilkunde*, n. F., Bd. ii. part 3, p. 158.

† Phil. Mag. [5.] vol. viii. 1879, p. 385.

from back, front, above, or below. By altering the position of the flaps, we alter the relative intensities of the two sounds as received in the ears; and this can be done without the blindfolded observer knowing how the flaps are set. If, for example, the flaps are set to catch sounds from behind, the observer will imagine that he is looking in the direction of the sound when he is looking in precisely the reverse direction. But when the *simple tones* of tuning-forks are used, the instrument fails to gives satisfactory illusions, except for very shrill tones, when the experiments are made out of doors.

9. The following criticisms on the rival theories and on some of the results of mathematical and experimental deductions therefrom will now be in place.

(*a*) All the theories assume that we have a rotation-sense—that is to say, that when we turn round to face a sound, we are conscious how much (in angular measurement) we have turned round. Lord Rayleigh, in his experiments, kept the head still, and estimated the angular direction of the sound without turning the head. In this case association with previous perceptions of rotational movement must, however, be admitted as entering into the mental process of judging of angular position.

(*b*) The theory of Steinhauser leads to the mathematical deduction that perception of direction is the most exact for sounds situated in the *plane of best hearing*, or in the median plane and in front of the head. The experiments of Graham Bell, on the other hand, as well as those of Mach and of Lord Rayleigh, are conclusive that the perception of "the direction of a sound is more accurately defined as it approximates to the axial line of the ears."

(*c*) Steinhauser's theory and his fundamental formula given above in § 4 assume that the ratio between the tangents of the angles will be the same in our perception as the ratio between the angles themselves. This is only true when both are small, and when therefore the source of sound is nearly in the median plane. Unfortunately for the theory, for very small angles the perception ceases to be accurate, especially for small angles near the median plane.

(*d*) Steinhauser's theory assumes the pinnæ to act as reflectors and funnels; he further assumes the magnitude of the "effective surface" to be equal for sounds coming at all angles, which is by no means proven or even probable.

(*e*) It is a mathematical consequence of Steinhauser's theory, that if the plane of the effective surface of the pinna makes an angle of less than 30° with the line of sight, a sound will be heard louder when opposite one ear than when placed

at an equal distance in front of the head. With most people this angle is less; but most people, at any rate, prefer to face the source of sound when listening.

(*f*) It may be deduced from Steinhauser's theory that the perception of direction should be more accurately defined in those persons for whom the angle between the effective plane of the pinna and the line of sight is as small as possible. If this be so, persons whose ear-flaps stand off from their heads at a considerable angle should be bad judges of the direction of sounds, which is not, in general, the case. The behaviour of animals in bringing forward the openings of the pinnæ when listening intently to a sound is contrary to this point. If the deduction were true, women should be better judges of the direction of sounds than men, as their pinnæ usually are less prominent. In the experiments of Graham Bell with telephone-receivers when they were set at an angle with one another, he was able to perceive the direction of sounds more accurately than when the receivers were set parallel, the difference between back and front being thus capable of discrimination; but he does not state whether this discrimination arose merely from difference in relative intensity or from difference of quality.

(*g*) If the theory of Steinhauser is true, the perception of direction should be more accurate for sounds when their source is far away than when it is near the head. If the theory of Lord Rayleigh and of Mach is true, the perception of direction should be more accurate for sounds whose source is nearer the head. The latter is shown by experiment to be true for all kinds of sounds.

(*h*) The theory of Lord Rayleigh is supported by the experimental fact that the quality of sounds does, as a matter of fact, differ when they are heard in different directions. Compare, for example, the sound of a musically ticking clock placed in front of the head with the sound of the same clock when the head is turned in the opposite direction. To the writer the tick of a watch always sounds deeper in pitch when held in front of the head than when held opposite either ear, in each case at equal distances of about one foot away.

(*i*) The theory of Lord Rayleigh also derives support from the following observation. To analyze a compound sound by ear, so as to say what partial tones are present, requires considerable practice, and is even then not easy. But the presence or absence of partial tones is rendered much easier of detection if the head be turned about, so that their various degrees of intensity as affected by diffraction or by phase-difference may bring one or other into prominence.

(*j*) The theories of Mach and Lord Rayleigh agree with that of Steinhauser in one respect. They assign, it is true, the perception of direction of a sound to the effect of resonance or of diffraction in partially resolving a compound tone; but they make the perception of this splitting-up of the sound to depend upon the *unequal intensity* with which the resolved sounds are received in the two ears.

(*k*) The experiments of Lord Rayleigh show that our perception of direction is much more accurate for compound sounds than for simple tones.

(*l*) The experiments of the writer with the pseudophone show that the acoustic illusions are produced much the most satisfactorily when shrill sounds, such as the sound of a whistle or the click of a metronome, are employed.

(*m*) Experiments made with very low tones show that for these the sense of direction is extremely inaccurate. In this case the extreme effect of diffraction would cause the sound to be heard almost equally loudly by both ears in all positions.

10. It would therefore appear that Steinhauser's theory may be true for shrill and simple tones, and therefore true for the particular case of the lark. But for baser tones and for compound tones it cannot be regarded as true. For these the resolution-theories of Mach and Lord Rayleigh are much more nearly in accord with the facts of observation; though it probably yet remains to be determined whether the resolution of compound tones supposed in those theories be due, as Lord Rayleigh assumes, to diffraction round the head, or, as Mach assumes, to the pinna and its convolutions acting as resonating-cavities to reinforce certain of the partial tones of the compound sound. The former is the more probable in the opinion of the present writer; but it is not without its difficulties.

If in binaural hearing the direction of sound were to be estimated only by the process of moving the head until the line of sight coincided with that of the sound, then Steinhauser's theory accounts best for the facts.

If, however, the direction be estimated while the head is held immovable, then the resolution theories are certainly the more satisfactory.

In view of the fact (as apparently shown by Graham Bell's experiments) that even in *monaural audition* there is a fair perception of the direction of sound, Steinhauser's theory utterly fails to have any significance; while the theories of Mach and of Lord Rayleigh are amply sufficient for the facts.

11. The writer therefore propounds the following theory, which he believes to cover all the facts observed up to the present time.

Judgments as to the direction of sounds are based, in general, upon the sensations of different intensity in the two ears: but the perceived difference of intensity upon which a judgment is based is not usually the difference in intensity of the lowest or fundamental tone of the compound sound (or "clang"), but upon the difference in intensity of the individual tone or tones of the clang for which the intensity-difference has the greatest effective result on the quality of the sound.

It must be admitted that the acoustical perception of direction is, after all, not intuitive, but associative, just as the optical perception of direction is. The ear has been trained from childhood to associate certain differences in the quality of sounds (arising from differences in the relative intensities of some of the partial tones that may be present) with definite directions; and, relying on these associated experiences, judgments are drawn concerning sensations of sound whose direction is otherwise unknown. For sounds that are familiar a difference of quality as heard *in one ear* will at once suggest a direction. It is completely open to doubt whether a pure simple tone heard in one ear could suggest any direction at all.

12. *Acoustical Perception of Distance.*—There remains for discussion the acoustical perception of distance.

In the case of known sounds we doubtless judge chiefly of their distance by their relative loudness, the intensity decreasing inversely as the square of the distance. The analogous fact in optics is the perception of distance by the apparent magnitude of objects familiar to the sight and of known size.

It might be possible indirectly, in the case of short distances, to judge of distance by "acoustic parallax"—that is to say, by the difference in the direction of the sound as perceived in the two ears. The quality of a compound sound also differs slightly with distance (independently of direction); and from this fact a judgment might be drawn. It must be held extremely doubtful, however, whether any such judgments are actually made.

13. *Conclusion.*—The foregoing arguments and deductions have been based upon observations made, so far as the author is aware, exclusively by and upon persons possessed of normal powers of hearing. Nevertheless the subject is of such a nature that its more obscure points would doubtless be greatly

elucidated by observations made by and upon persons whose hearing-power is abnormal. The author has had no opportunities of making such observations. He has heard of one case of interest—that of a gentleman stone-deaf of one ear from infancy, and in whom the acoustic perception of direction was almost wholly wanting. Not only are experiments greatly wanted upon semi-deaf persons, but also upon blind persons, who usually have the acoustic perception of direction abnormally acute. It would be still more desirable to have observations made on a blind person deaf of one ear.

It is also greatly to be desired that further experiments should be made to determine the exact function of the pinnæ and of their complicated forms. The convolutions of the pinnæ differ markedly in different persons, and such differences must have *some* effect on the perception of sound. The suggestion of Mach, that the convolutions have no direct acoustic function in man, but that they represent the corresponding structures which in animals serve to direct the opening of the pinnæ or to prevent them from being turned inside out, is probably true, and is in conformity with the doctrine of descent, which accounts for many other inherited but functionless appendages. Yet, if true, this does not affect the truth of the statement that the convolutions, though destitute of direct function, must still exercise some effect on the perception of sound. In particular, it may be remarked that the outer cartilaginous rim of the ear curves forward at the upper part of the pinna, as if to catch sounds proceeding from below the observer and in front of him. Is it possible that this arises by natural selection from the fact that, on level ground, sounds reach us more intensely from below, by reflexion from the ground? And is it not also connected with the fact that persons who are partially deaf in both ears habitually throw the head a little backward, so as to raise their line of sight above the direction of the sound of the speaker's voice?

Whether the varieties of form of the pinnæ eventually prove to be of little or no importance in the question, there is an ample field for observation for anatomists and physiologists in the problems of the acoustical perception of space; and it is hoped that research in this department of physiological physics will be facilitated by the attempt now made to lay down the physical and experimental bearings of the question.

LII. *A Theorem on the Dissipation of Energy.* By S. H. Burbury*.

I THINK the following theorem has never received a formal proof.

A material system being in motion, and there being dissipation of energy by friction or otherwise, external forces, F, are applied to maintain certain of the velocities constant. If under these circumstances the system ultimately acquires a motion in which it might move were there no dissipation and no external forces, then in such motion the energy dissipated per unit of time is minimum, given the constant velocities.

For let $q_1, q_2, \dots q_n$ be the generalized coordinates defining the position of the system; $\dot{q}_1, \dot{q}_2, \dots \dot{q}_n$ the corresponding velocities; $p_1, p_2, \dots p_n$ the corresponding generalized components of momentum.

Let $\dot{q}_1, \dot{q}_2, \dots \dot{q}_r$ be the velocities which are maintained constant by the application of the forces F.

Then we have for each coordinate, by Lagrange's equations,

$$\frac{dp}{dt} - \frac{d\mathrm{T}}{dq} + \frac{d\mathrm{U}}{dq} = \mathrm{F}_q - \frac{d\mathrm{H}}{d\dot{q}},$$

where T is the kinetic, U the potential energy, F_q is the generalized component of the external forces corresponding to q, and H is Lord Rayleigh's dissipation function, namely the energy dissipated per unit of time. The ultimate motion is by hypothesis one in which the system might move were there no dissipation and no forces F. That is ultimately, by hypothesis,

$$\frac{dp}{dt} - \frac{d\mathrm{T}}{dq} + \frac{d\mathrm{U}}{dq} = 0,$$

and

$$\mathrm{F}_q = \frac{d\mathrm{H}}{d\dot{q}}$$

for each coordinate.

Now let us suppose that in the infinitely small time dt, the given velocities $\dot{q}_1 \dots \dot{q}_r$ would be diminished by dissipation alone by the quantities $\dot{q}'_1 \dots \dot{q}'_r$. Then, in order to maintain them constant, the forces F must in that same time dt produce in the system the velocities $\dot{q}'_1 \dots \dot{q}'_r$. We may suppose the forces Fdt applied impulsively at the end of the interval of time dt to produce the velocities $\dot{q}'_1 \dots \dot{q}'_r$.

Now a set of impulses applied to produce in the system the *prescribed* velocities $\dot{q}'_1 \dots \dot{q}'_r$ will generally produce at the same time other velocities $\dot{q}'_{r+1} \dots \dot{q}'_n$, the whole impulsive motion being, according to Thomson's theorem, determined

* Communicated by the Author.

by the condition that the kinetic energy of the system moving with these velocities $\dot{q}'_1 \ldots \dot{q}'_r \ldots \dot{q}'_n$, and no others, is minimum, given $\dot{q}'_1 \ldots \dot{q}'_r$. That is, if $p'_1 \ldots p'_n$ be the components of momentum corresponding to $\dot{q}'_1 \ldots \dot{q}'_n$, $\Sigma p'\dot{q}'$ is minimum, given $\dot{q}'_1 \ldots \dot{q}'_r$. The solution of which is

$$p'_{r+1}=0 \ldots p'_n=0.$$

But $p'_1 \ldots p'_n$ being the components of momentum produced impulsively by the force $\mathrm{F}dt$, each component of momentum is equal to the corresponding component of force. That is, for each coordinate

$$p'=\mathrm{F}_q dt.$$

But, as we have seen,

$$\mathrm{F}_q=\frac{d\mathrm{H}}{d\dot{q}};$$

therefore

$$p'=\frac{d\mathrm{H}}{d\dot{q}}dt.$$

Therefore, since $p'_{r+1}=0$ &c., $p'_n=0$,

$$\frac{d\mathrm{H}}{d\dot{q}_{r+1}}=0 \ldots \frac{d\mathrm{H}}{d\dot{q}_n}=0.$$

And therefore H is minimum, given $\dot{q}_1 \ldots \dot{q}_r$.

As an example of the theorem we may consider the distribution of an electric current in multiple arc. Let a set of wires, $\mathrm{OC_1P}$, $\mathrm{OC_2P}, \ldots \mathrm{OC}_n\mathrm{P}$, be joined at O and P, and a constant current be maintained outside the junction at O. Let $\dot{q}_1 \ldots \dot{q}_n$ denote the currents in the several wires, $p_1 \ldots p_n$ the corresponding components of electrokinetic momentum. Then the current at O is $\Sigma\dot{q}$, and is to be maintained constant notwithstanding the resistance of the wires.

Let $\dot{q}'_1 \ldots \dot{q}'_n$ be the diminutions of the several currents which would be caused by resistance in the infinitely small time dt. Then $\Sigma\dot{q}'$ is the diminution of the current at O due to resistance in time dt. In order to maintain $\Sigma\dot{q}$ constant, we may suppose an impulsive electromotive force applied at O at the end of the interval dt to produce the current $\Sigma\dot{q}'$.

The distribution among the wires of this current $\Sigma\dot{q}'$, *at the instant of its creation*, for however short a time such distribution may continue, is determined by the laws of induction, and will, by Thomson's theorem, be such as to make the electrokinetic energy of the impulsive currents minimum, given $\Sigma\dot{q}'$. That is, if p' be the component of electrokinetic momentum corresponding to $\dot{q}'$, $\Sigma p'\dot{q}'$ is minimum, given $\Sigma\dot{q}'$. That is,

$$p'_1=p'_2=\ldots=p'_n.$$

But if E be the component of electromotive force correspond-

ing to any wire,

$$E_n dt = p',$$

and therefore

$$E_1 = E_2 = \ldots = E_n.$$

Now for each wire

$$\frac{dp'}{dt} = E - R\dot{q},$$

where R is the resistance in the wire. Ultimately, when the motion becomes steady, which it does generally in a very short time,

$$\frac{dp'}{dt} = 0,$$

and therefore

$$E = R\dot{q}.$$

But

$$E_1 = E_2 = \ldots = E_n;$$

therefore

$$R_1\dot{q}_1 = R_2\dot{q}_2 = \ldots = R_n\dot{q}_n;$$

and therefore $\Sigma R\dot{q}^2$, or the heat generated in the wires per unit of time, is minimum, given $\Sigma\dot{q}$.

This is the distribution according to Ohm's law, which thus appears as an analytical consequence of Thomson's theorem.

LIII. *On Retardation of Chemical Action.*

By John J. Hood, *B.Sc., Assoc. R.S.M.**

In two former papers published in this Journal† I have given the results of a series of experiments on the rate of oxidation of ferrous sulphate by potassic chlorate, and showed that the course of the reaction for equivalent quantities of the substances is represented by the equation $y(a+t)=b$; the variables being the amount of unoxidized iron expressed in cubic centimetres of a solution of potassic permanganate, and time elapsed from a fixed point in the reaction. This equation is established on the supposition that the products formed have no influence on the rate at which the oxidation goes on; but when an experiment was continued for a comparatively long period, or till about 80 per cent. and upwards of the oxidation had taken place, a small and ever increasing difference was apparent between the calculated and observed variables, indicating a retardation which was due, without doubt, to the products of the reaction.

The present paper contains the results of a series of experiments made to investigate this retarding effect, in a general

* Communicated by the Author.

† Phil. Mag. [5] vi. p. 371: viii. p. 121.

way, by employing such salts as could not influence the oxidation of the iron by any process of double decomposition.

A solution (about 8 litres) of ferrous sulphate and hydric sulphate was prepared from pure materials and preserved in a large glass jar, arranged in such a way that any required quantity of the solution could be siphoned off, while an atmosphere of coal-gas was always maintained inside the jar. This solution contained in 100 cubic centim.

3·1416	grams ferrous iron,
·038	gram ferric iron,
11·84	grams free hydric sulphate.

The solution of potassic chlorate contained in 100 cubic centim. 1·933 gram. 10 cubic centim. being the amount required to oxidize the iron in 16·86 cubic centim. of the ferrous sulphate solution. The method of performing the experiments was the same as has been already described in my former papers.

Each experimental solution consisted of 16·86 cubic centim. of the iron solution or ·5296 gram Fe'', and 1·996 gram free H_2SO_4, together with the requisite amount of the salt whose retarding effect was to be determined, the total volume being made up to 250 cubic centim. with distilled water.

The salts employed were purified from all traces of iron and chlorides, dissolved in distilled water, and the strengths of the solutions determined by double estimations of base and acid.

The flasks containing the experimental solutions were placed in a water-bath, usually six at one time, 10 cubic centim. of the chlorate solution run into each, and the amount of oxidation that had taken place after the lapse of various intervals of time determined by permanganate. From two observations of the amount of unoxidized iron and time, the constants in the equation $y(a+t)=b$ were calculated, three minutes after the addition of the chlorate being taken in every case as the point $t=0$.

As b in the above equation is inversely proportional to the rate of change, or

$$\frac{dy}{dt} = -\frac{y^2}{b},$$

by a comparison of its values for a series of experiments, in which other conditions being the same except the amount or kind of salt added, with its value for a blank experiment, or that in which no retarding agent is present, a measure of the effect is obtained: these ratios multiplied by 100 are given in Table II.

To determine the equation for the blank as accurately as possible, twenty-four experiments were made, in four batches

of six each, at various times during the course of the work, which lasted about two weeks, the first batch at the commencement and the fourth at the finish. The mean values obtained were:—

	a.	*b.*	
	113·8	1113·8	temp. 21° C.,
	112·9	1112·5	
	112·4	1110·8	
	113·0	1115·7	
Means . .	113·02	1113·2	

or the equation for the blank experiment,

$$y(113{\cdot}02+t)=1113{\cdot}2.$$

The following Table contains the mean values for these same constants from two and, in some cases, four experiments, and the weight (calculated as anhydrous) of the various salts present in each:—

TABLE I. Temp. 21° C.

Weight of salt.	K_2SO_4.		Na_2SO_4.		$(NH_4)_2SO_4$.	
	a.	*b.*	*a.*	*b.*	*a.*	*b.*
2 grams...	134·5	1340	134·8	1343	134·3	1345
3 „ ...	145·5	1456	146·5	1464	147	1473·7
4 „ ...	159·6	1592·7	159·5	1595·5	161·5	1618·4
5 „ ...	170·5	1708	167·7	1691	171·2	1725
6 „ ...	185·3	1860·4	184·3	1853·7	184·9	1850
8 „ ...	214	2180·5	210·8	2120·3	212	2150·7
10 „ ...	243·1	2468·6	233·5	2365	235	2381·3

Weight of salt.	$KAl(SO_4)_2$.		$(NH_4)Al(SO_4)_2$.	
	a.	*b.*	*a.*	*b.*
2 grams...	126·6	1249·3	127·8	1244
3 „ ...	133·3	1315·4	133·4	1316·6
4 „ ...	140·6	1387·6	139·7	1384·5
5 „ ...	144·8	1442·5	145·5	1444·5
6 „ ...	154·7	1537·5	154	1529·4
8 „ ...	163·7	1627·8	160·9	1608·8
10 „ ...	173·4	1727·5	171·7	1704·5

Weight of salt.	$MgSO_4$.		$ZnSO_4$.	
	a.	*b.*	*a.*	*b.*
2 grams...	128·5	1278·5	123·8	1225·6
3 „ ...	139·1	1375·4	129·8	1280·3
4 „ ...	144	1429·5	131·5	1308
5 „ ...	149·5	1492·2	138	1369·5
6 „ ...	156	1569·4	141·6	1414·3
8 „ ...	168	1680·1	147	1477
10 „ ...	179·2	1793·5	155	1540·3

Taking the value of b, or 1113·2, for the blank experiment as equal to 100, and expressing the corresponding values of this quantity contained in the above Table in terms of it, the numbers contained in the following Table are obtained; they, in point of fact, represent the number of minutes required to oxidize the iron in each solution from y' to y'', the time required to perform the same amount of oxidation in the blank experiment being 100 minutes.

TABLE II.

Weight of salt.	K_2SO_4.		Na_2SO_4.		$(NH_4)_2SO_4$.	
	$\frac{100b'}{b}$.	Retardation for 1 gram.	$\frac{100b'}{b}$.	Retardation for 1 gram.	$\frac{100b'}{b}$.	Retardation for 1 gram.
2 grams ...	120·3	10·1	120·6	10·3	120·8	10·4
3 „ ...	130·8	10·2	131·5	10·5	132·4	10·8
4 „ ...	143·1	10·8	143·3	10·8	145·3	11·3
5 „ ...	153·4	10·7	151·9	10·4	154·9	10·9
6 „ ...	167·1	11·2	166·5	11·1	166·2	11·0
8 „ ...	195·8	11·9	190·5	11·3	193·2	11·6
10 „ ...	221·7	12·2	212·4	11·2	213·9	11·4

Weight of salt.	$KAl(SO_4)_2$.		$(NH_4)Al(SO_4)_2$.	
	$\frac{100b'}{b}$.	Retardation for 1 gram.	$\frac{100b'}{b}$.	Retardation for 1 gram.
2 grams ...	112·2	6·1	111·8	5·9
3 „ ...	118·1	6·0	118·3	6·1
4 „ ...	124·7	6·2	124·4	6·1
5 „ ...	129·6	5·9	129·7	5·9
6 „ ...	138·1	6·3	137·4	6·2
8 „ ...	146·2	5·8	144·5	5·6
10 „ ...	155·2	5·5	153·1	5·3

Weight of salt.	$MgSO_4$.		$ZnSO_4$.	
	$\frac{100b'}{b}$.	Retardation for 1 gram.	$\frac{100b'}{b}$.	Retardation for 1 gram.
2 grams ...	114·9	7·4	110·1	5·0
3 „ ...	123·6	7·8	115	5·0
4 „ ...	128·4	7·1	117·5	4·4
5 „ ...	134	6·8	123	4·6
6 „ ...	140·9	6·8	127	4·5
8 „ ...	150·9	6·4	132·7	4·1
10 „ ...	161·1	6·1	138·4	3·8

From the numbers contained in Table II., it will be seen that the retardation in the rate of oxidation of ferrous sulphate

by potassic chlorate is proportional to the weight of the chemically inactive salt present, and that the amount of this retardation is the same for equal weights of those salts which more or less resemble each other constitutionally. This is seen to hold for the sulphates of the alkaline metals and ammonium, and also for the two alums; but in the case of the sulphates of zinc and magnesium the difference in their effects is too considerable to permit of their being classed together, in a dynamical sense, however much they may resemble each other statically. The fact that equal weights of the alkaline sulphates or the alums produce equal effects, shows that the retardation produced by a single molecule of those salts which belong to the same group is proportional to its weight.

Mills and Walton have shown that, in the formation of ammonia by the action of zinc amalgam on potassic nitrate, the presence of sodic or potassic sulphate produces an acceleration in the reaction, and that equal weights of the two salts have, as nearly as possible, equal working effects.

My best thanks are due to Dr. Frankland, in whose laboratory the above experiments were made.

LIV. *Note on the supposed Helical Path of the Electric Current in longitudinally Magnetized Conductors.* *By* J. A. Ewing, *B.Sc., F.R.S.E., Professor of Mechanical Engineering in the University of Tokio, Japan*†.

IN a paper entitled "Molecular Magnetism," read before the Royal Society on May 19, Prof. Hughes describes experiments which have led him to the conclusion "that the path of an electric current through an iron or steel wire is that of a spiral." Prof. Hughes mentions that the wires experimented on were never entirely free from longitudinal magnetization to begin with, and that when they were further magnetized the effects observed, which formed the ground of the conclusion just quoted, were greatly increased. It may fairly be assumed that these effects were in all cases due to the presence of longitudinal magnetism in the wires.

The object of this note is to point out that the experimental results obtained by Prof. Hughes admit of another interpretation than that which he has given them, and at the same time to suggest a method of determining whether there is, or is not, any such helical quality in the path of the current as he has inferred.

* Proc. Roy. Soc. 1879.

† Communicated by the Author.

In Prof. Hughes's experiments, magnetized iron or steel wire was placed inside and along the axis of a coil which was in circuit with a telephone. The coil could be turned about an axis perpendicular to the wire. By means of the telephone it was observed that, even when the wire was normal to the plane of the coil, a series of currents were induced in the coil when an intermittent current was made to pass along the wire. By turning the plane of the coil round through a certain angle, silence was restored in the telephone. This result would, of course, be consistent with the hypothesis that the path of the current in the wire is not rectilinear but helical.

But it is clear that the same inductive effects would be given by changes in the longitudinal magnetization of the wire accompanying the changes in the transmitted current.

Weber's theory of magnetization, which assumes that the molecules of iron or steel are always magnets, and that magnetization consists in bringing their axes into more or less complete parallelism, implies that if magnetization exists in one direction, then any supplementary magnetization at right angles to that must produce a diminution of the first. The degree to which the second magnetization affects the first will depend on the nearness of the latter to its limiting value. If it approaches that closely, then the second magnetization will take place wholly at the expense of the first. Hence, when a current traverses a longitudinally magnetized wire, the circular magnetization caused by the current must reduce the original longitudinal magnetization. The first passage of the current will produce a relatively large effect; and subsequent interruptions and renewals of it will cause fluctuations in the value of the magnetic moment, whose external inductions will be precisely the same as if the path of the traversing current were a helix.

The experiments of Prof. Hughes agree with this deduction from Weber's theory. A current entering at the nominal N. pole gave an inductive effect corresponding to a right-handed spiral; entering at the S. pole, it gave an effect corresponding to a left-handed spiral. Hence the inductions were in both cases of the kind which would be produced by a diminution of the original magnetization of the wire.

A second experiment consisted in passing a steady current through the wire from N. to S., and superposing on it an intermittent current. The inductive effects were then such as would correspond to a right-handed spiral, both when the direction of the intermittent current was the same as that of the steady one and when their directions were opposite. From

the point of view now suggested we have, in the first case, a further diminution of longitudinal magnetization by the intermittent current, and in the second case a partial restoration of it; and these changes would account for the observed facts. An examination of Prof. Hughes's paper will show that his other experiments bearing on this point admit equally well of the same explanation.

On the other hand, we have a reason for anticipating that the path of the electric current in a magnetized wire will be found to possess some helical quality.

The circular magnetization due to the current, together with the existing longitudinal magnetization, gives a helical magnetization which will be right-handed (like a common screw) when the current enters at the S. pole. It is known that magnetization develops an æolotropic difference of electrical conductivity along and across the lines of force. If then the conductivity be greater across than along the direction of magnetization, the path of a current entering at the S. pole will be a left-handed helix. The inductive effects will in that case be of the same sign as those observed by Prof. Hughes.

Sir William Thomson (Phil. Trans. 1856, part 3) has concluded that in iron the effect of magnetism on conductivity is to give greater resistance along than across the lines of induction. This would give a helical quality of the kind supposed by Prof. Hughes; but the difference in conductivities is so small as to make it highly improbable that this action accounts for the very considerable inductions observed by him. In steel the effect is more doubtful (*cf.* Thomson, *loc. cit.*; Tomlinson, Proc. Roy. Soc. June 17, 1875; F. Auerbach, Phil. Mag. 1879, Nos. 46–48).

These considerations indicate that the induced currents in the coil are, probably, due in chief part to changes of longitudinal magnetization which accompany the passage and stoppage of the current in the wire; also that there may be some effect caused directly by the solenoidal component of helical currents, whose helical path is attributable to the development of a difference of conductivities by helical magnetization. Further, it is obvious that if (from this or any other cause) there is a solenoidal quality in the path of the current, the change of magnetic moment and the induced currents will be indirectly increased by the magnetizing action of this solenoid on those parts of the metal which may be considered to lie within it.

The method used by Prof. Hughes does not enable us to distinguish between the part of the action due to change of

longitudinal magnetization, and the part (if any) due to a solenoidal component of the current. The same remark applies with equal force to any observation of the induction *outside* the magnetized conductor.

But if we use a hollow iron or steel rod in place of a solid wire, and test the magnetic force inside it, we may at once determine whether any part of the observed effect is due to a circulation of the current round the rod. Let the rod be long compared with its transverse dimensions, and let it be uniformly magnetized. In the first instance, the magnetic force at the middle of its length, either inside or outside, due to its longitudinal magnetization, is

$$-\frac{\mathrm{M}}{a^3},$$

where M is its magnetic moment and a its half length. When a current is passed along the rod there will be (by Prof. Hughes's experiment) a diminution of its moment. Let this diminution of moment be $\Delta\mathrm{M}$. Then outside the rod, at the centre, the magnetic force will be changed by the amount

$$\frac{\Delta\mathrm{M}}{a^3}.$$

But inside, if any part of the effect is due to a solenoidal arrangement of the current, an electromagnetic force will be produced equal to

$$-4\pi\mathrm{C}n,$$

where C is the current, and n the number of turns in this solenoid per unit of length. The actual change of force at an internal point midway between the ends will therefore be

$$\frac{\Delta\mathrm{M}}{a^3}-4\pi\mathrm{C}n.$$

The question whether the second term of this expression has any sensible existence, is to be answered by observing the change of force inside when the current is made, and at the same time observing the change of moment of the rod, or by making simultaneous or successive measurements of the force outside and inside.

Various ways of carrying out this experiment will suggest themselves. The two following plans, which I hope soon to put to the test, appear practicable; the second, in particular, seems capable of a high degree of delicacy.

1. Suspend inside the hollow rod a magnet of known moment, and small enough to leave some clearance for swinging. Time the oscillations of this magnet both before and

during the passage of the current, and observe the loss of moment (ΔM) of the hollow rod. See whether the change of magnetic force deduced from the oscillations is or is not less than $\frac{\Delta M}{a^3}$.

II. Prepare two small coils of precisely equal inductive power. Place one inside, and the other outside, the hollow rod at the middle of its length. Connect them both to a ballistic galvanometer or a telephone in such a manner that, if the currents induced in them are equal, there will be no resultant effect. Then pass a current along the rod. If the effects are simply magnetic, the induction will be the same for both coils; but if the passage of the current produces any electromagnetic force in the interior, this equilibrium will be much disturbed.

This last has the merit of being a null method; and it has the additional advantage, that a strong longitudinal magnetization may be given to the rod by enclosing it in a long solenoid through which a steady current may be kept circulating during the experiment.

Tokio, Japan,
September 22, 1881.

LV. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* By J. J. Thomson.

To the Editors of the Philosophical Magazine and Journal.

Trinity College, Cambridge,
May 16, 1882.

Gentlemen,

In the last number of the Philosophical Magazine there is a paper by Prof. J. D. Everett, on the Dimensions of a Magnetic Pole in the Electrostatic System of Units, in which he supports the view taken by Clausius, that in this system the dimensions of a magnetic pole are $M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2}$, and not $M^{\frac{1}{2}} L^{\frac{1}{2}}$ as stated by Maxwell. Clausius gets his value from the equation

$$\text{Moment of magnet} = \text{current} \times \text{area}.$$

It seems to me, however, that the equation should be

$$\text{Moment of magnet} = \mu\,\text{current} \times \text{area};$$

where μ is the magnetic permeability of the medium in which the current is placed, and is of dimensions $T^2 L^{-2}$ in the electrostatic system: if we use this equation we get the same dimensions for the magnetic pole as Maxwell. The following are the reasons which seem to me to show that the value given by Maxwell is the correct one. It will be convenient to see how Maxwell's value is obtained. The easiest way of getting

it is to deduce it from the expression given by Maxwell for the magnetic force produced by an electric current. Maxwell, in his 'Electricity and Magnetism,' 2nd edit. §§ 498. 499. makes the following statements about the magnetic force due to a current:—

"If any closed curve be drawn and the line-integral of magnetic force taken completely round it, then if the closed curve be not linked with the circuit the line-integral is zero; but if it is linked with the circuit so that the current i flows through the closed curve, the line-integral is $4\pi i$. The line-integral $4\pi i$ depends solely on the quantity of current and not on any other thing whatever. It does not depend on the conductor through which the current is passing. Again, the line-integral $4\pi i$ does not depend on the medium in which the closed curve is drawn. It is the same whether the closed curve is drawn entirely through air, or passes through a magnet or soft iron, or any other substance whether paramagnetic or diamagnetic."

Hence, if F be the magnetic force due to an infinitely long straight current of strength i at a distance r from the current, it may without any ambiguity be written

$$F = \frac{2i}{r}.$$

Now the work done when a magnetic pole of strength m is carried round the current so as to remain at a constant distance r from it, is $m F 2\pi r$, or, by the above equation, $4\pi m i$. Hence the product mi is of the dimension of energy; but on the electrostatic system i is of the dimension $M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2}$: hence m must be of dimension $M^{\frac{1}{2}} L^{\frac{1}{2}}$, the value given by Maxwell.

The dimensions of F the magnetic force are $M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-2}$, the magnetic induction $m r^{-2}$ is of dimension $M^{\frac{1}{2}} L^{-\frac{3}{2}}$; hence the magnetic permeability μ, which is the ratio of magnetic induction to magnetic force, is of dimensions $L^{-2} T^2$.

If R be the magnetic force due to a pole of strength m at a distance r from it, the magnetic induction is μR. Now the magnetic induction integrated over any surface containing the pole and no other magnetic matter $= 4\pi m$. Hence, taking the induction across the surface of a sphere whose centre is the pole and radius r, we get

$$\mu . R . 4\pi r^2 = 4\pi m;$$

$$\therefore R = \frac{m}{\mu r^2}.$$

Hence the force between two poles m, m' is $mm' \mu r^{-2}$. Thus the force between two magnetic poles depends on the medium in

which they are placed; but, according to Maxwell, the magnetic force between a current and a magnetic pole does not. Hence the moment of the magnetic shell equivalent to a given current depends on the medium surrounding the shell. If the field be occupied by one medium only, the moment of the magnetic shell $=\mu$. current $\times$ area. This equation gives the dimensions $M^{\frac{1}{2}}L^{\frac{1}{2}}$ for m.

The effect of the medium surrounding a current upon the magnetic force produced by the current has, as far as I know, not been investigated. The only experiments I know bearing on this point are some experiments of Faraday on the effect of the surrounding medium on the electric currents induced by a moving magnet. The result of the experiments was, that he could not detect that changing the medium had any effect on the induced currents. If this be so, then Maxwell's statement must be right. For, imagine a long magnet in presence of an electric current, and let one pole be in one medium and the other in another; then, if the forces on the two poles are different in magnitude, we may carry the magnet round the current so as to get work out of it. Now this work must have come from the battery; but, since the inductive action of the magnet on the current is independent of the medium, there will be on the whole no currents induced by the motion of the magnet: hence no work can come from the battery; hence the forces on the poles must be equal, or the magnetic force produced by a current must be independent of the surrounding medium. I may mention that Mr. E. B. Sargant, of Trinity College, is at present experimentally investigating this point. Mr. W. D. Niven has pointed out to me that the value given by Clausius for the dimensions of a magnetic pole does not make the magnetic force between two such poles of the dimensions of a force, which ought clearly to be the case.

I am, Gentlemen,

Your obedient servant,

J. J. Thomson.

LVI. *On the Electrostatic Dimensions of a Magnetic Pole.* By Prof. J. Larmor*.

CLAUSIUS'S formula to connect the dimensions of current and pole on the static system, viz.

(A) Moment of Magnet = Current $\times$ Area,

seems really to involve the electromagnetic system of units. It is plain that, to be able to make and establish *practically*

* Communicated by the Author.

such a relation as to the magnetic action of a current, we must assume tacitly these previous relations (which in fact supply our only mode of comparison and verification), viz.:—

$$(1)\quad (\text{Pole})^2 \times \mathrm{L}^{-2} = \text{Force},$$

$$(2)\quad \text{Current} \times \mathrm{L} \times \text{Pole} \times \mathrm{L}^{-2} = \text{Force}.$$

The first of these is the purely magnetic starting-point of the electromagnetic system; and the second is the relation which must be used to connect current with pole *on any system*. But the electrostatic system naturally starts with the dimensions of electric quantity as deduced from the law of inverse squares, whence those of current are deduced at once. Now, the only simple fact of nature which connects pole with current is Oersted's phenomenon (as developed by Ampère) of the existence of a mutual force between them: this is expressed by (2); and hence by this relation must we deduce the dimensions of pole from those of current already found. The somewhat recondite fact that the magnetic action of a small circuit carrying a current may be represented as due to two *fictitious* magnetic poles, does not seem to possess any claims to supplant the natural statement of the only fundamental relation which makes *natural* poles play a part in electric theory at all. That relation is, of course, itself in its very nature electromagnetic.

It might be said that the system objected to built up the theory from electric foundations, inasmuch as its fundamental magnet is a small electric current. But, in reply, it is the existence of actual magnets which introduces the idea of pole at all, other than as in Ampère's purely mathematical directrix of electrodynamic action; and the above considerations seem to show that this result is only obtained at the cost of preferring the fulfilment of the practically composite relation (A) to that of the fundamental electromagnetic relation (2), in the specification of an electrostatic system. This, though allowable if consistently adhered to, does not seem to be natural—if indeed the specification of a magnetic pole on an electrostatic system is a matter of importance at all.

The method indicated gives Maxwell's dimensions, $\mathrm{L}^{\frac{1}{2}}\mathrm{M}^{\frac{1}{2}}$.

In practice, one would most probably investigate the dynamic relations of statical charges with a galvanometer, and not with an electrodynamometer.

Queen's College, Galway,
May 10, 1862.

LVII. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* (*Second Article.*) *By* Prof. J. D. Everett*.

MY appeal for an explicit statement of Maxwell's definition of the unit pole in the electrostatic system has brought me several communications from correspondents; and the diversity between them is a sufficient proof of the necessity for such an appeal. My correspondents do not refer me to any explicit definition by Maxwell himself, but give investigations which they regard as substantially his and which lead to his result. Two of these investigations seem quite satisfactory, and show that Maxwell's result can be obtained with as much simplicity as that of Clausius. They are given under the heads I., II., below.

Before entering on the points in dispute with respect to the electrostatic system, I may premise that the definitions of the unit quantity of electricity, the unit current, the unit electromotive force (or difference of potential), and the unit resistance (all of which may be called purely electrostatic definitions), are not in question, but are accepted by all parties. The divergence begins when we attempt to express magnetic quantities in an electrostatic system; and different results may be obtained according to the particular relation between magnetism and electricity which we select as the guiding principle in our definitions. In a strict sense there is no such thing as an electrostatic unit of any magnetic quantity; since magnetism and its relations to electricity lie outside the domain of electrostatics.

There are three laws of nature any one of which may be used to connect electrical with magnetic units.

I. The *galvanometer law*, as I may for brevity call it, because it is the law which determines the force which a current passing through the coil of a galvanometer exerts upon either pole of the needle. This law, stated without any assumption as to units, is that the force varies directly as the length of the wire, the strength of the current, and the strength of the pole, and inversely as the square of the distance of the wire from the pole. We must therefore, in every system, have

$$\text{Force} = k_1 \times \text{Current} \times \text{Pole} \times \text{Length} \div (\text{Distance})^2, \quad (1)$$

k_1 being a factor which depends on the system employed. Maxwell's unit pole may be defined by making $k_1 = 1$. This gives, for determining the dimensions of a pole,

$$\mathrm{MLT^{-2}} = \text{Current} \times \text{Pole} \times \mathrm{L^{-1}}.$$

* Communicated by the Author.

But

$$\text{Current} = M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2};$$

therefore

$$\text{Pole} = M^{\frac{1}{2}} L^{\frac{1}{2}},$$

which is Maxwell's result. This proof was supplied to me by Professor Larmor. Clausius alludes to such a proof as having been given by Maxwell, and objects to it on the ground that the force which a current exerts upon a pole is not an electrostatical but an electrodynamical force. But, inasmuch as this objection applies equally to all definitions of "the unit pole in the electrostatic system," not excepting that offered by Clausius himself, it cannot be admitted as valid when we are discussing the merits of one as against another.

Again, we may employ

II. The *magneto-electric law*, which determines the electromotive force produced by moving a conductor in a magnetic field. This law, when stated without any assumption as to units, is that the electromotive force is directly as the length of the conductor, the velocity of its motion resolved in a certain direction, and the intensity of the field. Hence, bearing in mind that the intensity of the field due to a single pole is directly as the strength of the pole and inversely as the square of the distance, we must have, in every system,

$$\left.\begin{aligned}\text{Electromotive force . . .}\Big\} &= k_2 \times \text{Length} \times \text{Velocity} \times \text{Pole} \div (\text{Distance})^2 \\ &= k_2 \times \mathrm{L} \times \mathrm{LT}^{-1} \times \text{Pole} \times \mathrm{L}^{-2} \\ &= \text{Pole} \times k_2 \mathrm{T}^{-1}.\end{aligned}\right. \quad (2)$$

If we define our unit pole by the condition $k_2 = 1$, we have

$$\begin{aligned}\text{Pole} &= \text{Electromotive force} \times \mathrm{T} \\ &= \mathrm{M}^{\frac{1}{2}} \mathrm{L}^{\frac{1}{2}} \mathrm{T}^{-1} \times \mathrm{T} \\ &= \mathrm{M}^{\frac{1}{2}} \mathrm{L}^{\frac{1}{2}}.\end{aligned}$$

This mode of obtaining Maxwell's result was in substance supplied to me by Professor Fitzgerald. So far we have no discrepancy.

On the other hand, we may employ with Clausius,

III. The *law of the magnetic shell*, which asserts the equivalence of a current to a magnet. Taking the simplest case—that of a current in a plane circuit—the law of nature is that the moment of the equivalent magnet is jointly proportional to the strength of the current and the area of the circuit. Hence, since the moment of the magnet is the product of the

strength of either pole by the distance between the poles, we must have, in every system,

$$\text{Pole} \times \text{Length} = k_3 \times \text{Current} \times \text{Area}; \quad . \quad . \quad (3)$$

that is,

$$\text{Pole} = k_3 \times \text{Current} \times \text{L},$$

where k_3 depends on the system employed. Clausius defines his unit pole by making $k_3 = 1$, and thus obtains

$$\begin{aligned} \text{Pole} &= \text{Current} \times \text{L} \\ &= \text{M}^{\frac{1}{2}}\text{L}^{\frac{3}{2}}\text{T}^{-2} \times \text{L} \\ &= \text{M}^{\frac{1}{2}}\text{L}^{\frac{5}{2}}\text{T}^{-2}. \end{aligned}$$

This result disagrees with that of the two preceding investigations; and hence the two equivalent assumptions $k_1 = 1$, $k_2 = 1$ are inconsistent with the assumption $k_3 = 1$. Maxwell has chosen the former alternative. Clausius the latter. On Maxwell's system we have

$$k_1 = 1, \quad k_2 = 1, \quad k_3 = \text{T}^2\text{L}^{-2}.$$

On Clausius's system we have

$$k_1 = \text{T}^2\text{L}^{-2}, \quad k_2 = \text{T}^2\text{L}^{-2}, \quad k_3 = 1.$$

In fact it can be shown that, if v be the ratio of the electromagnetic to the electrostatic unit of quantity of electricity, k_3 would be $\frac{1}{v^2}$ on Maxwell's system, and k_1 and k_2 would each be $\frac{1}{v^2}$ on Clausius's system. When we bear in mind how frequently laws I. and II. are applied in practical calculation, and how extremely rare is any practical application of law III., it seems clear that, if we were driven to employ an electrostatic system in calculations relating to magnetism, the best choice we could make would be Maxwell's.

It is further clear that electrostatic systems are essentially inconvenient for calculations relating to electromagnetism. The electromagnetic system makes k_1, k_2, and k_3 each unity, and also gives the value unity to the factor k_4 which occurs in the general expressions for the attractions and repulsions between currents. For example, the force with which two parallel currents, one of very great length and the other of length l, attract or repel each other, is given in any system by the formula

$$\text{Force} = k_4 \times \text{Product of currents} \times 2l \div \text{Distance}, \quad . \quad (4)$$

where k_4 has the value unity in the electromagnetic system, and the value $\frac{1}{v^2}$ in the electrostatic system, there being here no difference between Maxwell and Clausius.

The mutual force of two magnetic poles is

$$k_5 \times \text{Product of poles} \div (\text{Distance})^2, \quad . \quad . \quad (5)$$

where k_5 is v^2 in the electrostatic system of Maxwell, and $\frac{1}{v^2}$ in that of Clausius, but is defined as unity in the electromagnetic system.

On the other hand, the mutual force of two charges of electricity is

$$k_6 \times \text{Product of charges} \div (\text{Distance})^2, \quad . \quad . \quad (6)$$

where k_6 is defined as unity in the electrostatic system, but has the value v^2 in the electromagnetic system.

A comparison of the foregoing six equations shows how cautious we ought to be in asserting that a particular dimensional relation "must hold in every system of units." The laws of nature which connect dissimilar quantities are laws of *proportion*, and it is only by convention that they can be stated as laws of *equality*. Clausius was in error in the assertion, which I adopted from him in my last communication, that in every system the product of the current by the area enclosed must be *equal* to the moment of the equivalent magnet. *Proportional* would be the correct word; and the factor k_3, which remains constant as the current and area vary, does not necessarily retain the same value when we pass from one set of units of length and time to another. Maxwell says (§ 482):—"It has been shown by numerous experiments . . . . . that the magnetic action of a small plane circuit . . . . . is the same as that of a magnet . . . . whose magnetic moment is equal to the area of the circuit multiplied by the strength of the current;" but the passage occurs in a discussion in which the "Electromagnetic system of measurement" is employed and defined (see § 479). I do not think that the charge of mistake brought against Maxwell has been substantiated. The controversy, however, has done good in exposing the difficulties and dangers which lurk in applications of electrostatical units to magnetism.

LVIII. *Action of Free Molecules on Radiant Heat, and its Conversion thereby into Sound.* *By* JOHN TYNDALL, *F.R.S.**

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§ 1. *Introduction.*

THE experimental researches of Rumford and Leslie raised the subject of Radiant Heat to an extraordinary pitch of interest and importance. Both of these philosophers occupied themselves with what may be called superficial emission and absorption.

Melloni is to be regarded as the founder of our knowledge of the transmission of radiant heat through solids and liquids. Save in a passing inference, to be noticed immediately, Melloni left untouched the gaseous form of matter—thinking, probably, that gases and vapours, though their diathermancy could hardly be supposed theoretically perfect, came in this respect so near perfection as to be placed beyond the grasp of laboratory experiment. It was doubtless the general prevalence of this conviction which caused this field of inquiry to lie fallow for so many years after the discovery of the thermo-electric pile.

By an experimental arrangement characteristic of the genius of the man, though, it may be, not quite equal to the requirements of the problem, Melloni proved that the law of inverse squares held good for radiant heat *in air*; and from this he inferred the absence of all sensible absorption, by air, within the distance embraced by his experiments †. Melloni extended to radiation his conclusion regarding absorption. "On ne connaît," he writes, "aucun fait qui démontre directement le pouvoir émissif des fluides élastiques purs et transparents" ‡. Such was Melloni's relation to the subject now before us.

* Communicated by the Author, having been read before the Royal Society, January 23, 1882. The substance of this paper was delivered orally as the Bakerian Lecture on November 24, 1881.

† "Pour un intervalle de cinq à six mètres, l'air n'exerce aucune absorption sensible pour le rayonnement des corps chauds."—*La Thermochrose*, p. 136.

‡ *Annales de Chimie et de Physique*, vol. xxii. p. 494.

In 1855 Dr. Franz, of Berlin, published a paper "On the Diathermancy of certain Gases and Coloured Liquids"*. He found that air contained in tubes 452 and 900 millimetres long, absorbed 3·54 of the radiation from an Argand lamp; and he concluded that all transparent gases would behave like air. I have given reasons for holding that Dr. Franz in these experiments did not touch the question in hand †. In the arrangement which he describes, the absorption by air was quite insensible. But 60 per cent. of the radiation from his powerful source was lodged in the glass ends of his tubes; these, as secondary sources, radiated directly and indirectly against his pile; and it was their chilling by the cold air that slightly lowered his deflection and produced the supposed absorption.

It is not improbable that other attempts were made to bring gaseous matter under the dominion of experiment; but none to my knowledge are recorded.

§ 2. *Partial Summary of previous Work.*

My researches on magne-crystallic action carried with them the incessant use of conceptions and reasonings touching molecular constitution and arrangement. At an early period of these studies it occurred to me that heat, both in its radiant and in its ordinary thermometric form, might be turned to good account as an explorer of molecular condition. The first fruit of this idea was a paper "On Molecular Influences" ‡, in which it was shown that wood possesses three axes of calorific conduction coincident with the axes of elasticity discovered by Savart. Experiments on certain crystals recorded in this paper suggested a possible connexion between diathermancy and conductivity; and in 1853 I worked at this question. The substances then submitted to experiment were rock-crystal, amethyst, topaz, beryl, rock-salt, smoky quartz, fluor spar, tourmaline, Iceland spar, dichroite, arragonite, heavy spar, flint, and glass of various kinds. These minerals were employed in the shape of cubes carefully cut and polished, the transmission through each of them, in different directions, both of radiant and conducted heat being determined.

A desire for completeness, not then attained, caused me to postpone, and finally to forego the publication of the results

* Poggendorff's *Annalen*, vol. xciv. p. 337.
† Philosophical Transactions, 1861, vol. cli. p. 27, and elsewhere.
‡ Ibid. 1853, vol. cxliii. p. 217.

of this inquiry. It, however, kept alive reflections on the influence of molecular constitution on the phenomena of radiation and absorption. Encountered continually by the thought that in liquids and solids the pure molecular action was, or might be, hampered by cohesion, the desire to bring, if possible, free molecules under the dominion of experiment beset me more and more.

At the beginning of 1859 I definitely attacked this problem, meeting at the outset difficulties and negations the reverse of encouraging. But after some weeks of labour, I found myself in secure possession of the result, that gases and vapours exhibited, in relation to radiant heat, phenomena far more surprising than those observed by Melloni in liquids and solids. On the 26th of May, 1859, the subject was brought before the Royal Society *; and on the 10th of June I was able, by illuminating the dial of a galvanometer and casting its image upon a screen, to demonstrate in the Royal Institution not only the fact of absorption, but the astonishing differences of absorption which gases and vapours equally transparent to light manifested in regard to radiant heat †.

The following gases and vapours were then examined:— Air, oxygen, hydrogen, nitrogen, carbonic oxide, carbonic acid, nitrous oxide, coal-gas, ammonia, olefiant gas, bisulphide of carbon, chloroform, benzol, iodide of ethyl, cyanide of ethyl, formate of ethyl, acetate of ethyl, propionate of ethyl, iodide of amyl, chloride of amyl, amylene, absolute alcohol, amylic alcohol, methylic alcohol, ethylic ether, ethylamylic ether, sulphuric ether, and some others. In the Philosophical Magazine for 1862 I have given samples of the results obtained with a few of these substances; and I will here confine myself to the remark that were the measurements there recorded multiplied a hundredfold, they would fall far short of the number actually executed in 1859.

With the view of compelling the feeblest gases and vapours to show, if they possessed it, their capacity to absorb radiant heat, the "method of compensation" was invented ‡. Without prejudice to the delicacy of the galvanometer, this method enabled me to bring into play quantities of heat far greater than those ever previously invoked, my object being so to exalt the total radiation that a minute fraction of that total should reveal itself to experiment. By this method not only were the feebler gases and vapours coerced, but the vastness of the diathermic range, if I may use the phrase, was esta-

* Proceedings of Royal Society, vol. x. p. 37.
† Proceedings of Royal Institution, vol. iii. p. 155.
‡ Philosophical Transactions, 1861, vol. cli. pp. 6 & 7.

blished with a clearness and an evidence unattainable by any other means then existent *.

Notices of the investigation having appeared in many English and continental journals †, I was induced to defer the detailed publication of the experiments. The investigation itself had taught me the difficulties and dangers which beset it. These had reference both to the methods of experiment and to the purity of the substances employed. To secure the perfect constancy of the sources of heat, and the perfect steadiness of the galvanometer, when the flux of heat was powerful, involved a lengthened discipline. With neither gases nor vapours, moreover, was it easy to obtain uniform results. When generated in different ways, the action of the same gas would sometimes prove itself so discordant as to suggest to me the possible existence of novel allotropic conditions to account for such variations of behaviour. Two samples, moreover, of nominally the same liquid, would furnish vapours yielding results far too divergent to be tolerated. The drying apparatus also contributed its quota of disturbance. These anomalies were finally traced to the fact that an incredibly small amount of impurity, derived from the stronger gases or vapours, sufficed to disguise and falsify the action of the weaker ones. All this had to be learnt; and when learnt, I thought it desirable, for the sake of accuracy, not to publish the results which had been gained with so much labour, but to go once more, with improved appliances, over the same ground. This I did, though it involved the total abandonment of seven weeks' uninterrupted experimental work in 1859, of seven weeks' similar work in 1860, and of many fragmentary efforts. On the 10th of January, 1861, the memoir containing an account of the investigation was handed in to the Royal Society ‡.

* With moderate total heats the method of compensation is extremely easy of application; but when the total radiation is very large, some discipline is required to keep the galvanometer-needle steady in its most sensitive position. With due training, however, perfect mastery over this difficulty may be obtained.

† Proceedings of the Royal Society, May 26, 1859; Proceedings of the Royal Institution, June 10, 1859; *Bibliothèque Universelle*, July 1859; *Cosmos*, vol. xv. p. 321; *Nuovo Cimento*, vol. x. p. 196; *Comptes Rendus*, 1859; and in other journals.

‡ Section 3 of the Bakerian Lecture for 1861 reveals some of the difficulties which beset the earlier stages of these inquiries. To secure strength of radiation and steadiness of the needle I passed from source to source, obtaining my temperatures in turn from water, oil, fusible metal, sheets of copper heated by regulated flames, and from other things. Approximate results were readily obtainable; but I aimed at a degree of accuracy which would render any material retractation afterwards unnecessary. Soundness of work I thought preferable to rapidity of publication.

The first point of importance established in 1859, and developed in the memoir just mentioned, was that already referred to, namely the fact of absorption and large differences of absorption. The second point—destined, I think, to throw light on the deeper problems of molecular physics, was the proof, that while elementary gases offered a scarcely sensible impediment to radiant heat, equally transparent compound gases exhibited, in many cases, an energy of absorption comparable to that of the most athermanous solids and liquids. Determining, for example, the action of a mechanical mixture of two elementary gases, it was proved that without altering either the quantity of matter, or its perfect transparency to light, the absorption of invisible heat might be increased many hundredfold by the passage of the constituents of the mixture into a state of chemical combination.

A similar deportment may be detected in liquids and solids. The quantity of iodine vapour generated at ordinary temperatures is so small that its action on radiant heat is, as might be expected, insensible. But iodine itself, when liquefied by a powerful solvent, behaves as an almost perfectly transparent body to the obscure calorific rays, even when it is able to extinguish totally the light of the sun. Liquid bromine is also highly diathermanous. The same may be said of phosphorus. In Melloni's table Sicilian sulphur comes next to rock-salt in transmissive power. A concentrated solution of sulphur in bisulphide of carbon exerts no sensible action on radiant heat. By fusing together iodine and sulphur Professor Dewar has produced a "ray-filter" which separates with extreme sharpness the visible from the invisible rays. The remarkable diathermancy of certain specimens of vulcanite, brought to light in the experiments of Mr. Graham Bell and Mr. Preece, is probably due to the sulphur they contain. Melloni showed that lampblack is to some extent diathermanous. But when a suitable source of heat is chosen, lampblack proves far more pervious to radiant heat than Melloni found it to be. An opaque layer of this substance transmits 41 per cent. of the radiation from a hydrogen-flame. Were the lampblack optically continuous, the transmission would, doubtless, be still greater. An opaque solution of iodine transmits 99 per cent. of the radiation from the same source; while a layer of pure water, 0·07 of an inch in thickness, transmits only 2 per cent. of the radiation from a hydrogen-flame. Such results indicate that a profound change in the relation of ponderable matter to the luminiferous ether accompanies the act of chemical combination.

One of my principal aims in the Bakerian Lecture of 1861

was to illustrate the hold which experiment had obtained of a subject previously considered intractable. The densities of the gases and vapours employed were therefore varied within wide limits. In the experimental tube first made use of, a full atmosphere of olefiant gas absorbed more than 80 per cent. of the entire radiation; and it was therefore evident that a small fraction of an atmosphere of such gas would exert a measurable action. On trial, it was found possible to measure the absorption of $\frac{1}{10,000}$ of an atmosphere of olefiant gas. The action of this gas was determined at sixteen different densities, the absorption, as long as the density was very small, being accurately proportional to the quantity of gas present. Similar experiments were made, and similar results obtained with other gases. The action of sulphuric ether vapour upon radiant heat was proved to be still more powerful than that of olefiant gas. The vapour was first carried into the experimental tube by a current of dry air: and the pure vapour was afterwards examined at seventeen different densities. Bisulphide of carbon was tested at twenty different densities, amylene at ten, benzol at twenty, and so of the others.

Considering the views previously entertained regarding the diathermancy of gases and vapours, I was naturally impressed with these results. Sceptical when I first observed them, I scrutinized them closely, until repeated scrutiny abolished every doubt. For my own instruction I illustrated the action of the stronger gases and vapours in a variety of ways. Turning, for example, once rapidly round a cock connecting the exhausted experimental tube with a holder containing a powerful gas, the needle would fly aside, owing to the stoppage of the heat by the infinitesimal amount of gas which entered the tube during the rotation. Discharging a powerful gas or vapour in free air, between the source of heat and the thermopile, a similar energetic action would be produced by the perfectly invisible agent.

I was schooled in such actions before the thought of testing the omnipresent vapour of our atmosphere occurred to me. When it did occur, there was in my mind no *à priori* ground for supposing that its action would prove insensible: for why should I assume that $\frac{1}{100}$ of an atmosphere of aqueous vapour would prove neutral, after I had proved a small fraction of this fraction, on the part of other gases and vapours, to be active? There was no reason for such an assumption on my part—nothing to deter me from hopefully submitting the question to experiment. I accordingly tested the water vapour of the atmosphere in which I worked, and found its

action on a first trial to be thirteen times that of the air in which it was diffused.

It is not uninstructive to compare this approach to the problem with that of a very distinguished man—the late Professor Magnus, of Berlin*. Subsequent to me, he subjected the aqueous vapour of our atmosphere to an experimental test; but he made the experiment under the assured conviction that his result would be negative. "It could," he says, "be foreseen with certainty that the small amount of aqueous vapour taken up by air at ordinary temperatures could exert no influence on the transmission." I think it must be obvious that if Magnus had gone through the discipline to which I had been subjected, he would not have used this language. His mistake, however, was a natural one. In fact during the earlier stages of the inquiry my mind was exactly in the condition of his mind—I also thinking, until practically instructed to the contrary, that the action of aqueous vapour at ordinary temperatures must be immeasurably small. It is well known that Magnus tested his foregone conclusion, and found it verified; while I, on the other hand, as above stated, justified mine.

The various gases which had been examined in the experimental tube with regard to their powers of absorption, were next tested as regards their powers of radiation. Columns of the heated gases were allowed to ascend in free air, and to radiate against the pile. In this simple way the radiative power of "transparent elastic fluids" was for the first time established. The order in which the gases ranged themselves, in regard to radiation, was exactly the order of their absorptions. Here, as in other cases, I instructed myself by observing how gases might be made to play the part of solids. Permitting, for example, a film of one of the stronger gases to glide over a heated surface of polished silver, the radiation from that surface was augmented as it might have been by a coating of isinglass or lampblack.

A surmise has been mentioned regarding new allotropic conditions, as occurring to me amid the perplexities of my earlier experiments. In one instance, that of electrolytic oxygen, the surmise proved correct. My first experiments indicated that the modicum of ozone which went forward with the oxygen exerted four times the absorption of the gas in which it was diffused†. Subsequently, by changing the apparatus, and taking pains to augment the quantity of ozone,

* In many respects my generous and helpful friend; but, in regard to this question, my steadfast antagonist for many years.

† Philosophical Transactions, vol. cli. p. 8.

the multiple rose successively from 4 to 20, 35, 47, 85, ascending finally to 136 *. The behaviour of ozone was thus proved to be similar to that of molecules composed of heterogeneous atoms. Hence the conclusion, drawn at the time, that the molecule of ozone was formed of oxygen atoms so grouped as to render their action upon radiant heat virtually that of a compound body. This, it is needless to say, is the constitution now assigned to ozone.

With the view of including corrosive gases and vapours among the number of those examined, and for other reasons, the brass experimental tube was displaced by a tube of glass of the same diameter and nearly 3 feet long. The source of heat was also changed from a Leslie's cube containing boiling water, to a plate of copper against which a sheet of flame was permitted to play. Extraordinary precautions were found necessary to ensure perfect steadiness on the part of the flame. With this arrangement the practical inability of the elementary gases to absorb radiant heat was further illustrated and confirmed. Chlorine gas and bromine vapour, for example, were proved to be highly diathermanous.

At the pressure of an atmosphere it was found that the diathermic range of colourless gases extended from 1 to about 1000. The portion of gas first entering the experimental tube, having the whole heat to act upon, produced, as might be expected, the greatest effect, the increment of absorption (after a certain quantity of gas or vapour had entered) being infinitesimal †. It was therefore interesting to compare together the various gases at very small pressures. When the pressure was that of 1 inch of mercury the diathermic range was greatly increased, the absorption by olefiant gas being then at least 6000 times the absorption by atmospheric air.

With the changed apparatus the action of the aqueous

* Philosophical Transactions, vol. clii. pp. 84, 85.

† This is well illustrated by an experiment on sulphuric ether vapour recorded in the Bakerian Lecture for 1861:—

Pressure.	Absorption.
1 inch.	214
2 inches.	282
3 „	315
4 „	330
5 „	330

The absorption of air being taken as unity, that of sulphuric ether vapour at 1 inch mercury pressure is here shown to be 214. When, however, vapour corresponding to a pressure of 4 inches was already in the experimental tube, the addition of another inch did not sensibly augment the absorption.

vapour of our atmosphere was again taken up, and proved to be, not 13 times, as I had at first supposed, but, on fairly humid days, at least 60 times that of the air in which it was diffused. When carefully dried air was caused to pass over moistened glass, and then carried into the experimental tube, the absorption was still greater.

A power has been claimed for mist or haze which has been denied to aqueous vapour; but in these experiments concentrated luminous beams, which would have infallibly brought into view the least trace of suspended matter, revealed no mist or dimness of any kind. It is, moreover, demonstrable that an amount of turbidity, rendered strikingly evident by a luminous beam, exerts only a fractional part of the action of the pure aqueous vapour. When well-dried air was led, not through water or over wet glass, but over bibulous paper, taken apparently dry from the drawers of the laboratory, the amount of vapour carried forward from the pores of the paper produced 72 times the absorption of the air which carried it. After five repetitions of the experiment, wherein the same air was carried over the same paper, a quantity of vapour was still sent forward capable of exerting 47 times the absorption of the air in which it was diffused.

Here the possible action of odours upon radiant heat naturally suggested itself. Many perfumes were accordingly subjected to examination, the odorous substance being in each case carried into the experimental tube by a current of dry air. Thus tested, pachouli exerted 30 times, cassio 109 times, while aniseed exerted 372 times the absorption exerted by the air in which it was diffused.

A novel method of exhibiting the absorption and radiation of gaseous bodies, the germ of which had been previously discovered *, was illustrated and developed by the new apparatus. Suppose the experimental tube exhausted, and the needle, under the joint action of the two sources, to be at 0°. On admitting a strong vapour the usual deflection would occur. Suppose it to be 50 galvanometric degrees. Let dry air be now introduced until the experimental tube is filled. Although fresh matter is thus thrown athwart the rays of heat, the needle behaves as if the matter within the experimental tube had wholly disappeared. It sinks to zero, and not only so, but passes, say to 50°, on the other side.

After the first moments of perplexity succeeding the observation of this effect, its cause became clear. On entering the experimental tube the air, having its *vis viva* destroyed, was

* Philosophical Transactions, vol. cli. p. 32.

heated dynamically. Incompetent to radiate itself, it imparted its warmth to the vapour, and this powerful radiator poured the heat thus received against the pile. This heat sufficed not only to neutralize the deflection of 50° due to absorption, and indicating cold, but to carry the needle up to 50° on the side of heat. So likewise, when the experimental tube was filled with mixed air and vapour, the needle being at 0°, a stroke of the pump, though opening a freer passage for the rays from the source, caused a deflection indicative not of heat, but of cold. Here, the vapour within the tube, being chilled by the dilatation of the air, the pile radiated its uncompensated warmth into the vapour and produced the observed deflection.

Such observations suggested a new means of demonstrating the absorption and radiation of heat by gases and vapours. Abandoning all external sources of heat, and permitting the various gases already examined to enter the experimental tube at a common velocity, they became self-heated and radiated against the pile. Their radiation, thus determined, corresponded exactly with the results obtained when heated columns of these gases were permitted to rise freely in the atmosphere.

Both the radiation and absorption of vapours were determined in the same manner. The external source of heat was abandoned, and a measured quantity of every vapour was introduced into the experimental tube. Through an orifice of fixed dimensions dry air was then permitted to enter the tube, where the destruction of its *vis viva* raised its temperature. The heated air warmed the vapour, which in its turn radiated the heat imparted to it against the pile. The deflection of the galvanometer declared the strength of this radiation. Absorption was determined by permitting the mixed air and vapour to dilate by a measured quantity, the pile being here the warm body, and the chilled vapour the absorbent. The order in which the vapours stood as regards absorption was here exactly the order of their radiation; while both absorption and radiation, thus determined, agreed with the results obtained by sending the rays from an external source of heat through the pure vapours in the experimental tube.

What has been called "vapour-hesion," whereby liquid films are produced, has been supposed to play a dominant part in my experiments. But it can hardly be imagined that an irregular action of this kind could produce results of such precision and consistency as those here recorded. Such results are, in my opinion, only compatible with the con-

clusion that the veritable radiators and absorbers are the molecules of the vapours. Apart from all experiment, the notion that vapours must act in this way comes commended to us by the proved, and conceded, deportment of gases. It would be unreasonable to admit that a compound gaseous molecule is active, and at the same time to affirm that a compound vaporous molecule is inert.

This hypothesis of liquid films formed on the interior surface of the experimental tube, and on the plates of rock-salt, becomes, I think, more embarrassed as we proceed. It depends on the unproved assumption that liquids possess powers of absorption which are denied to their vapours. To water and brine, for instance, Magnus largely concedes such powers, but not to aqueous vapour. That the state of aggregation exerts an influence is not denied; but that it is here the dominant factor is open to doubt. To admit this would be to concede that the seat of absorption is the molecule as a whole, to the practical exclusion of the constituent atoms of the molecule. For if the atoms exert any influence, the mere passage from the liquid to the vaporous condition, which separates the molecules from each other, but leaves them individually intact, cannot destroy their powers of absorption.

At an early stage of these researches the parallelism of liquid and vaporous absorption forced itself upon my attention. Thus my experiments on bisulphide-of-carbon vapour were connected with the deportment of liquid bisulphide, as set forth in Melloni's table. The vapours, moreover, of chloride of sulphur and chloride of phosphorus, whose liquids stand in Melloni's table next to bisulphide of carbon, were afterwards proved by me to possess a diathermancy corresponding to that of their liquids. After various references to this subject in preceding memoirs, a portion of the Bakerian Lecture for 1864 was devoted to its examination. Liquid layers enclosed between plates of transparent rock-salt were tested in regard to their diathermancy; and, for the sake of control and verification, they were employed in five different thicknesses. The vapours of these liquids were examined in quantities proportional to the quantity of liquid, the same quality of heat being applied both to liquids and vapours. By these experiments it seemed to me placed beyond a doubt that the liberation of the molecule from the liquid condition did not destroy its absorbent power, the order of absorption being proved to be precisely the same for liquids and their vapours. Ten different substances were shown at the time here referred to to follow this rule. The list has since been extended; and I am not acquainted with a single real exception to the rule.

Any reasoning, therefore, which ascribes a potent absorption to perfectly impalpable films, condensed on the surface of my apparatus, and which denies that absorption to the free molecules within the experimental tube, is in my opinion untenable.

The relation between liquids and their vapours here indicated is very thorough. It extends beyond the field of experiment which we have hitherto had in view. I have, for example, published some researches on the action of rays of high refrangibility on gaseous matter, and have shown, in a great variety of cases, that the molecules are shaken asunder by such rays. The actinic clouds, as I have called them, produced by this decomposition reveal vividly the track of the beam by which they are generated, and render it easy to observe the distance to which the action penetrates. In the case of nitrite of amyl, for example, the power of decomposition is soon exhausted, the actinic cloud ceasing abruptly at a point about 18 inches from the place where the beam enters the vapour. An experimental tube 3 feet long has therefore one half of its vapour shielded by the other half; and on reversing the tube, the shielded half comes instantly down as an actinic cloud. In the case of iodide-of-allyl vapour, on the other hand, the beam may pass through a charged experimental tube 5 feet long, fill it with an actinic cloud, and still effect decomposition in another tube placed beyond it. What is true of these vapours is true equally of their liquids; for while a layer of the liquid nitrite $\frac{1}{8}$ of an inch thick prevents, when placed in the track of the beam, the decomposition of its vapour, a layer of the liquid iodide, of quadruple thickness, does not arrest the decomposition. The power, and the lack of power, to be penetrated to considerable depths is shared alike by the liquids and their vapours. Other, and still more subtle and penetrating illustrations of parallelism between liquid and vaporous absorption are mentioned in the Bakerian Lecture for 1864*

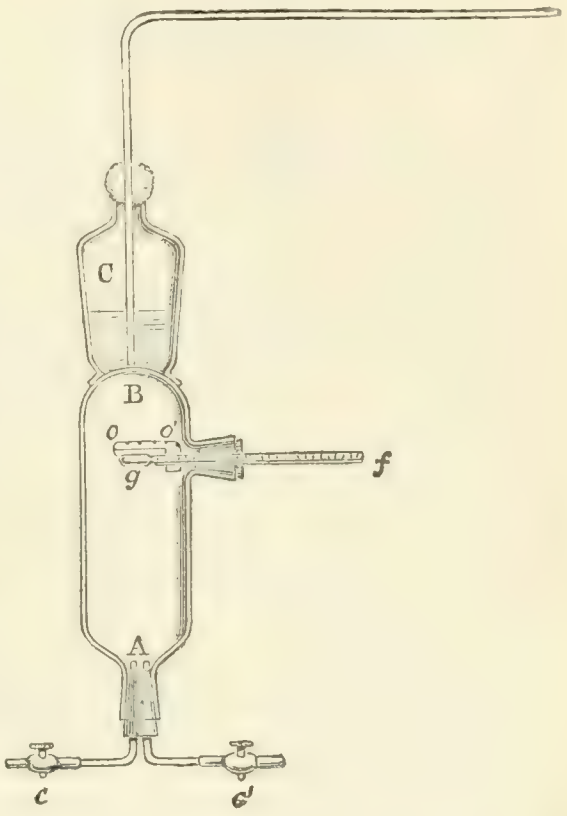
§ 3. *Researches of* Magnus.

Prompted by the experiment of Grove, illustrating the chilling action of hydrogen, Magnus, in 1860, began an in-

* Carbonic acid is one of the feeblest of the compound gases, as regards the radiation from solid bodies; but for the radiation from a carbonic oxide flame it transcends all other gases in absorbent power. The action of aqueous vapour is also enhanced when it acts upon the rays emitted by a hydrogen-flame. The enhancement extends to water. Curious reversals of diathermic position, when heat from different sources is employed, are moreover shown to occur simultaneously with liquids and vapours.

vestigation on the power of gases to *conduct* heat*. His apparatus, traced in outline from his own plate, is shown in fig. 1, where A B is the recipient for the gases, and C a flask containing water kept boiling by a current of steam. The bottom of C, which formed the top of A B, was the source of heat. A thermometer, *g f*, shielded by a cork or metal screen *o o'* from the radiation of the source, was intended to receive and measure the heat transmitted by conduction. The recipient A B was mounted in a space surrounded by water of a constant temperature. The heating of the thermometer when A B was exhausted †, was compared with its heating when A B was filled with various gases; and in every case but one the heating through the gas was found less than the heating through the vacuum. The exception was hydrogen, which carried more heat to the thermometer than was transmitted by the vacuum. The conclusion drawn by its author from this experiment was that hydrogen conducted heat like a metal.

Fig. 1.



One remark only in this Note has any reference to the diathermancy of gases; but it is a significant one. Magnus had no doubt as to the power of every one of his gases to conduct heat. There could, he supposed, be only a difference of degree between them and hydrogen. Whence, then, the

* A Preliminary Note of this inquiry is published in the *Bericht* of the Berlin Academy for July 30, 1860. No measurements are given; but certain results are announced. The experiments were first published in Poggendorff's *Annalen* for April 1861.

† This vacuum-temperature, at least in so far as it exceeded that of the sides of the recipient, was obviously derived from the screen.

lowering of the thermometer? He answers thus:—"From this it is not to be inferred that the gases do not conduct heat, but merely that in their case conduction is so feeble as to be neutralized by adiathermancy." These are the only words in the note which have any reference to radiation.

In his next inquiry Magnus dealt directly with the subject of diathermancy, a Preliminary Note of the investigation being published in the *Monatsbericht* for February 7th, 1861. This note, like its predecessor, consisted of general and descriptive statements, no actual measurements being given. The completed memoir was first published in Poggendorff's *Annalen* for April 1861. For the purposes of this new inquiry the apparatus used in the experiments on gaseous conduction was modified, as shown in fig. 2. To the recipient A B a second one G F was attached, the two being connected by the tubulure shown in the figure. The recipient G F rested upon the plate of an air-pump, on which also stood the thermopile *p*, with one of its faces turned towards the source. From the pile, through the air-pump plate, wires passed to the galvanometer. With this apparatus, the absorption by atmospheric air and by oxygen was found to be 11·12, and by hydrogen 14·1 per cent. of the total radiation. The alleged conductivity of hydrogen did not therefore manifest itself in these experiments.

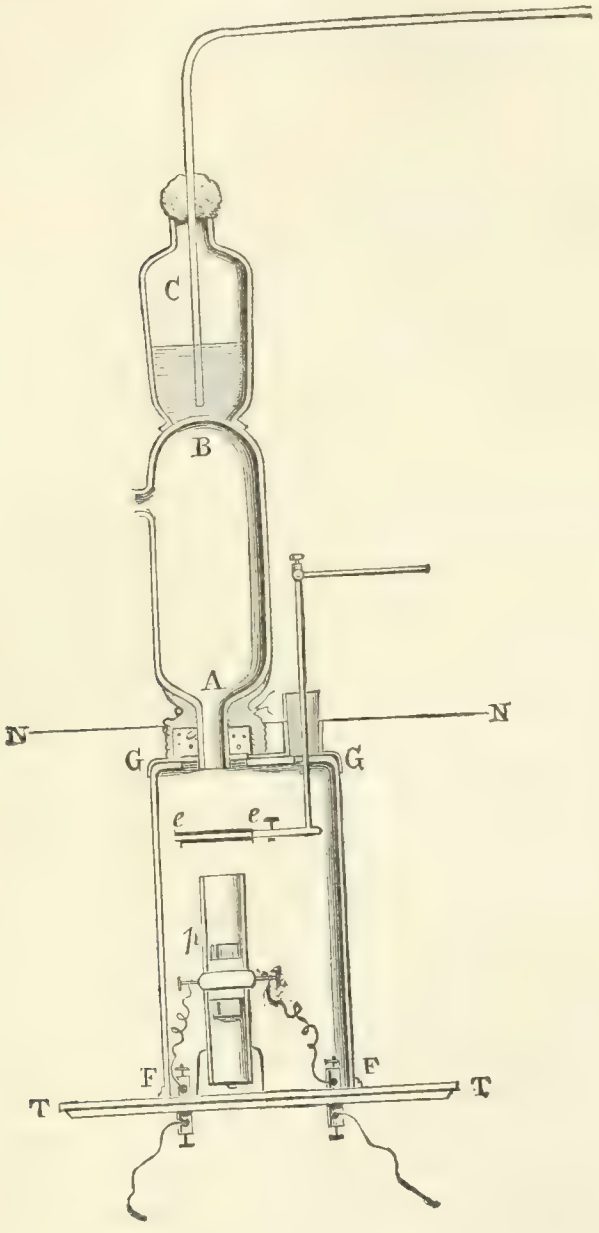
Let us analyze these results. In the first experiments the distance of the thermometer from the source of heat was 35 millimetres. The action on the thermometer through a vacuum being represented by 100, the action through air and through oxygen of this depth was found to be 82. The loss of 18 per cent. in air and in oxygen was alleged to be due to the adiathermancy of these media; to which percentage, if we wish to ascertain the *total* absorption by air, we should have to add such heat as reached the thermometer by conduction.

Turning now to the modified apparatus, which is evidently drawn to scale, the gas here traversed by the radiant heat was about 275 millimetres in depth, while the stratum traversed in the first experiments was, as stated, only 35 millimetres. Yet in these first experiments an absorption of 18 per cent., while in the later ones an absorption of only 11·21 per cent., is assigned to air. In other words, when the depth of the aerial stratum was augmented more than sevenfold, the absorption, instead of increasing, fell to less than two thirds of that of the shallower stratum. It is pretty obvious that an influence different from pure absorption came here into play. That influence was convection.

Anxious to probe this matter to the bottom, and to abolish,

or account for, the differences between my friend and myself, I wrote to him proposing an exchange of apparatus—that he should send his to London, and I mine to Berlin. I afterwards had a facsimile of his apparatus constructed in London,

Fig. 2.



and satisfied myself by actual trial that it was really hampered with the defects I had ascribed to it. By means of the striæ of incense smoke and of chloride of ammonium, the fact of convection in air was rendered plainly visible to the eye; while the behaviour of hydrogen, under like circumstances,

revealed the cause of its transporting more heat than the vacuum in the first experiments, and less heat, not only than the vacuum, but than air or oxygen, in the second experiments. In the one case the thermometer, being close to the source, came within the range of the convection-currents of this mobile gas, the heat being transported to it by these currents. In the other case a considerable distance intervened between the source and the pile, which was further effectually protected by the narrow tubulure. Through it the currents could not pass; but they nevertheless existed in the recipient A B, lowering the temperature of the source without heating the thermoscopic instrument.

The experimental resources of Magnus were great, and he here applied them; but the defects of his method were radical and irremovable. These defects reached their culmination in the subsequent researches of Professor Buff*, who, by pursuing substantially the same method, arrived at the result that a stratum of air $2\frac{1}{2}$ inches thick absorbed 60 per cent. of the radiation from a source of 100° C.† Buff also found olefiant gas to be more diathermanous than air; whereas at atmospheric pressures it is many hundred, and at a pressure of $\frac{1}{30}$ of an atmosphere many thousand times more opaque to heat.

This is the point at which aqueous vapour enters into the experiments of Magnus. When dry and humid air were compared together in his apparatus, no difference between them was observed. But, apart from all disturbance, it would require an instrumental arrangement far more delicate and powerful than that here employed, to bring into view the action of a stratum of mixed air and aqueous vapour 11 inches deep, and having a temperature of only 15° C. Disturbances, however, were not absent. In the first place, the convection currents which enabled dry air to reduce the radiation by 11·12 per cent., were more than sufficient to mask the action of the vapour. Secondly, dry and humid air were brought in succession into direct contact with the face of the thermopile. The pile was therefore affected by any difference of temperature between it and the air, and it could scarcely be supposed that these temperatures were always alike. It was also affected by the condensation and evaporation which occurred when humid air and dry air were brought successively into contact with its lampblack-coated face. To "vapour-hesion" Magnus subsequently ascribed very large effects. Here we have the conditions specially suited to the development of the

* Phil. Mag. 5th ser. vol. iv. p. 401. For my reply see Proc. Roy. Soc. vol. xxx. p. 10.

† Ibid. 1877, vol. iv. p. 424.

action, and yet no reference is made to it. Either, then, the disturbance was overlooked, or the apparatus was not sufficiently delicate to reveal it. To these two sources of disturbance (the lowering of the temperature of the source by convection, and the warming and chilling of the pile by contact, condensation, and evaporation) is to be added another, due to the warming which must have occurred when the dynamically heated air came into direct contact with the thermopile—an action which, in my apparatus, proved sufficient to whirl the needle of the galvanometer more than once through an entire circle.

Magnus next experimented with glass tubes 1 metre long and closed at the ends with plates of glass. His source of heat was a strong gas-flame aided, as in the experiments of Dr. Franz, by a parabolic mirror. Two tubes were employed, the one blackened within, and the other unblackened. With the blackened tube an absorption of 2·44 per cent. was found for air, and an absorption of 3·75 per cent. for hydrogen. In the unblackened tube the absorption by air was 14·75 per cent., and by hydrogen 16·27 per cent. of the total radiation.

I went over this ground with the utmost care, using invisible as well as visible heat. But, substituting plates of pellucid rock-salt for the plates of glass, I failed to realize the effect obtained by Magnus. He ascribed the difference between the results obtained with his blackened and his unblackened tube to a change of quality in the heat, produced by reflection from the interior surface of the latter. With plates of rock-salt, however, though the reflection abides, the change of quality does not occur. My position, therefore, in regard to these experiments is similar to what it was in regard to those of Dr. Franz. The results obtained with air, oxygen, and hydrogen were, I hold, due to the chilling of the heated glass ends of the tube by the cold gases, and the consequent lowering of the secondary radiation.

It was shown by Magnus himself, and is moreover obvious at first sight, that the unblackened tube sent a far greater amount of heat to the glass plate adjacent to the thermopile than the blackened one. That plate being more heated by the source, was more chilled by the air when it entered. The greater cooling-power of hydrogen accounts, moreover, for the advance of the supposed absorption from 14·75 to 16·27 per cent. With carbonic acid Magnus detected a difference which had escaped Dr. Franz. Instead of making the action of this gas equal to that of air, he found in the blackened tube an absorption of 8·19 per cent., and in the unblackened tube an absorption of 21·92 per cent., exerted by carbonic acid.

Here true absorption mixed itself with the effect of mere chilling, while with still more powerful gases the effect of chilling retreated by comparison more and more.

Such were the experiments which determined, in the first instance, the attitude of this distinguished man towards that portion of my work which related to the action of the air and vapour of our atmosphere on radiant heat. In the defence of his position he brought to bear all the resources of consummate skill and large experience. His position, however, was by no means a wholly defensive one. He dwelt repeatedly and emphatically on the dangers—and they are real—to which the method pursued by me was exposed. I had closed my experimental tube with plates of transparent rock-salt; and he urged against me the hygroscopic character of this substance. Placing rock-salt beside a vessel of water under a glass shade, he found that it could be rendered dripping wet *. Hence his argument that, instead of measuring the action of vapour, I had really measured the action of brine. This, however, I could not admit. I was aware of the danger, and had avoided it. In many hundred instances the rock-salt plates had been detached from my experimental tube while filled with the very air which had produced the observed absorption, and found to be as dry as polished plate-glass. For a week at a time I have charged my experimental tube alternately with dried and undried air, removing every evening the plates of salt while the humid air filled the tube. Their dryness and polish were found unimpaired †. I have frequently flooded the experimental tube with light, and watched narrowly whether any dimness showed itself on the salt, or on the interior surface, when the humid air entered. There was nothing of the kind. I finally abandoned the plates of salt altogether, and obtained in a tube opened at both ends substantially the same effects as those obtained when the tube was closed with plates of rock-salt.

In 1862 Magnus came to London. He had been previously working at the points of difference between us, and had strengthened his first conviction. The action of the air he had found to be considerable, and the action of aqueous vapour practically nil. I also had been working, but with an entirely

* It has been shown by Professor Dewar that the exposure of a dry plate of rock-salt for five minutes to saturated air sensibly augments the weight of the salt as determined by a delicate balance.

† This mastery over the apparatus was not attained without training. Any lapse of care soon declared itself by the condition of the plates of salt.

different result. It was hoped by both of us that our differences would be settled during this visit. With my closed experimental tube I showed him the neutrality of dry air and the activity of humid air; and while the latter was in the tube I detached the rock-salt plates and placed them in his hand. He closely inspected them, passed his dry handkerchief over them, and frankly and emphatically pronounced them perfectly dry. I then executed in his presence the experiments with the open tube, and reproduced the results which I had previously published. I subjected the method of compensation to a severe test, and showed him how exact it could be made. He frankly confessed his inability to find any flaw in my experiments, and, save in one particular, made no attempt to reconcile our differences. He accounted for the neutrality of dry air observed by me by pointing to my thermopile, between which and the experimental tube a space of air intervened. He argued, and justly argued, that though the calorific rays were permitted to enter the tube from a vacuum, if the air intervening between tube and pile could produce the effect which he ascribed to it, the heat would be robbed of its absorbable rays before the dry air entered the tube, the subsequent neutrality of dry air being a matter of course. The logic was good; but its basis I knew to be more than doubtful; and I therefore asked him whether a layer $\frac{1}{20}$ of an inch thick between pile and tube would produce any sensible effect. His reply was an emphatic negative. In subsequent experiments, therefore, the conical reflector was removed from my pile, and placed within the experimental tube, its narrow end being caused to abut against the plate of rock-salt. The face of the pile was then brought within less than $\frac{1}{20}$ of an inch of the rock-salt plate; and in this way my former measurements, which had declared the pure air of our atmosphere to be a practical vacuum to radiant heat, were verified to the letter.

The well-earned fame of Magnus as an experimenter, and his personal friendliness to myself, rendered it specially incumbent on me to deal respectfully with every one of his suggestions. He once intimated to me that the absorption, which I had supposed due to aqueous vapour, might be really due to the smoke and dust suspended in London air. To meet this I carried air myself from the Isle of Wight, had it carried from Epsom Downs and other places, and found the aqueous vapour diffused in such air to be from 60 to 70 times more energetic than the air itself. London air, moreover, was freed from its suspended matter, and tested when dry: it proved neutral. The self-same air was then rendered humid:

its absorbent power was restored. Then, with dry air as a carrier, I introduced smoke into the experimental tube, until it far exceeded in density that suspended in the London air when Magnus drew my attention to it. The quantity of heat intercepted by this smoke proved to be only a fraction of that absorbed by the perfectly invisible aqueous vapour.

On his return to Berlin he resumed his labours. He had been especially impressed by the experiments with the open tube; and to this point he directed his chief attention. "The result of this experiment," he writes, "was so surprising, and so little in accord with what I had found by other methods, that on reaching home I determined to repeat the experiment." He did so, with this result:—"I have," he says, "repeated the blowing-in of dry air and moist air many hundred times; but in no single case was the deflection such as to indicate a greater absorption by moist air"*. Humid air, in his experiments, produced the deflection of heat; dry air the deflection of cold—a result diametrically opposed to mine. In London he had seen that my deflections were as large as I had affirmed them to be; but he had not criticised them with the view of ascertaining whether they were, or were not, in the right direction. In these new experiments, however, he had, he thought, hit upon their origin. The moving air had reached the face of the thermopile, producing, when humid, heat by condensation, and, when dry, cold by evaporation.

I read the account of these experiments with some concern; for it was thereby made plain to me that Magnus had by no means realized the anxious care that I had bestowed upon my work. The testimony of an independent observer would, I thought, set the matter right. My apparatus, carefully adjusted, was accordingly handed over to Dr. Frankland, who minutely tested every point involved in, or arising out of, the objection of Magnus. He verified all my results. His opinion as to the accuracy of the method of compensation is worth recording. "In conclusion," he writes, "I cannot but express my surprise and admiration at the precision and sharpness of the indications of your apparatus. Without having actually worked with it, I should not have thought it possible to obtain these qualities in so high a degree in determinations of such extreme delicacy"†. To this may be

* Poggendorff's *Annalen*, 1863, vol. cxviii. p. 580; Phil. Mag. 1863, vol. xxvi. p. 25.

† The total heat here employed amounted to 86°·2 of a quadrant. This exceedingly large deflection was neutralized by the radiation from the compensating cube. But so accurately were the two sources balanced, and so constant was the radiation on both sides, that the determinations were made with ease, and without sensible disturbance or fluctuation.

added the subsequent testimony of Professor Wild, now of the University of St. Petersburg, who went carefully over the same ground. "In all my experiments," he says, "conducted according to Tyndall's method, which included more than a hundred distinct observations, I have never obtained deflections of the galvanometer-needle in contradiction to the statements of Professor Tyndall"*.

In an extremely able paper, a translation of which is published in the Philosophical Magazine for October 1866, the Petersburg philosopher compares the methods pursued by Magnus and myself respectively. Insufficient sensitiveness, and the disturbance due to convection-currents, caused him, he says, to abandon the method of Magnus. "Although," he continues, "this method of investigating absorption may, in the hands of so experienced and expert an experimenter as Professor Magnus, be an appropriate one for determining absolute values with great certainty, I feel bound, from my own experience, to give a decided preference to Tyndall's method, not only on account of the greater facility with which it furnishes qualitative [quantitative] results, but also in consequence of its greater delicacy. It is principally in consequence of this greater delicacy that, notwithstanding the negative results furnished by Magnus's method, I maintain that the greater absorptive power of moist air, as compared with dry, has been fully established by the experiments made according to Tyndall's method; and I am of opinion that meteorologists may without hesitation accept this new fact in their endeavours to explain phenomena which hitherto have remained more or less enigmatical."

In 1866 Magnus varied his method of experiment, seeking to solve the question of absorption by observations on radiation. "I have," he says, "made a few determinations of the radiation of dry and moist air, and of some other gases and vapours. Up to the present time," he continues, "the capacity of these bodies to transmit heat has alone been determined"†. He then describes his arrangement:—"The gases and vapours were passed through a brass tube of 15 millimetres internal diameter, which was placed horizontally and heated by gas-flames. One end of the tube was bent upwards, so that the heated air ascended vertically, while at a distance of 400 millimetres from the vertical current was placed the thermopile." When dry air was sent through this tube, the deflection produced was three divisions of a scale; when air which had passed through water at a

* Phil. Mag. 4th ser. vol. xxxii. p. 252.

† This is an inadvertence. Exhaustive experiments on the radiation of gases and vapours had been made and published many years previously

temperature of 15° C. was sent through the tube, the deflection rose to five divisions; when the water was warmed to 60° or 80° Fahr., the deflection was 20 divisions; and when the water boiled, the deflection was 100 divisions. In this last experiment, however, a mist appeared; so that, as urged at the time, the radiation could not be said to have been purely from vapour. In the other cases no mist was visible; but it was nevertheless concluded that the 20-division deflection was due to the formation of mist at the boundary of the ascending current.

I should be disposed to claim these experiments as telling in my favour. The first of them, in my opinion, dealt with the radiation, not from dry air, but from the adjacent aqueous vapour which had been warmed by the dry air. That the deflection in the second experiment was small is not surprising. The radiation which could reach the pile from a jet of air only 15 millimetres in diameter, and containing such moisture as could be taken up at 15° C., must have been extremely small under any circumstances. But in the present case, even this small radiation was diminished by the passage of the heat through 400 millimetres of undried air. I should demur to the explanation of the third experiment, and question the warrant to imagine a mist which could not be seen. Even the fourth experiment, where mist was visible, yielded, I doubt not, a mixed result—part of the effect, and probably the smallest part, being due to the mist, and part of it to the vapour.

With regard to the radiation from hot aqueous vapour, the following experiment is typical of some hundreds which I have had occasion to make. A burner, consisting of two rings provided with numerous small apertures, was placed within a square tin chimney. At some height above the burner the chimney was perforated, so as to enable the radiation from a heated gaseous column within the chimney to reach a distant thermopile. The side of the chimney facing the pile was so protected by screens that the radiation from the chimney itself was nil. Connecting the burner with a bottle of compressed hydrogen, the gas was ignited. A column of hot vapour rose from the burner and passed the aperture in the chimney through which it sent its rays to the pile. Mere tips of flame were first employed, the column of vapour rising from them sufficing nevertheless to produce a permanent deflection of

40°.

A slight augmentation of the flame sent the needle up to

60°.

A still further augmentation sent it up to

75°.

This last deflection was equivalent to more than 400 of the degrees in the neighbourhood of zero.

The radiating column was here considerably above the flame. To examine the condition of the column, a concentrated luminous beam was directed upon it. There was no precipitation. On the contrary, the suspended matter in the air of the chimney was much less than that of the surrounding air. Instead of a white mist, we had the blackness due to the destruction of the floating matter by the hydrogen-flame.

On quenching the flame, the needle returned accurately to zero.

In his objections Magnus, for the most part, dealt with true causes; but he erred as to their scope of action. I never denied the existence of the dangers which he emphasized. The hygroscopic character of rock-salt, for example, to which he recurred so often, cannot be questioned. It has a strong attraction for moisture, especially when cold. On this point my experience has been large; and I applied it in the execution of my experiments. These, as I have so frequently stated, were conducted with plates of salt as dry as polished glass or rock-crystal. Thus, while conceding the fact that rock-salt is hygroscopic, I demur to its application.

A similar remark applies to the last solution offered by Magnus of the differences between us. In 1867 he showed that vapours were condensed by surface-attraction to a greater extent than had previously been supposed. Blowing vapour-laden air into a metal tube, he found that heat was generated. He inferred, and rightly inferred, that this heat was produced by the condensation which occurred on the interior surface. This condensation he found to depend on the condition of the surface, being greater when it was tarnished or coated than when it was polished. He saturated air with moisture at a temperature of 16° C., and then raised both it and his pile to a temperature of 38°. When such air was blown against the dry face of the pile, heat was generated. Condensation therefore occurred upon a surface 22° C. higher than the dew-point of the vapour. Against this I have nothing to urge. But the fact by no means justifies the inference drawn from it, which was, that the vapours in my experimental tube were converted by "vapour-hesion" into liquid layers of high opacity to radiant heat: these layers, acting upon the calorific rays impinging on the interior surface of my tube, produced the absorption which I had erroneously ascribed to the

vapours. More than this, the liquid layers were supposed to be broken up into discontinuous patches, which not only absorbed the heat but scattered it. "Vapour-hesion," it may be added, was found to vary with the liquid which produced the vapour, being particularly strong in the case of alcohol.

Magnus brought this generalization to the test of experiment, but failed to verify it. He urged humid air against a dry mirror, from which radiant heat was reflected; but unless he wetted the mirror visibly, no effect was produced on the reflected beam. Still he held that reflection, oft repeated, rendered sensible an action which eluded a single reflection. My position here is clear. I do not doubt surface attraction, or deny the existence of impalpable films. No experiment was ever made on the reflection of light or radiant heat in which such films did not intervene; but they had as little effect upon my results as they had upon those of De la Provostaye and Desains*, and of other refined experimenters. As early as 1859 I was made aware of the danger which might arise from condensation. Warned by the action of chlorine on my brass experimental tube, I coated it inside with lampblack, and retested with it all my vapours. The result removed from my mind the suspicion that surface condensation had any thing to do with the observed absorption. Many similar experiments with blackened tubes were subsequently made by me, for my own safety and instruction. There was no substantial difference between the results obtained with such tubes and those obtained with polished tubes in which internal reflection came into play.

Such are the general features and phases of a discussion which, though dealing only with a small item of my work, has consumed a considerable amount of time. Other able experimenters have entered this field, the latest of whom, MM. Lecher and Pernter, have published a long and learned memoir in Wiedemann's *Annalen*, which has been translated in the Philosophical Magazine for January 1881. My experiments with gases they corroborate, but not those with vapours. Regarding the action of aqueous vapour they are especially emphatic, their conclusion being "that moist air does not perceptibly absorb the heat-rays from a source of 100° C." In fact, they found moist air a little more transpa-

* Considering the energy of water as a radiator, exceeding, according to Leslie, that of lampblack itself, the film of this liquid which must have covered the plates of silver in the experiments of the two French philosophers ought, if Magnus be correct, to have sensibly raised the emission. Calling the emission from lampblack 100, that from polished silver plus the film was only 2·1.

rent than a vacuum. "No imaginable source of error," they affirm, "has here been left out of account." The arrangement for filling with moist air was varied, the air allowed to stand for a long time over the water in the gas-holder, and this moist air then passed through several wash-bottles into the experimental space, but with the same *negative* result. In common with Magnus, MM. Lecher and Pernter ascribe my results to the condensation of liquid films on the rock-salt plates, and on the polished inner surface of my tube*.

§ 4. *Experiments resumed: Verifications.*

With a view to my own instruction and to the removal of uncertainty from other minds, these researches on radiant heat were resumed in November 1880. A brass experimental tube 4 feet long, 2⅜ inches in diameter, and polished within, was first employed†. Interposed between it and the source was a "front chamber," through which, when exhausted, the rays passed into the experimental tube. A plate of transparent rock-salt separated the tube from the chamber, while a second plate of salt closed the distant end of the experimental tube. The source of heat was at first a Leslie's cube containing water at 100° C., to one of the faces of which the end of the front chamber was carefully soldered. The chamber also passed air-tight through a copper cell in which a continuous circulation of cold water was kept up. The heat which might otherwise have reached the experimental tube by conduction from the source was thus cut off. One face of a thermopile, provided with a reflecting cone, received the rays which passed through the experimental tube. The other face, also provided with a cone, received the rays from a "compensating cube," used, as formerly, to neutralize the radiation from the source, and to bring the needle of the galvanometer to zero when the experimental tube was exhausted. On the entrance, then, of

* However I may otherwise differ from MM. Lecher and Pernter, I agree with their opening remark, that few other questions of experimental physics present difficulties so great as the one here under consideration. Nor do I see reason to differ from their closing words, that "the extraordinary difficulty of investigations of this sort would be richly repaid by the attainment of quantitative results; whilst the corresponding optical investigations (immeasurably easier) will always remain more of a qualitative nature." It is the difficulty here signalized that has caused so many distinguished investigators to go astray in this field of inquiry. I may state here that on the receipt of their paper I wrote to MM. Lecher and Pernter, but my communication was returned from Vienna through the dead-letter office.

† The plate answering to this description will be found in the Philosophical Transactions for 1861.

any absorbent gas or vapour the equilibrium was destroyed, the needle moved from zero; and from the observed permanent deflection the absorption was calculated. Other qualities of heat, and other experimental tubes than that here described, were afterwards introduced into the inquiry.

I here give the measurements executed in 1880 with the vapours of nine different liquids, in experimental tubes of the dimensions above given.

Table I.

		Vapours.		Liquids.
	Pressures.	Leslie's cube, vacuum.	Leslie's cube free.	Red-hot spiral.
Bisulphide of carbon .	·48 in. mer.	4·4	5·0	7·6
Chloroform	·36 „	12·8	12·9	28·8
Benzol	·32 „	14·8	15·0	44·5
Iodide of ethyl . .	·36 „	18·4	19·3	47·0
Iodide of methyl . .	·46 „	25·0	26·2	59·0
Amylene	·26 „	26·1	27·2	65·0
Sulphuric ether . .	·28 „	35·0	35·6	71·0
Acetic ether . . .	·29 „	43·3	43·7	77·5
Formic ether . . .	·36 „	43·3	44·0	78·0

The "pressures" in this table are chosen with a view to the comparison of liquids and vapours. They express quantities of vapour which are proportional to the quantities of matter in the respective liquids at a common thickness. The two next columns contain the absorptions per 100 of the heat from two Leslie's cubes, the one with a vacuum in front of it, the other placed in free air and well protected from air-currents. The close agreement of the two columns proves the "front chamber" to be a superfluity. It also illustrates the coincidence to be attained in these measurements when they are carefully made. In the last column I have placed the absorptions exerted by liquid layers of the respective substances at a common thickness of one millimetre. The source of heat here was an incandescent platinum spiral. The order of absorption of liquids and vapours is the same.

This order is, as might be expected, undisturbed when we apply heat of the same quality to liquids and vapours respectively. This is shown by the following table:—

TABLE II.

	Polished tube.		Blackened tube.
	Spiral dull.	Spiral bright.	Spiral dull.
Bisulphide of carbon .	3°·0	2°·4	3°·0
Chloroform	7·0	4·6	5·0
Benzol	9·4	7·5	10·0
Iodide of ethyl . . .	13·6	8·6	12·3
Iodide of methyl . . .	15·6	10·7	13·8
Amylene	20·6	14·3	17·5
Sulphuric ether . . .	27·0	21·0	23·8
Acetic ether	33·0	23·0	29·0
Formic ether	33·0	23·0	30·0

The vapour-pressures here are the same as those employed in Table I. The order of absorption is the same in both tables; but its amount is diminished. This was to be expected, from the difference in the quality of the heat. We are dealing with transparent vapours—in other words, vapours pervious to the luminous rays; and the greater the proportion of these rays in the calorific beam, the less will be the absorption. This is well illustrated by the second column in the table, which shows the fall of the absorption when the spiral is raised from a dull red to almost a white heat. The polished tube was used in both these cases.

The third column of figures in Table II. shows the results obtained when the experimental tube was coated within with lampblack. The absorptions are in the same order, and almost of the same amount as those of the first column. The case is representative, and might be multiplied to any extent. It is incompatible with the notion that my results were due to films collected on the polished interior surface of my experimental tube.

Placing the substances recently experimented on in the order of their absorption, and also in the order which they exhibited in 1864, we have the following two columns:—

1880.	1864.
Bisulphide of carbon.	Bisulphide of carbon.
Chloroform.	Chloroform.
Benzol.	Iodide of methyl.
Iodide of ethyl.	Iodide of ethyl.
Iodide of methyl.	Benzol.
Amylene.	Amylene.
Sulphuric ether.	Sulphuric ether.
Acetic ether.	Acetic ether.
Formic ether.	Formic ether.

In 1864, therefore, the iodide of methyl proved itself more diathermanous than the iodide of ethyl, while both of them were more diathermanous than benzol. In 1880 the case was precisely the reverse. Suspecting that the discrepancy might be due to impurity, I requested my friend Professor Dewar to subject the liquids to a further process of purification. Tested afterwards, they produced the following deflections:—

	A.	B.	C.
	°	°	°
Bisulphide of carbon .	5·0	5·0	4·0
Chloroform	17·0	15·0	6·0
Iodide of methyl . . .	39·4	33·0	8·0
Iodide of ethyl . . .	33·0	35·0	12·5
Amylene	42·0	41·0	16·0
Sulphuric ether . . .	44·3	43·5	18·2
Acetic ether	46·2	45·5	22·0
Formic ether	47·5	46·9	22·2

Under A and B respectively are the deflections produced by the liquids prior to and after purification. The iodide of methyl falls from 39°·4 to 33°, while the iodide of ethyl rises from 33° to 35°. The relative positions occupied by the liquids in 1864 are thus restored. Benzol, however, remained permanently lower than before. Under C are the deflections produced by the vapours of the purified liquids. Here also the positions of the two iodides are reversed, vaporous absorption following the order of liquid absorption. I have frequently encountered cases of this character. The simultaneous change of diathermic position of liquid and vapour indicates that the foreign ingredient, whatever it was, possessed approximately the same volatility as the substance which it vitiated.

[To be continued.]

LIX. *Notices respecting New Books.*

Kant's Critique of Pure Reason: translated into English by F. Max Muller. 2 vols. London: Macmillan and Co.

THIS translation of Kant's great work appears in commemoration of the Centenary of its first publication. In the second volume we have the work proper, namely, a translation of the first edition: and in the first volume we have a preface by the translator, an Historical Introduction by Prof. Noiré, and a translation of the supplements which were added by Kant to the second edition.

In his preface the eminent translator states why he thought he might translate Kant's Critique, why he thought he ought to do it,

why a study of the work in question seemed necessary at present, what he considers the relation of Kant's Metaphysic to Physical Science, and in what manner Kant's Philosophy has been judged by History. As regards the first point, it may well be asked where could a philosopher more happily fitted for the task be found; and as regards the second, it is most interesting to learn the extent to which the Professor's own life-work has been influenced by the Critique. As regards the fourth point, the translator says:—"Metaphysical truth is wider than physical truth; and the new discoveries of physical observers, if they are to be more than merely contingent truths, must find their appointed place and natural refuge within the immovable limits traced by the metaphysician. It was an unfortunate accident that gave to what ought to have been called pro-physical, the name of metaphysical science: for it is only after having mastered the principles of metaphysic that the student of nature can begin his work in the right spirit, knowing the horizon of human knowledge, and guided by principles as unchangeable as the pole star" (p. xxxii). Prof. Max Müller, notwithstanding his great authority, will get few natural philosophers to agree with what he here lays down. On more than one occasion that which the metaphysician has pronounced to be impossible has been that which the physicist has accomplished. For instance, as was pointed out by Sir John Lubbock in his Address as President of the British Association (p. 29), Comte laid it down as an axiom regarding the heavenly bodies, that "Nous concevons la possibilité de déterminer leurs formes, leurs distances, leurs grandeurs et leurs mouvements, tandis que nous ne saurions jamais étudier par aucun moyen leur composition chimique ou leur structure minéralogique." That was in 1842; and now, in 1882, there exists an elaborate science of Solar and Stellar Physics due to the method of Spectrum Analysis. Just above the place quoted, Professor Max Müller asks, "But how can any one who weighs his words say that the modern physiology of the senses has in any way supplemented or improved Kant's theory of knowledge? As well might we say that Spectrum Analysis has improved our logic." It is evident that Spectrum Analysis would have been capable of improving Comte's logic. If it be objected that Comte was not a true but a false metaphysician, we quote in reply what Professor Max Müller states at p. xxi:—"Even Comte, ignorant as he was of German and German philosophy, expressed his satisfaction and pride when he discovered how near he had, though unconsciously, approached to Kant's philosophy." The majority of scientists will probably fail to see any misfortune in the name *meta*physical, but, on the contrary, hold that a theory of knowledge, to be of any value, must be dictated by the exact and natural sciences; that such a theory may dictate back to the sciences, and they may re-dictate to it; and that it is only by a mutual development of this kind that either can be made to approach perfection.

Professor Noiré, in his Introduction, traces the course of

philosophy through ancient and modern times, ending with Hume. He concludes with the following estimate of Kant; and from this estimate one may infer the point of view from which the great thinkers of ancient and modern times have been surveyed:—"It is therefore not too much to say that Kant is the greatest philosophical genius that has ever dwelt upon earth, and the 'Critique of Pure Reason' the highest achievement of human wisdom."

In this notice we have not the requisite space for discussing the Critique itself; but we may state briefly certain fundamental objections which may, it appears to us, be urged against it. The following extract indicates in a summary manner Kant's fundamental position. "Even if we remove from experience everything that belongs to the senses, there remain nevertheless certain original concepts, and certain judgments derived from them, which must have had their origin entirely *à priori*, and independent of all experience, because it is owing to them that we are able, or imagine we are able, to predicate more of the objects of our senses than can be learnt from mere experience, and that our propositions contain real generality and strict necessity, such as mere empirical knowledge can never supply" (p. 2). His object is to make a complete inventory of this *à priori* knowledge. "Whatever the understanding possesses, as it has not to be looked for without, can hardly escape our notice, nor is there any reason to suppose that it will prove too extensive for a complete inventory" (p. 11). "It is true that our Critique must produce a complete list of all the fundamental concepts which constitute pure knowledge" (p. 12). The inventory of the elements of transcendental æsthetic consists of two—namely, space and time; motion is rejected on the ground that it presupposes something empirical—the perception of something moving. But if we turn to Modern Science, which for the present purpose means *Thomson and Tait's Treatise on Natural Philosophy*, we find that there is a branch of Pure Mathematics, called Kinematics, which treats of the circumstances of mere motion considered without reference to the bodies moved. Its fundamental ideas are Velocity and Acceleration, neither of which involves the conception of Mass. If Space and Time only are *à priori*, how is it that Space, Time, and Mass are treated of by writers on Dynamics as the three fundamental ideas?

Kant was a mathematician: otherwise the Critique would not have been written. The influence of his study of the Mathematics is apparent everywhere. He views that science as so much *à priori* knowledge. "How far we can advance independently of all experience in *à priori* knowledge is shown by the brilliant example of Mathematics" (p. 4). "In the same manner all geometrical principles, *e.g.* that in every triangle two sides together are greater than the third, are never to be derived from the general concepts of side and triangle, but from an intuition, and that *à priori*, with apodictic certainty" (p. 22). That geometrical principles cannot be evolved out of mere definitions would not require to be denied,

had it not actually been held by some philosophers. Mathematicians will also agree with Kant in the stress which he lays upon the construction. But there is one point which it seems to us has not been dwelt upon sufficiently. Let us take Euclid. i. 16. If we examine the proof, we shall find that it turns upon the assertion that the angle ACF is less than the angle ACD. Now this may be easily asserted for a particular triangle; but before we can assert it for any triangle, we have to investigate the matter and glance over all the possible cases. If this be intuition, then it is hard to distinguish intuition from the process of generalization which the mind goes through in forming general judgments which Kant would characterize as empirical, such as that contained in the last clause of the following sentence:—"We know nothing but our manner of perceiving them, that manner being peculiar to us, and not necessarily shared in by every being, *though, no doubt, by every human being*" (p. 37).

Since Kant's day the domain of Mathematics has been greatly extended, and subjects formerly treated in a manner which was truly empirical are now treated in a manner which is exact. That domain includes even his own science of Logic. If, then, that vast body of knowledge is to be put down as *à priori*, by *à priori* can be meant nothing else than *exact*. If the Modern Logic, as developed and expounded by De Morgan, Boole, Harley, Jevons, Macfarlane, and Venn in this country, by Halsted and Pierce in America, and by Schröder in Germany, be sound science, then Kant's 'Critique' must contain considerable imperfections: for it proceeds on the assumption that the scholastic logic was perfect. "I mean only to treat of reason and its pure thinking, a knowledge of which is not very far to seek, considering that it is to be found within myself. Common logic gives an instance how all the simple acts of reason can be enumerated completely and systematically" (p. xxiv). "That Logic, from the earliest times, has followed that secure method may be seen from the fact that since Aristotle it has not had to retrace a single step, unless we choose to consider as improvements the removal of some unnecessary subtleties, or the clearer definition of its matter, both of which refer to the elegance rather than to the solidity of the science. It is remarkable also, that to the present day it has not been able to make one step in advance, so that, to all appearance, it may be considered as completed and perfect" (Sup., p. 364). Whence are the Pure Concepts of the Understanding (the Categories) obtained? From a classification of judgments "differing in some, though not very essential, points from the usual technicalites of logicians." The classification according to Quantity is Universal, Particular, Singular. It may be asked, What of doubly universal judgments, such as, for example, Κρῆτες ἀεὶ ψεῦσται? It may be pointed out that we may have judgments universal with respect to both Space and Time, or with respect to Space only, or with respect to Time only, or with respect to neither. But the proper

objection is, that the number of logical forms frequently occurring in judgments is by no means so small as stated by the old logicians. They have no proper place for the equations of Algebra, or for statements about Relationships. In what intelligible sense can $7+5=12$ be called a singular judgment? (p. 145). The list of the categories, being derived from an imperfect list of logical forms, cannot be perfect. It may be described, to use one of Kant's own phrases, as the result of a search after pure concepts undertaken haphazard, the completeness of which, as based on induction only, can never be guaranteed.

Lessons on Form. *By* R. P. Wright, *Teacher of Mathematics, University College School, London.* 8vo. pp. 103. Longmans and Co.: 1882.

This short treatise aims at teaching the first elements of Geometry in an informal manner, as compared with the martinetism of Euclid, after the method of Pestalozzi. Commencing with the forms of the principal regular solids, it passes to the analysis of the relations among straight lines intersecting one another, and of the angles which they thus make, whether two are parallels cut by a secant or whether no two are parallel. In the three chapters treating of these matters formal proofs are sparingly introduced, the treatment being rather descriptive than demonstrative. Chapter 4 is a short initiation into the study of the triangle: the conception of similar triangles, having commensurable sides, being early introduced. The subject of the varieties of the Quadrilateral and the method of measuring areas occupy the remaining two chapters of the First Part.

Part II. treats of the metrical properties of triangles, as depending on Pythagoras's Principle, of which several proofs are given. The consideration of regular Polygons and of the Circle, as their limit, closes the treatise.

From this brief analysis of the scheme and contents of the book, it will be seen that pupils who have been guided to master it intelligently will have acquired a considerable amount of Geometrical knowledge: and this, as it is hoped, with little or no drudgery. The skilful teacher will know how to select those parts particularly which seem most suited to the degrees of development and aptitudes of the learners. That he will find the book most helpful and suggestive in a mode of instruction out of the usual routine path will be evident. This smaller work may be regarded as introductory to Mr. Wright's larger and more formal 'Elements of Plane Geometry', now in its fourth edition, a fact which stamps it as a standard treatise.

LX. *Intelligence and Miscellaneous Articles.*

ON THE LIQUEFACTION OF OZONE.

BY P. HAUTEFEUILLE AND J. CHAPPUIS.

M. CAILLETET having placed at our disposal the apparatus he has set up at the Ecole Normale for the liquefaction of ethylene, as well as the whole arrangement which he employs in order to study the changes of state of what are called the permanent gases, we have been able to complete our first experiments upon the liquefaction of ozone.

The study of the conditions under which a sudden expansion determines the formation of a mist in a mixture of oxygen and ozone has permitted us to establish that pure ozone is less easily liquefied than carbonic acid: the addition of carbonic acid to that mixture furnishes by compression a liquid of a pale blue colour, which coloration we attributed to ozone liquefied at the same time as the carbonic acid. From all these facts we concluded that it was possible to obtain ozone in a liquid form, and that that liquid would be strongly coloured.

Those deductions have just been fully confirmed by experiment: for we have obtained ozone in liquid drops of a deep indigo-blue colour. This liquid has been preserved nearly thirty minutes under a pressure of 75 atmospheres; its vaporization is not very rapid, even under the atmospheric pressure.

The liquefaction was attained by compressing with about 125 atmospheres a mixture of oxygen and ozone contained in the test-tube of M. Cailletet's apparatus, which is terminated by a capillary tube recurved at its upper portion, permitting the descending branch to be immersed in a jet of liquid ethylene, lowering its temperature probably below -100°. When the gas operated on contains not more than 10 per cent. by weight of ozone, the gas compressed with 125 atmospheres is not perceptibly coloured in the ascending branch, while the blue coloration is very distinct in all the refrigerated portion of the capillary tube.

Does this coloration depend on the presence of a mixed liquid formed of ozone and oxygen, or on that of a thin film of liquid ozone lining the interior of the capillary tube? The absence of a distinct meniscus leaves this question undecided; but, in favour of the first of these hypotheses, we have noticed that the coloration was not more intense in the lower than in the upper part of the refrigerated tube, and that a sudden expansion does not determine the formation of a mist, which so well indicates, in M. Cailletet's experiments, the passage from the gaseous to the liquid state.

The tube becomes instantaneously colourless in consequence of the expansion, and contains in the thin part which terminates it a liquid drop of a deep indigo-blue: the ozone contained in the mixture is almost all condensed in the sloping part; for a fresh compression with 125 atmospheres does not communicate to the tube any appreciable coloration.

Once the ozone is liquefied in the capillary tube, it remains in that state long enough, even under the pressure of the atmosphere, for it to be examined either through the liquid ethylene or the refrigerated tube being withdrawn for a moment from that liquid. The deep-blue liquid gradually diminishes in volume: the vaporization of the ozone is sufficiently slow, its diffusion sufficiently rapid, for the gas to appear colourless above the nearly black liquid: it is only at the moment when the last traces of the liquid disappear that one ascertains the production of an azure-blue gas. The vaporization of the liquid ozone would bring back the system to its initial state, if the ozone were not slowly decomposed by the mercury employed to compress the gases.—*Comptes Rendus de l'Académie des Sciences*, May 1, 1882, pp. 1249–1251.

ON THE ACTION OF TELEPHONIC CURRENTS UPON THE GALVANOMETER. BY M. DE CHARDONNET.

If, in the telegraphic circuit, the receiver be replaced by a very sensitive galvanometer, and the transmitter be influenced by means of a diapason, an organ-pipe, or the voice, no deflection is observed as long as the sound keeps the same intensity; but as soon as the sonorosity is increased or diminished, the needle is deflected: the direction of the deflection varies according to whether the amplitude of the vibrations of the sounding body is increasing or decreasing. The effect is more marked when the transmitter is shaken by a noise of short duration, such as a detonation, or by a body lightly tapped upon the vibrating plate. In this last case the galvanometer-needle leaps like the seconds-hand of a clock. The experiment also succeeds very well when the sounding body is briskly moved nearer to or away from the transmitter. These deflections appear, of course, much more distinctly when a microphone-transmitter, like Ader's, is employed; but they are visible with every kind of telephone.

The explanation appears very simple, at least for the whole of the facts collectively:—

As long as the oscillations of the vibrating plate retain the same amplitude, and consequently the same velocity, the currents induced alternately in the two directions at each complete vibration compensate each other's action upon the galvanometer, whether they emanate from an electromagnet or a microphone; but if the oscillations tend towards zero, for example, each odd semioscillation has more amplitude than the following even semioscillation, and the direct and the inverse induced currents no longer put in motion the same quantities of electricity. The residues, of the same sign at each complete oscillation, accumulate so as to deflect the needle: and the more rapidly the sound diminishes, the greater is the deflection. This is what experiment demonstrates.—*Comptes Rendus de l'Académie des Sciences*, March 27, 1882, t. xciv. p. 857.

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LXI. *Preliminary Notice of the Results accomplished in the Manufacture and Theory of Gratings for Optical purposes.* *By* Prof. H. A. ROWLAND, *of the Johns Hopkins University, Baltimore**.

IT is not many years since physicists considered that a spectroscope constructed of a large number of prisms was the best and only instrument for viewing the spectrum where great power was required. These instruments were large and expensive, so that few physicists could possess them. Professor Young was the first to discover that some of the gratings of Mr. Rutherfurd showed more than any prism-spectroscope which had then been constructed. But all the gratings which had been made up to that time were quite small, say one inch square, whereas the power of a grating in resolving the lines of the spectrum increases with the size. Mr. Rutherfurd then attempted to make as large gratings as his machine would allow, and produced some which were nearly two inches square, though he was rarely successful above an inch and three quarters, having about thirty thousand lines. These gratings were on speculum-metal, and showed more of the spectrum than had ever before been seen, and have, in the hands of Young, Rutherfurd, Lockyer, and others, done much good work for science. Many mechanics in this country and in France and Germany have sought to equal Mr. Rutherfurd's gratings, but without success.

Under these circumstances I have taken up the subject with the resources at command in the physical laboratory of the Johns Hopkins University.

One of the problems to be solved in making a machine is to make a perfect screw; and this mechanics of all countries have sought to do for over a hundred years, and have failed. On thinking over the matter, I devised a plan whose details I

* Communicated by the Author.

shall soon publish, by which I hoped to make a practically perfect screw; and so important did the problem seem, that I immediately set Mr. Schneider, the instrument-maker of the university, at work at one. The operation seemed so successful that I immediately designed the remainder of the machine, and have now had the pleasure since Christmas of trying it. The screw is practically perfect, not by accident, but because of the new process for making it; and I have not yet been able to detect an error so great as one one-hundred-thousandth part of an inch at any part. Neither has it any appreciable periodic error. By means of this machine I have been able to make gratings with 43,000 lines to the inch, and have made a ruled surface with 160,000 lines on it, having about 29,000 lines to the inch. The capacity of the machine is to rule a surface $6\frac{1}{4} \times 4\frac{1}{4}$ inches with any required number of lines to the inch, the number only being limited by the wear of the diamond. The machine can be set to almost any number of lines to the inch; but I have not hitherto attempted more than 43,000 lines to the inch. It ruled so perfectly at this figure that I see no reason to doubt that at least two or three times that number might be ruled in one inch, though it would be useless for making gratings.

All gratings hitherto made have been ruled on flat surfaces. Such gratings require a pair of telescopes for viewing the spectrum. These telescopes interfere with many experiments, absorbing the extremities of the spectrum strongly; besides, two telescopes of sufficient size to use with six-inch gratings would be very expensive and clumsy affairs. In thinking over what would happen were the grating ruled on a surface not flat, I thought of a new method of attacking the problem; and soon found that if the lines were ruled on a spherical surface, the spectrum would be brought to a focus without any telescope. This discovery of concave gratings is important for many physical investigations, such as the photographing of the spectrum both in the ultra-violet and the ultra-red, the determination of the heating-effect of the different rays, and the determination of the relative wave-lengths of the lines of the spectrum. Furthermore it reduces the spectroscope to its simplest proportions, so that spectroscopes of the highest power may be made at a cost which can place them in the hands of all observers. With one of my new concave gratings I have been able to detect double lines in the spectrum which were never before seen.

The laws of the concave grating are very beautiful on account of their simplicity, especially in the case where it will be used most. Draw the radius of curvature of the mirror

to the centre of the mirror, and from its central point, with a radius equal to half the radius of curvature draw, a circle ; this circle thus passes through the centre of curvature of the mirror and touches the mirror at its centre. Now, if the source of light is anywhere in this circle, the image of this source and the different orders of the spectra are all brought to focus on this circle. The word focus is hardly applicable to the case, however; for if the source of light is a point, the light is not brought to a single point on the circle, but is drawn out into a straight line with its length parallel to the axis of the circle. As the object is to see lines in the spectrum only, this fact is of little consequence provided the slit which is the source of light is parallel to the axis of the circle. Indeed it adds to the beauty of the spectra, as the horizontal lines due to dust in the slit are never present, as the dust has a different focal length from the lines of the spectrum. This action of the concave grating, however, somewhat impairs the light, especially of the higher orders; but the introduction of a cylindrical lens greatly obviates this inconvenience.

The beautiful simplicity of the fact that the line of foci of the different orders of the spectra are on the circle described above, leads immediately to a mechanical contrivance by which we can move from one spectrum to the next and yet have the apparatus always in focus ; for we only have to attach the slit, the eye-piece, and the grating to three arms of equal length, which are pivoted together at their other ends, and the conditions are satisfied. However we move the three arms, the spectra are always in focus. The most interesting case of this contrivance is when the bars carrying the eye-piece and grating are attached end to end, thus forming a diameter of the circle, with the eye-piece at the centre of curvature of the mirror, and the rod carrying the slit alone movable. In this case the spectrum as viewed by the eye-piece is normal; and when a micrometer is used, the value of a division of its head in wave-lengths does not depend on the position of the slit, but is simply proportional to the order of the spectrum, so that it need be determined once only. Furthermore, if the eye-piece is replaced by a photographic camera, the photographic spectrum is a normal one. The mechanical means of keeping the focus is especially important when investigating the ultra-violet and ultra-red portions of the solar spectrum.

Another important property of the concave grating is that all the superimposed spectra are in exactly the same focus. When viewing such superimposed spectra, it is a most beautiful sight to see the lines appear coloured on a nearly white ground. By micrometric measurement of such superimposed spectra,

we have a most beautiful method of determining the relative wave-lengths of the different portions of the spectrum, which far exceeds in accuracy any other method yet devised. In working in the ultra-violet or ultra-red portions of the spectrum, we can also focus on the superimposed spectrum, and so get the focus for the portion experimented on.

The fact that the light has to pass through no glass in the concave grating makes it important in the examination of the extremities of the spectrum, where the glass might absorb very much.

There is one important research in which the concave grating in its present form does not seem to be of much use; and that is in the examination of the solar protuberances; an instrument can only be used for this purpose in which the dust in the slit and the lines of the spectrum are in focus at once. It might be possible to introduce a cylindrical lens in such a way as to obviate this difficulty. But for other work on the sun the concave grating will be found very useful. But its principal use will be to get the relative wave-lengths of the lines of the spectrum, and so to map the spectrum; to divide lines of the spectrum which are very near together, and so to see as much as possible of the spectrum; to photograph the spectrum so that it shall be normal; to investigate the portions of the spectrum beyond the range of vision; and, lastly, to put into the hands of any physicist at a moderate cost such a powerful instrument as could only hitherto be purchased by wealthy individuals or institutions.

To give further information of what can be done in the way of gratings I will state the following particulars:—

The dividing-engine can rule a space $6\frac{1}{4}$ inches long and $4\frac{1}{4}$ inches wide. The lines, which can be $4\frac{1}{4}$ inches long, do not depart from a straight line so much as $\frac{1}{100000}$ inch, and the carriage moves forward in an equally straight line. The screw is practically perfect, and has been tested to $\frac{1}{100000}$ inch without showing error. Neither does it have any appreciable periodic error; and the periodic error due to the mounting and graduated head can be entirely eliminated by a suitable attachment. For showing the production of ghosts by a periodic error, such an error can be introduced to any reasonable amount. Every grating made by the machine is a good one, dividing the 1474 line with ease; but some are better than others. Rutherfurd's machine only made one in every four good, and only one in a long time which might be called first-class. One division of the head of the screw makes 14,438 lines to the inch. Any fraction of this number in which the numerator is not greater than, say, 20 or 30 can be

ruled. Some exact numbers to the millimetre, such as 400, 800, 1200, etc., can also be ruled. For the finest definition either 14,438 or 28,876 lines to the inch are recommended—the first for ordinary use, and the second for examining the extremities of the spectrum. Extremely brilliant gratings have been made with 43,314 lines to the inch; and there is little difficulty in ruling more if desired. The following show some results obtained:—

Flat grating, 1 inch square, 43,000 lines to the inch. Divides the 1474 line in the first spectrum.

Flat grating, 2×3 inches, 14,438 lines to the inch, total 43,314. Divides 1474 in the first spectrum, the E line (Ångström 5269·4) in the second, and is good in the fourth and in even the fifth spectrum.

Flat grating, 2×3 inches, 1200 lines to 1 millimetre. Shows very many more lines in B and A groups than were ever before seen.

Flat grating, $2 \times 3\frac{1}{4}$ inches, 14,438 lines to the inch. This has most wonderful brilliancy in one of the first spectra, so that I have seen the Z line, wave-length 8240 (see Abney's map of the ultra-red region), and determined its wave-length roughly, and have seen much further below the A line than the B line is above the A line. The same may be said of the violet end of the spectrum. But such gratings are only obtained by accident.

Concave grating, 2×3 inches, 7 feet radius of curvature, 4818 lines to the inch. The coincidences of the spectra can be observed to the tenth or twelfth spectrum.

Concave grating, 2×3 inches, 14,438 lines to the inch, radius of curvature 8 feet. Divides the 1474 line in the first spectrum, the E line in the second, and is good in the third or fourth.

Concave grating, $3 \times 5\frac{1}{2}$ inches, 17 feet radius of curvature, 28,876 lines to the inch, and thus nearly 160,000 lines in all. This shows more in the first spectrum than was ever seen before. Divides 1474 and E very widely, and shows the stronger component of Ångström 5275 double. Second spectrum not tried.

Concave grating, $4 \times 5\frac{3}{4}$ inches, 3610 lines to the inch, radius of curvature 5 feet 4 inches. This grating was made for Professor Langley's experiments on the ultra-red portion of the spectrum, and was thus made very bright in the first spectrum. The definition seems to be very fine, notwithstanding the short focus, and divides the 1474 line with ease. But it is difficult to rule so concave a grating, as the diamond marks differently on the different parts of the plate.

These give illustrations of the results accomplished; but of course many other experiments have been made. I have not yet been able to decide whether the definition of the concave grating fully comes up to that of a flat grating; but it evidently does so very nearly.

Baltimore, May 25, 1882.

LXII. *Crystallographic Notes.* *By* L. Fletcher, *M.A., of the Mineral Department, British Museum**.

[Plate X.]

IX. *Skutterudite.*

THE first mention of this mineral was made in 1827 by Breithaupt†, to whom it had been submitted by his brother-in-law on returning from a voyage in Norway. Though none of the specimens presented crystal-faces, Breithaupt found that there were distinct cleavages parallel to the sides of a cube, for which reason he assigned to the species the name of Tesseral-Kies. Cleavages, more or less interrupted and indistinct, were found to exist parallel to planes truncating the edges and quoins of this cube, indicating that the crystallisation was that characteristic of the cubic system. Breithaupt went a step further, and, from traces of separation which manifested themselves in the direction of the octahedral planes, hazarded the conjecture, since (curious to say) verified, that the crystals would prove to present the octahedron as the predominant form. The specimens were of a bright metallic lustre and of a tin-white colour. The specific gravities of five different fragments were determined to be 6·659, 6·681, 6·718, 6·748, 6·848, and thus had a considerable range. As, however, after breaking up the fragments, it was seen that the first three included particles of quartz and actinolite, Breithaupt regarded the true specific gravity as lying between 6·748 and 6·848. The hardness was greater than that of cobaltite or Cobaltkies; and as blowpipe examination had indicated that cobalt and arsenic were the chief constituents, Breithaupt suggested the secondary title of Hartkobaltkies. The associated minerals were cobaltine, copper pyrites, glassy actinolite, serpentine, quartz, and sometimes also cobalt-bloom.

* Communicated by the Author, having been read before the Crystallological Society, May 30, 1881.

† "Ueber eine neue Kies-Species von Skutterud," Pogg. *Ann.* vol. ix. p. 115 (1827).

Fig. 1.

Fig. 2.

c (111)
d (110)
n (hkl)
n (hkl)
n (hkl)
n (hkl)

Fig. 3.

Fig. 4.

Skutterudite

Mason

Ten years later Scheerer* met with a mineral which examination led him to regard as identical with that previously described by Breithaupt. A quantitative analysis, afterwards confirmed by Wöhler†, gave the following result:—

Arsenic	77·84
Cobalt	20·01
Sulphur	0·69
Iron	1·51
Copper	traces
	100·05

a composition expressed by the formula $CoAs_3$.

The description given by Scheerer agrees very closely with that of Breithaupt; but not only did he find the mineral in its massive condition, but also as isolated crystals. These had the form of octahedra, modified by the cube dodecahedron and an icositetrahedron: the inclination of the faces of the latter form to the adjacent face of the octahedron, as measured, was 19° 27′, proving clearly that the icositetrahedron was (2 1 1), for which the calculated angle is 19° 28′; the faces of this particular icositetrahedron truncate the edges of the dodecahedron. He further observes that this form (2 1 1) was present on every crystal he had examined. Scheerer notices also the interesting fact that the crystals are often found implanted upon crystals of cobaltite, and states that he had not been able to trace any regularity in the relative position of the crystals of the two species. As the term Tesseralkies might be taken to imply that the mineral crystallises in the form of cubes, he suggests Arsenikkobaltkies as a more appropriate name.

In Haidinger's 'Handbuch der bestimmenden Mineralogie' (1845) the mineral appears under still another title—Skutterudite, suggestive of the locality where the crystals are found.

In Brook and Miller's edition of 'An Elementary Introduction to Mineralogy by the late William Phillips,' published in 1852, the forms presented by Skutterudite are given as (1 0 0), (1 1 0), (1 1 1), and (2 2 1), and the angles according with this description are calculated. It will be observed that the form (2 1 1) described by Scheerer is not mentioned, but that a new form, (2 2 1), a triakisoctahedron, is substituted. In 1862 vom Rath‡, apparently not having referred to the

* "Ueber zwei norwegische Kobalterze von den Skutteruder Gruben," Pogg. *Ann.* vol. xlii. p. 553 (1837).

† Pogg. *Ann.* vol. liii. p. 591.

‡ "Neue Flächen am Tesseralkies," Pogg. *Ann.* vol. cxv. p. 480.

original paper of Scheerer, and assuming that the information contained in the last-mentioned 'Mineralogy' incorporated all the results of observation up to that date, determined anew the form (2 1 1), and also added a triakisoctahedron (3 3 2), a tetrakishexahedron (3 1 0), and a hexakisoctahedron (6 4 3).

The prominent forms, according to both Scheerer and vom Rath, are the octahedron (1 1 1), the dodecahedron (1 1 0), and the icositetrahedron (2 1 1); while, according to the latter mineralogist, the additional forms (3 3 2), (3 1 0), (6 4 3) are only subordinately developed. The disposition of the faces of this hexakisoctahedron (6 4 3) will be more easily imagined, if it be remarked that they cut off the edges of intersection of the octahedron with the tetrakishexahedron (3 1 0). With regard to the faces of the triakisoctahedron, vom Rath observes that they are small and unsuited for measurement, but that in one case he had been able to measure the angle made with the adjacent face of the octahedron as lying between $9\frac{1}{2}°$ and $10\frac{1}{2}°$; whence he concludes that the form is not (2 2 1), as given by Miller, which requires an angle of 15° 48′, but (3 3 2), for which the calculated angle is 10° 2′.

As vom Rath considered it unlikely that the subordinate triakisoctahedron present on some crystals should be (2 2 1) and on others (3 3 2), he suggests that the symbol given by Miller is a mistaken one, and appears to think that the form (2 2 1) may have been determined by inspection and not by measurement. It seems, however, much more probable that the information given by Miller is intended to be merely a statement of the results of Scheerer, and not of a later examination by Miller himself, who could not have failed to remark the existence of the icositetrahedron (2 1 1), which is so characteristic that it was present on every crystal examined by Scheerer, and in fact can be distinguished on every specimen in the collection of the British Museum. It seems fairly evident, then, that Miller's (2 2 1) was a simple error introduced in the copying of Scheerer's results, and that the angles were afterwards calculated without this error in the translation of the symbol from the German notation being remarked. If this hypothesis be correct, (2 2 1) must be expelled from the list of observed forms.

The next mention of Skutterudite is to be found in an extremely interesting and important paper by Schrauf and Dana on the thermoelectric properties of mineral-varieties*, where they remark that, although the mineral undoubtedly crystallises in the cubic system, and has never been observed to

* "Ueber die thermoelektrische Eigenschaften von Mineral-Varietäten," *Sitz.-Ber. Ak. Wien*, vol. lxix. p. 153 (1874).

present any hemihedral development, they nevertheless find some specimens to be thermoelectrically positive and others negative.

The bearing of this remark will be more obvious when it is recalled to mind that the investigation of Schrauf and Dana was undertaken with a view to test the accuracy of the induction made by Gustav Rose from his experiments on iron pyrites and cobaltine*—that the opposition in the thermoelectric behaviour of different specimens of the same mineral is due to the diplohedral hemisymmetry,—and of his inference that the faces observed on the positive and negative crystals belong only apparently to the same, really to complementary, semiforms. The observation of Schrauf and Dana, that the same thermoelectrical peculiarities are shown by a holohedral mineral of the same crystalline system to which iron pyrites and cobaltine belong, will clearly not strengthen the position taken up by Rose.

It is therefore of some interest to find that the specimens in this Collection prove that not only is Skutterudite undoubtedly hemihedral, but that it presents the particular variety of hemisymmetry which is so characteristic of both iron pyrites and cobaltine.

Brezina† has pointed out that the opposition of thermoelectric properties shown by various specimens of iron pyrites and cobaltine cannot be satisfactorily explained by diplohedral symmetry, and holds that haplohedral hemisymmetry must be at the same time present—that, in other words, the crystals must be tetartohedral in structure; and, assuming that the differences in the specimens are only differences of molecular grouping, Brezina regards iron pyrites and the allied species as being really tetartohedral—a view which as yet there is little, if any, crystallographic evidence to support. It seems, however, more probable that Schrauf and Dana are right in attributing this difference, not to crystalline hemisymmetry, but to slight differences of composition or of density; and the force of their argument will not be very much weakened by the reference of the symmetry of the crystalline forms of Skutterudite to a hemihedral type.

It may be useful to remark that attention was first directed to the hemihedry by the discovery of a crystal of Skutterudite among the specimens of that typically hemihedral mineral

* "Ueber den Zusammenhang zwischen hemiëdrischer Krystallform und thermo-elektrischen Verhalten beim Eisenkies und Kobaltglanz," Pogg. *Ann.* vol. cxlii. p. 1 (1871).

† "Ueber die Symmetrie der Pyritgruppe," Tschermak's *Min. Mittheil.* p. 23 (1872).

cobaltine, to which it was very similar in habit, though different in colour. Closer examination, however, made known the fact that the hemihedrally-developed faces belonged, not to the pentagonal dodecahedron $\pi(2\,1\,0)$, so common in both cobaltine and iron pyrites, but to the much rarer form $\pi(3\,1\,0)$. In this Collection there are fourteen more or less perfect crystals, presenting respectively the following development :—

No. 1 shows only the octahedron $o(1\,1\,1)$ modified by faces of the icositetrahedron $n(2\,1\,1)$, as shown in fig. 1 (Pl. X.).

Nos. 2, 3, 4 show also the faces of the dodecahedron $d(1\,1\,0)$, and are represented in fig. 2—each of these four crystals being implanted upon a crystal of cobaltite in the way described by Scheerer and vom Rath. On Nos. 5 and 6 the same forms recur; but some of the quoins are truncated by small planes of the cube $a(1\,0\,0)$, probably due to cleavage.

As in all the above forms the poles of the faces lie in dodecahedral planes, the abeyance of symmetry of the latter would have no effect in reducing the number of faces; in other words, so long as only these forms are present it is impossible to distinguish crystallographically whether the structure is characterized by holohedral symmetry or by diplohedral hemisymmetry.

Nos. 7 to 11 present faces of the tetrakishexahedron $f(3\,1\,0)$; and it now becomes possible to determine whether the internal structure as shown by the external form is to be regarded as holohedral or hemihedral. As a matter of fact, in each of these crystals the faces of only one semiform $\pi(3\,1\,0)$ are found to be present, the number varying with the more or less fractured state of the crystal. This combination is represented in fig. 3, from which, for the sake of simplicity, the small faces of the cube have been omitted. No. 7 presents all the twelve faces required by perfect hemisymmetry; No. 8 shows eight faces, No. 9 shows five, while on Nos. 10 and 11 only two are present; but on none of these crystals can any faces of the complementary semiform $\pi(1\,3\,0)$ be distinguished.

Crystal No. 12 is five eighths of an inch (=1·6 centim.) long, and projects from a matrix of quartz and mica. It is particularly interesting as showing the hemihedral development, not only of the above tetrakishexahedron, but also of an hexakisoctahedron having its planes in the edge-zones of the dodecahedron. The angle made by the faces of this new form with the adjacent faces of the form (2 1 1) was measured by help of the telescopic images to be 10° 48′ in one case and 11° 2′ in another; there is no doubt, then, that the faces belong to the semiform $\pi(3\,1\,2)$, for which the corresponding calculated angle is 10° 54′. Fig. 4 represents the actual development of the various faces on that part of the crystal

which is not concealed by the matrix: no fewer than ten faces of this semiform $\pi(3\,1\,2)$ can be seen, while not a single face of the complementary semiform $\pi(3\,2\,1)$ is to be found.

Gustav Rose has remarked that, in cases of diplohedral hemisymmetry, the poles of all the faces present belong in general to one set of systematic triangles: this crystal of Skutterudite forms another of the rare exceptions to this rule, which would require the association of $\pi(3\,2\,1)$ instead of $\pi(3\,1\,2)$ with the semiform $\pi(3\,1\,0)$.

This semiform $\pi(3\,1\,2)$ is again to be observed on crystal No. 13, though it is there represented by only a single face. The angle with the adjacent face of the form $(2\,1\,1)$ was in this case measured to be $10°\ 52\frac{1}{2}'$, a result according well with the calculated angle $10°\ 54'$: on the same crystal eight faces of the semiform $\pi(3\,1\,0)$ are developed.

The last crystal, No. 14, differs from the rest in showing three faces, which, if the crystal were simple, would undoubtedly be attributed to the complementary semiform $\pi(1\,3\,0)$; the crystal, however, still presents a hemihedral habit, since the three faces of this complementary semiform $\pi(1\,3\,0)$ only appear at quoins where the faces of $\pi(3\,1\,0)$ are missing. It is very possible indeed that the crystal may be twinned about the normal to a dodecahedron-face, as is at times the case in iron pyrites—which theory would likewise account for the presence of reentrant angles, otherwise to be attributed to parallel growth.

The subordinate forms $(3\,3\,2)$, $(6\,4\,3)$, described by vom Rath, do not seem to be present on any of the above crystals, and must be very rare. In some cases indeed the edges of intersection of the octahedron with the form $\pi(3\,1\,0)$ are "rounded off" by very small faces not susceptible of measurement; while the edges of the octahedron itself are bevelled by narrow planes, which, if crystal-faces at all, can only be approximately determined. On one crystal the angles made with the adjacent octahedral faces by the four tautozonal rudimentary planes of the triakisoctahedron were measured by the method of maximum illumination as $18\frac{1}{2}°$, $17\frac{1}{4}°$, $13°$, $16\frac{1}{2}°$ respectively—thus suggesting the form $(2\,2\,1)$, which requires an angle of $15°\ 48'$, and, as we have seen, was given by Miller, no doubt in mistake, as an observed form. On another crystal a series of images could be obtained from each of two faces of the triakisoctahedron; and the limiting-values thus determined for the angle corresponding to the one just mentioned were, in one case $13\frac{1}{2}°$–$16\frac{1}{4}°$, and in the other $15\frac{3}{4}°$–$16\frac{1}{2}°$; on a third crystal two similar angles were measured at $15\frac{1}{4}°$ and $19°$, thus again indicating the form $(2\,2\,1)$.

The range of specific gravity of the crystallised mineral is a little wider than that of the massive as determined by Breithaupt: one crystal presenting no external sign of impurity was found to have a specific gravity of 6·48; a second had a specific gravity of 6·37, but, like the less dense specimens of Breithaupt, evidently contained foreign matter. Two other fairly large crystals had specific gravities of 6·56 and 6·72 respectively; while a small apparently very pure crystal, with smooth lustrous faces, gave a result as high as 6·86.

LXIII. *Action of Free Molecules on Radiant Heat, and its Conversion thereby into Sound.* *By* JOHN TYNDALL, *F.R.S.*

[Concluded from p. 462.]

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§ 5. *New Experiments. Hypothesis of Internal Films.*

a. *The Experimental Tube.*

I WISH now to come to closer quarters with the argument urged by Magnus and repeated by MM. Lecher and Pernter, namely that my results were due to "vapourhesion"—that is to say, to liquid films condensed on my experimental tube and on my plates of rock-salt. The two investigators last named express unqualified surprise that I should have neglected the simple precaution of experimenting with blackened tubes. But this precaution was by no means neglected by me. I have repeatedly fortified myself by experiments of this character, with the result recorded in Table I. But I went further. A smooth coating of lampblack, however powerful as an absorber, might be competent to reflect a certain portion of the incident heat. Hence my desire to get entirely rid of reflection, by avoiding all contact with the interior surface of the experimental tube.

In fig. 3 an apparatus is sketched, by means of which this has been accomplished. T T′ is a stout tube of brass, 36 inches long and 6 inches internal diameter. Projecting from

its otherwise closed ends are screws 1 inch each in depth, surrounding circular apertures $3\frac{1}{8}$ inches in diameter. By means of screw-caps these apertures may be closed air-tight by trans-

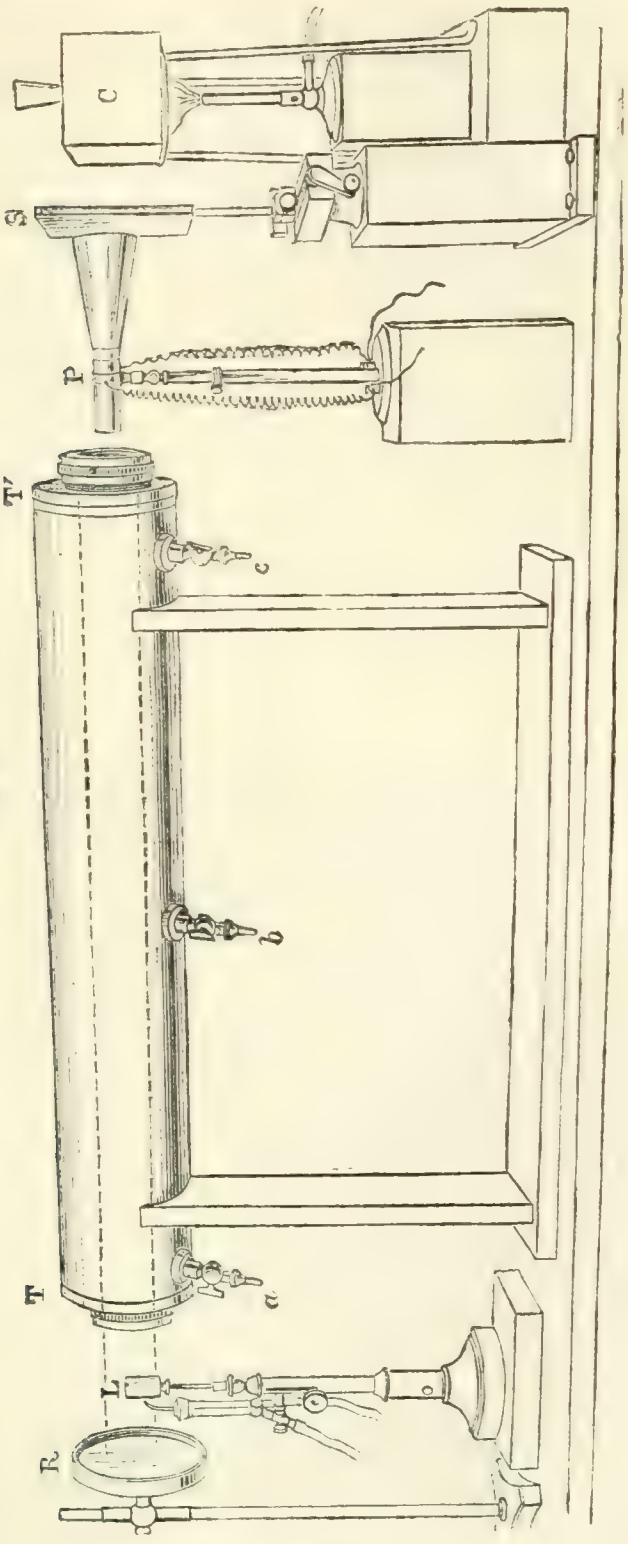


Fig. 3.

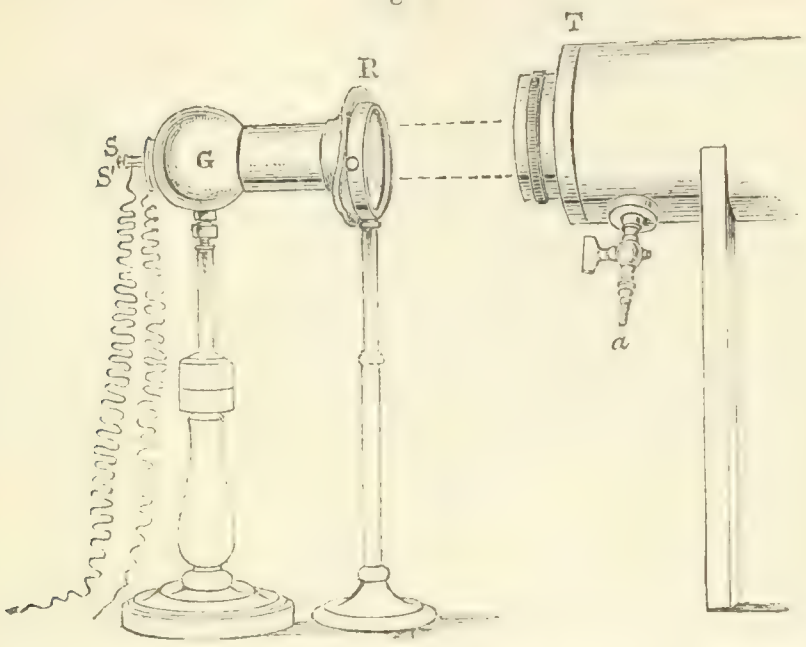
parent plates of rock-salt, the plates being protected from crushing by washers of indiarubber. *a* is a cock leading to an air-pump and a barometer-tube; *b* is a cock to which flasks can be attached; while *c* is a cock connected with a purifying-apparatus (not shown). This consists of a U tube filled with fragments of clean glass moistened with sulphuric acid, and of a second tube filled with fragments of Carrara marble wetted with caustic potash. A plug of cotton-wool intercepts the floating matter of the air. The source of heat L is a cylinder of carefully prepared lime, against which a flame of coal-gas and oxygen impinges. The incandescent lime faces the concave mirror R, which receives the rays and sends them back in a parallel beam through the tube T T′. P is the thermopile, from which wires proceed to a distant galvanometer, the reflected dial of which is observed through a telescope. S is an adjusting-screen. At C is what, in my former researches, I have called the "compensating cube," used to neutralize the radiation from the source L and to bring the galvanometer-needle to zero. The chief object of this arrangement is to enable the experimenter to send a calorific beam along the axis of the tube T T′ which shall never touch its internal surface. The great width of the tube, aided when necessary by diaphragms, renders this easy of accomplishment.

The mode of operation is readily understood. Suppose the heat impinging on the pile to produce a galvanometric deflection of 50°. This, the "total heat," is neutralized by the radiation from the compensating cube. To render the compensation accurate, the double screen S is shifted by an extremely fine screw-motion. Even when the total heat is very large it is possible, by means of this screen, to neutralize accurately the radiation of the source and bring the needle to zero. Suppose it to be there when the tube T T′ is exhausted; on permitting a gas or vapour capable of absorbing radiant heat to enter T T′, the preexisting equilibrium is destroyed, the needle moves promptly away from zero, and from the observed deflection the absorption may be calculated.

Instead of the concave reflector, a rock-salt lens of great purity is sometimes used to render the beam parallel. When the lens is used, the incandescent portion of the cylinder of lime is caused to face the tube T T′, between which and the lime the lens is introduced. Instead of the incandescent lime, a spiral of platinum-wire heated to redness by a voltaic current is frequently employed as the source of heat. The spiral is placed in the centre of a brass globe G, fig. 4, and is connected with the screws S S′ fixed in the back of the globe.

from which wires pass to the battery. R shows the position of the rock-salt lens, while one end of the experimental tube is shown at T. The greatest care is here necessary to protect the spiral from any agitation of the air. Such care, however,

Fig. 4.



it is always in the experimenter's power to bestow. To secure constancy in the radiation, the battery was charged three times a day, the strength of the current being regulated by a rheostat, and checked by a tangent-galvanometer.

An experimental tube of the description shown in fig. 3 was first employed by me about twelve years ago; and with it I then verified all the experiments I had previously made on the absorption of radiant heat by gases and vapours. The experiments were not published in any scientific journal; but they are thus referred to at page 394 of my volume of collected memoirs, entitled 'Contributions to Molecular Physics in the domain of Radiant Heat:'—"The two ends of an experimental tube, 38 inches long and 6 inches in diameter, were each provided with an aperture 2·6 inches in diameter. These apertures were closed with plates of rock-salt. The source of heat was a platinum spiral, well defended from air-currents, and heated to redness by an electric current. In front of the spiral was a rock-salt lens, which sent a slightly convergent beam through the tube. Behind the most distant plate was formed a sharply-defined image of the spiral, its size being such *that it was wholly embraced by the plate of salt*. Here, then, was a beam of heat passing through an experimental

tube without coming into contact either with the surface of the tube itself, or with any coating or lining of that surface. With this apparatus all my old experiments on vapours have been frequently repeated. There is no substantial difference between the results thus obtained and those obtained with an experimental tube where nineteen twentieths of the heat which reached the pile was reflected heat."

The tube referred to in this extract was of rough brass, tarnished within. Hence, when air entered it, after exhaustion, the dynamic heating of the tube and the partial condensation of the vapour when the air was moist, produced an amount of radiation from its internal surface which, though small, was a source of some disturbance. In my present experiments, therefore, another tube, of the dimensions above given, is employed; and to reduce to a minimum any radiation from its internal surface, it is coated within with silver deposited electrolytically, and highly polished. Experiments with this tube show that it does not in any way disturb the true radiation from the source.

In Table I. are recorded five series of measurements executed with a brass tube polished within. But as the liquids, though reported pure by the manufacturing chemist, vary slightly from time to time, I thought it advisable to adhere to the same samples in experiments wherein a tube with reflecting interior is to be compared with one permitting of no reflection. The following five series of new measurements were therefore executed with the tube first mentioned.

Table III.

Brass tube with internal reflection.

	Pressures.	A Spiral dark.	B Spiral bright.	C Lime, free.	D Lime, mirror.	E Lime, lens.
Bisulphide of carbon .	·48	5·0	3·1	3·4	3·1	2·5
Chloroform	·36	7·9	5·3	5·6	4·8	5·0
Benzol	·32	9·6	8·6	7·9	6·9	6·3
Iodide of methyl . . .	·46	12·1	8·8	10·2	8·1	7·5
Iodide of ethyl . . .	·36	15·0	11·9	12·5	11·3	10·6
Amylene	·26	21·9	15·6	16·0	14·4	12·5
Sulphuric ether . . .	·28	30·9	23·1	22·7	19·8	18·1
Acetic ether . . .	·29	36·9	27·3	29·6	24·1	...
Formic ether	·36	...	...	29·6	25·0	23·8

Column A in this table contains the absorptions of the respective vapours, in hundredths of the total radiation, when the source was a platinum spiral just under incandescence, unaided by either lens or mirror. Under B are the absorptions when the source was the same spiral heated to bright redness.

The same vibrating atoms are preserved; but in A they vibrate on the whole more slowly than in B; and as the atomic periods of the transparent vapours synchronize best with the slower oscillations, the absorptions in A are considerably higher than those in B. Under C are the absorptions when the source was a moderate lime-light, produced by coal-gas and oxygen. Under D are the absorptions when the source was the lime-light with its heat gathered up and sent through the experimental tube by a concave mirror; while under E are the absorptions of the heat of the incandescent spiral aided by a rock-salt lens. The absorptions in D and E are somewhat less than those in C, because the path of the rays was diminished, through the reduction of internal reflection by the mirror and lens.

With the foregoing results, obtained with the brass experimental tube polished within, where the greater portion of the heat reaching the pile had undergone reflection, are now to be compared those obtained with the silvered tube, where internal reflection was wholly avoided.

Table IV.

Wide silvered tube with no reflection.

	L A Spiral dark.	L B Spiral bright.	M C Lime bright.	M D Lime moderate.	L E Lime dull red.
Bisulphide of carbon . .	4·0	1·8	1·5	1·5	3·3
Chloroform	5·6	3·0	3·5	4·3	6·0
Benzol	7·0	5·1	4·4	6·5	8·0
Iodide of methyl . . .	7·5	5·9	5·0	6·5	9·2
Iodide of ethyl	11·3	7·6	6·8	8·5	11·2
Amylene	15·0	11·8	8·8	12·0	17·3
Sulphuric ether	21·2	17·1	12·5	15·0	25·0
Acetic ether	26·0	20·0	16·3	24·0	32·0
Formic ether	27·0	21·0	17·0	25·0	34·0

The vapour-pressures here were those given in Table III. Column A contains the absorptions measured when the source of heat was a spiral under incandescence, aided by a rock-salt lens. B contains the absorptions measured with the same arrangement, the spiral being raised to bright redness. As usual, the heat of lowest refrangibility is most absorbed. C contains the absorptions of a tolerably intense lime-light aided by the silvered mirror; D the absorptions of the same light with its intensity reduced; while E contains the absorptions of the rays from a lime-cylinder heated to dull redness by a steady flame of hydrogen, burning in air, and aided by a rock-salt lens.

Here, then, where no trace of reflected heat is sent to the pile, we have substantially the same results as those obtained when by far the greater portion of the heat reaching the pile had undergone reflection. The agreement shows that the films, the action of which had been posited, but never proved, are, in regard to this action, what I always knew them to be, imaginary.

b. *The Plates of Rock-salt.*

Having thus, I trust effectually, disposed of the hypothesis which ascribed my results to liquid layers covering the interior surface of the experimental tube, I proceed to the examination of the plates of rock-salt, which have also been credited with a liquid deposit. The hypothesis is to a great extent disposed of in a paper of mine published in the Philosophical Transactions for 1864, which, like other papers, has been overlooked by writers on this question. An apparatus is there described which enabled me to operate upon gaseous strata of different thicknesses, the thickness in each case being measured with great accuracy by aid of a vernier. Let two polished plates of rock-salt be supposed in close contact with each other, and let them be gradually separated by a suitable rack-and-pinion movement. From the first moment of their separation let the space between them be copiously supplied with vapour. Liquid films, if they form at all, will be deposited when the distance between the plates of salt is but small. As far as the films are concerned, they will be as influential when the plates are $\frac{1}{20}$ of an inch apart as when they are 2 inches apart. Hence, if the hypothesis of my opponents be correct, the absorption ought to declare itself with the former amount of separation as clearly as with the latter. But if, as I allege, the absorption be the act of the vapour molecules, then the deepening of the vapour stratum will be accompanied by an increase of the absorption. The following experiment, bearing directly on this subject, was executed in 1864. My plates of salt were first fixed at a distance of $\frac{1}{20}$ of an inch asunder; and the space between them was copiously supplied with air saturated with sulphuric-ether vapour. The distance between the plates of salt was then augmented by steps, as shown in the following table; and at each step the absorption was determined. Here are the results:—

Thickness of vapour stratum.	Absorption.
0·05 inch	2·1 per cent.
0·1 „	4·6 „
0·2 „	8.7 „
0·4 „	14·3 „
0·8 „	21·0 „
1·5 „	34·6 „
2·0 „	35·1 „

We thus see that when the depth of the vapour stratum augments from $\frac{1}{20}$ of an inch to 2 inches, the absorption augments from 2 per cent. to 35 per cent. of the total radiation.

It is only with sulphuric ether that an absorption of 2 per cent. by the thinnest stratum could be obtained. With most other vapours the action of a layer $\frac{1}{20}$ of an inch in thickness proved insensible. We may thus begin with a layer which yields an absorption immeasurably small, gradually augment the thickness of the absorbing layer until it becomes a column 38 inches long, and find, throughout, the increase of absorption running hand in hand with the increase of the length of the absorbent layer. This result is utterly irreconcilable with the hypothesis that liquid layers on the plates of salt played any important part in my experiments.

With regard to "vapour-hesion," I have to make the following further remarks. Let a thin plate of polished rock-salt be placed upon, or against, the face of the thermopile, with its lampblack removed, so as to expose a clean metallic surface. Let an open glass tube end about a quarter of an inch above the plate of salt; and through this tube let a current of mixed air and vapour be gently urged downwards against the plate. No fine spray, which might readily arise from bubbling through a liquid, is to be permitted to mingle with the vapour. My vapours, I may say, have been usually formed, without bubbling, in large flasks, each containing a portion of a volatile liquid, the vapour of which was permitted to diffuse in the air of the flask. Fletcher's foot-bellows were employed to urge the vapours forward. The needle being at zero, or thereabouts, on causing the mixed current to impinge upon the rock-salt, the needle immediately swings aside, the deflection varying from 20° to 80° and more, according to the delicacy of the galvanometer and the quality of the vapour.

From such experiments, which prove heat to be liberated when vapour comes into contact with the salt, the condensation of the vapour has been inferred. I accept the inference.

This, however, does not imply the acceptance of the inference from this inference, that the condensed films exert the action ascribed to them on radiant heat. To test whether they do so or not, I had a circular plate of rock-salt mounted on its edge, and so placed that a beam of heat passed through it normally to a distant thermopile. The source of heat was an incandescent spiral, aided by a concave mirror; and the total radiation, after passing through the salt and impinging on the pile, produced a deflection of 51°. This radiation was accurately neutralized by a compensating cube, the needle under the operation of the two opposing forces pointing to 0°. By an arrangement which every experimenter can imagine for himself, sheets of air, laden with various vapours, could be poured in succession over the plate of salt. If, under these circumstances, absorbent films were formed, the equilibrium would be destroyed and the needle would move from 0°. My slit, which was the flattened base of an open tin cone, was placed across the upper part of the plate of rock-salt, so that the vapours, which were all heavier than air, should fall over the plate downwards. Here are the results obtained when the following vapours were permitted to form films upon the plate:—

Bisulphide of carbon . . .	A barely sensible action.
Chloroform	No action.
Benzol	No action.
Iodide of methyl	No action.
Iodide of ethyl	No action.
Amylene	No action.
Sulphuric ether	A barely sensible action.
Acetic ether	No action.
Formic ether	No action.
Alcohol	No action.

The minute deflections produced by bisulphide of carbon and sulphuric ether had nothing to do with liquid films; so that the words "no action" might have been written against these substances as against the others. While therefore recognizing the fact of condensation, these simple experiments prove how incorrect it is to credit the condensed films with the effects which have been ascribed to them. According to Magnus, alcohol exhibits a force of vapour-hesion particularly strong; but we here see that even this vapour produces no sensible effect. The vapours have been tested with other sources of heat, with the same result.

The deportment of dry and humid air is, as usual, very instructive. When the thin plate of rock-salt resting on the

naked face of the pile was exposed to the common air of the laboratory, it, of course, contracted a film corresponding to the humidity of the air. The sweeping away of this film by dry air produced a very small deflection indicating cold. When, on the contrary, humid air was urged against the salt, the deflection indicating heat was prompt and large. We have now to test the action of the liquid film thus formed upon radiant heat. The circular plate of rock-salt before referred to was mounted, with the incandescent spiral on one side and the pile on the other. The slit was so arranged that dry and humid air could be sent in succession over the surface of the plate. As before, the radiation from the spiral was neutralized by the compensating cube, the needle pointing to zero when the two sources were in equilibrium. On sweeping dry air over the plate of salt, so as to remove the film contracted from the laboratory air, there was no motion of the needle. On pouring humid air from the slit over the salt, there was no motion of the needle. To moisten the air it was urged from bottom to top through a tall jar filled with wet bibulous paper. The same apparatus had furnished humid air which produced a swing of 80° when urged against the thin plate of rock-salt resting on the face of the pile. As a barrier to radiant heat it was nevertheless powerless. The condensation may even be considerably enhanced without producing any sensible effect. Through a glass tube I urged my breath against the plate of salt so as to produce the colours of thin plates, without any sensible effect upon the galvanometer.

Having thus clearly shown what films, even in an exaggerated form, cannot accomplish, I hope, in testing the action of humid air, that the use of the silvered tube, closed with polished plates of rock-salt, on which no trace of visible moisture is deposited, will be conceded to me. This tube involves the use of a source of heat of small dimensions. The lime-light, though fulfilling this condition, is not suitable, because of the high refrangibility of its heat. The incandescent platinum spiral would be better; still the radiation from this source is but feebly absorbed by the aqueous vapour taken up by air at ordinary temperatures. The oxyhydrogen flame fulfils the required conditions best*. It has the advantage of high temperature and low refrangibility; while the fact

* I was careful to assure myself that, unless it amounted to the visible wetting of the plates of salt, there was no sensible stoppage of the rays from the oxyhydrogen flame. This quite agrees with the result obtained by Magnus himself, in the experiment with the concave mirror already referred to. Unless he visibly wetted the mirror, he failed to impair the energy of the reflected beam.

that its heat, as coming from aqueous vapour, is absorbed with special energy by aqueous vapour, is also a strong recommendation. In the Bakerian Lecture for 1864 I have illustrated this point. It was then shown that when a platinum spiral, rendered incandescent by a voltaic current, had 5·8 per cent. of its heat absorbed by undried air, a hydrogen-flame had from 17 to 20 per cent. of its heat absorbed. The mere plunging of a platinum spiral into the flame caused the absorption to fall from 17 per cent. to 8·6 per cent. Hence my reason for choosing a hydrogen-flame in the present instance.

Dry air and humid air being caused to occupy the experimental tube in succession, both of them were compared with the radiation through the tube when very perfectly exhausted by a Bianchi's pump. The following results were obtained :—

	Deflection.	Absorption per 100.
Vacuum	0°·0	0·0
Dry air	0°·0	0·0
Humid air	8°·0	11·7
Vacuum	0°·0	0·0
Dry air	0°·0	0·0
Humid air	7°·6	11·2

The deflection through a vacuum, otherwise the total heat, was 47°, which, according to my calibration-table, is equivalent to 68 units.

A fresh supply of hydrogen was here introduced into the gas-holder. The total heat being 46° or 65 units, the follow-results were obtained :—

	Deflection.	Absorption per 100.
Dry air	0°·0	0·0
Humid air	6°·2	9·5
Dry air	0°·0	0·0
Humid air	6°·7	10·3

Mean of the four determinations with humid air, 10·7.

A column of humid air 38 inches long absorbs, according to these experiments, 10·7 per cent. of the radiation from a hydrogen-flame. I have been criticised for estimating the absorption of the earth's rays within 10 feet of the earth's surface at 10 per cent. This estimate I consider a moderate one; and the foregoing experiments prove it to be so.

It would be an error to suppose that determinations like these are easily made. They require the most scrupulous care for their successful accomplishment. The hydrogen issued

from the gas-holder through an orifice of fixed dimensions in a stream of the utmost possible constancy. It was then led into a Sugg's regulator, whence it issued under an absolutely constant pressure. The flame issued from a circular brass burner with an aperture $\frac{3}{16}$ of an inch in diameter. It was carefully surrounded by a hoarding, the space within the hoarding being packed with horsehair. Every precaution in fact was taken to avoid the agitation of the air around the flame. Proper care was also taken to secure the pile against disturbance by air-currents. The air being first purified, by passing it through caustic potash and sulphuric acid, was rendered humid by carrying it over wet bibulous paper contained in a suitable tube. It required some minutes to enter; and it was therefore necessary, by prior patient observance of the needle, to make sure that during this interval no change occurred in the radiation save that effected by the humid air itself. The humid air was removed from the experimental tube, not by exhaustion (which always causes precipitation), but by gently forcing, by means of a compressing-pump, dry air through the tube. When this was done, the needle, in all the experiments above recorded, returned within a small fraction of a degree to zero*.

With the rough, wide experimental tube to which reference has already been made, I, ten years ago, found the absorption of a column of humid air 38 inches long to be 8 per cent. of the total radiation from a flame of hydrogen.

§ 6. *Conservation of Molecular Action.*

If the absorption of radiant heat be the act of the constituent atoms of compound molecules, its amount depending solely on the number of molecules encountered by the calorific waves, then, whatever may be the changes of density which gases and vapours undergo, so long as the number of molecules remains the same, the absorption ought to continue constant. Such constancy, should it be proved to exist, I name the "conservation of molecular action." The experiments now to be described deal with this question.

Besides the silvered experimental tube already described as 38 inches long, I had another constructed of the same diameter, and with similar terminal apertures. Its length was 10·8 inches. The one tube was, therefore, 3·5 times the length of the other. The shorter tube was constructed with the view of proving that the absorptions I had recorded in previous

* The temperature of the laboratory air during the foregoing experiments was 60° Fahr.

memoirs were exerted by the vapour molecules, and not by liquid layers deposited on my plates of salt. The hypothesis of such layers, however, being completely disposed of, we can carry the experiments a step further. Assuming that the absorption does not change with change of density, so long as the quantity remains constant, it would follow that 1 mercury inch of vapour in the long tube ought to quench as much heat as 3·5 inches in the short one. The same conclusion ought, of course, to hold good when we compare 2 inches of vapour in the long tube with 7 inches in the short one.

The experiments have been made, and with the following results :—

Sulphuric-ether vapour.

Short experimental tube.

Pressure.	Deflection.	Absorption per 100.
3·5 inches . .	24°	30
7 „ . .	30	37·5
Totat heat .	50	

Again,

3·5 inches . .	24°	30
7 „ . .	31·5	39·4
Total heat .	50	

Taking the means of these two experiments, we have the absorptions—

For 3·5 inches . . . . 30 per cent.
For 7 „ . . . . . 38·5 „

Such is the result obtained with the short tube. We now turn to the long one :—

Long experimental tube.

Pressure.	Deflection.	Absorption per cent.
1 inch. . . .	23°	30·3
Total heat .	49	
2 inches . . .	31	38·8
Total heat .	50	

These results are almost identical with those obtained with the short tube and greater pressures.

The source of heat in this case was a dull lime-light. In subsequent experiments the light was brightened. Here is the result :—

Long experimental tube.

Pressure.	Absorption.
1 inch	22·3 per cent.
2 inches	29·5 „

Short experimental tube.

Pressure.	Absorption.
3·5 inches	22·5 per cent.
7·0 „	30·0 „

The agreement here is almost as close as that established by the first experiment.

With a still brighter source of heat the absorptions were:—

Long tube.

Pressure.	Absorption.
1 inch	18·4 per cent.
2 inches	25·7 „

Short tube.

3·5 inches	18·8 per cent.
7·0 inches	25·6 „

When, therefore, the density of the vapour varies inversely as the length of it traversed by the calorific rays, the absorption remains constant.

The hydride of amyl lends itself conveniently to experiments of this character. With it, and with the lime-light as source, the following measurements have been made:—

Hydride of amyl.

Long experimental tube.

Pressure.	Deflection.	Absorption.
1 inch	10°	12·8
2 inches	15	19·2
Total heat .	49·4	

Short experimental tube.

Pressure.	Deflection.	Absorption.
3·5 inches	10°	12·2
7·0 „	15	18·3
Total heat .	50·5	

The agreement here is close enough to illustrate the law, the greatest difference being under 1 per cent.

At this point the following entry appears in my note-book:—"It might have been wise on my part to rest content with the comparison of the long and short tubes with the lime-light as source of heat. But for the sake of completeness I wished to introduce the incandescent spiral. The fulfilment of this wish—that is to say, the successful performance of a single experiment—has cost me more than a week's work. Once, however, committed to the task, I could not leave it incomplete.

"The discrepancy between the two tubes was in no case great, hardly ever exceeding 2 per cent. But the difference was uniformly in favour of the long tube and small density. Diaphragms were employed; the position of the short tube was shifted; and it was finally placed so that the pile should occupy the same position in relation to its adjacent end as it did in relation to the adjacent end of the long tube. The discrepancies then disappeared, the absorptions in the two tubes proving practically identical.

"Many leaves of paper were covered with observations during the week; but it is useless to take up time and space in copying them here. One representative observation will suffice."

Sulphuric ether.

Source of heat—bright-red spiral with rock-salt lens.

Long experimental tube.

Pressure.	Deflection.	Absorption.
1 inch	20°·0	23·5
2 inches	27°·3	32·1
Total heat .	51°·0	

Short experimental tube.

Pressure.	Deflection.	Absorption.
3·5 inches	17°·8	23·4
7·0 „	24°·8	32·6
Total heat .	49°·0	

The agreement between the two tubes is as perfect as could be desired.

It is easy to record these experiments; but it is not so easy to make them. On every portion of the apparatus (the source, the tube, the thermopile, and the galvanometer) extraordinary care must be bestowed to make the experiments strictly comparable. The results were checked by taking the total heat after every experiment—a precaution which ensured the detection of any variation on the part of the source. When the platinum spiral was used, the battery of ten Grove's cells, employed to render it incandescent, had to be very carefully prepared, freshly amalgamated zinc plates being used in each fresh battery. The requisite deflection of a tangent compass being produced, it was kept constant by means of a rheochord throughout the day. When the battery showed signs of rapid falling, it was always renewed. It would be tedious to dwell upon the precautions taken to protect the source and the pile from the least agitation of the air. Such precautions are essential; but their necessity and form must be learnt by each experimenter for himself.

§ 7. *Thermal Continuity of Liquids and Vapours.*

I have amply illustrated by experiments, recently made, the correspondence which subsists between vapour absorption and liquid absorption, when the quantities of matter traversed in the two cases by the calorific rays are proportional to each other. This correspondence, as I have already stated, was established eighteen years ago. And though the result goes to the very core of the discussion which my researches have aroused, though in relation to that discussion they had, in my estimation, a weight and import greater than those of any other experiments published by me, they seem never for a moment to have attracted the attention of those who have taken part in the discussion. Here is the result, published in 1864, which illustrates the point now under consideration :—

		Absorption per 100.	
		Vapour.	Liquid.
Bisulphide of carbon . .	0·48	4·3	8·4
Chloroform	0·36	6·6	25·0
Iodide of methyl	0·46	10·2	46·5
Iodide of ethyl	0·36	15·0	50·7
Benzol	0·32	16·8	55·7
Amylene	0·26	19·0	65·2
Sulphuric ether	0·28	21·5	73·5
Acetic ether	0·29	22·2	74·0
Formic ether	0·36	22·5	76·3
Alcohol	0·50	22·7	78·6

The magnitude of the absorption in the liquids is far greater than in the vapours, because the quantity of absorbent matter is far greater in the former than in the latter ; but the order of absorption is the same.

When the vapours are doubled in quantity, the absorptions are considerably increased. When trebled they are still further augmented ; in other words, they approach more and more in magnitude to the absorptions of the liquids ; but the harmony as regards order is never disturbed. What, then, would occur if the vapours were so increased as to render the quantities of matter in the two states, not proportional, but *equal* to one another ? This is the question with which I now propose to deal. At the time when the results above recorded were obtained, I thought it probable that if a circular liquid layer of a given diameter could be vaporized in a tube of the same diameter, the absorption would remain unchanged. In other words, I thought that the liberation of the molecules from liquid cohesion would neither augment nor diminish their

action upon radiant heat. Since 1864 this problem has been often in my mind. The wide silvered tube, I am happy to say, has rendered the solution of the problem possible.

It is only highly volatile liquids that lend themselves to this experiment, because from them alone can vapours be be derived of sufficient density to produce liquid layers of practicable thickness. On the 22nd of last October the experiment was first attempted. The source of heat was the lime-light, the rays of which were received by a concave mirror silvered in front, and sent in a nearly parallel beam through the experimental tube. At the end nearest the source the tube was provided with a diaphragm having a circular orifice 1 inch in diameter. At the other end was a diaphragm with an orifice $\frac{1}{2}$ an inch in diameter. Beyond this was placed the thermopile, furnished, not with its reflecting cone, but with a tube of brass (shown in fig. 3) 2 inches long and blackened within. In this arrangement, the heat which reached the pile did not even approach the interior cylindrical surface. The total heat employed produced a deflection of 60 galvanometric degrees, which, when the tube was exhausted by a powerful Bianchi's air-pump, was accurately neutralized by a compensating cube. Liquid sulphuric ether was then placed in a large flask provided with a sound stopcock, the object being to expose a considerable evaporating surface. The flask was plunged in water, with the view of keeping the liquid and its vapour at an approximately constant temperature. The air being carefully removed from the flask, it was attached to the experimental tube, and a quantity of vapour was allowed to enter sufficient to render a column 38 inches long equivalent to a liquid layer 1 millimetre in thickness. Two concurrent experiments made the deflection produced by the vapour

$$41^\circ.$$

Without altering the quality of the heat, the absorption exercised by a liquid layer of sulphuric ether 1 millimetre thick was next determined. The rock-salt cell with which the experiment was made is described in detail in the Bakerian Lecture for 1864 (Phil. Trans. vol. 154. p. 328.) The annexed figure (fig. 5) will give a sufficiently clear notion of its construction and disposition. Between two stout plates of brass, *c c* and its fellow, two rock-salt plates of extreme purity are firmly clasped by suitable screws, due care being taken to protect the plates from a crushing pressure. The two brass plates referred to are perforated by circular apertures, as shown in the figure. The two plates of salt are not allowed to come into contact, but are separated from each other by a carefully

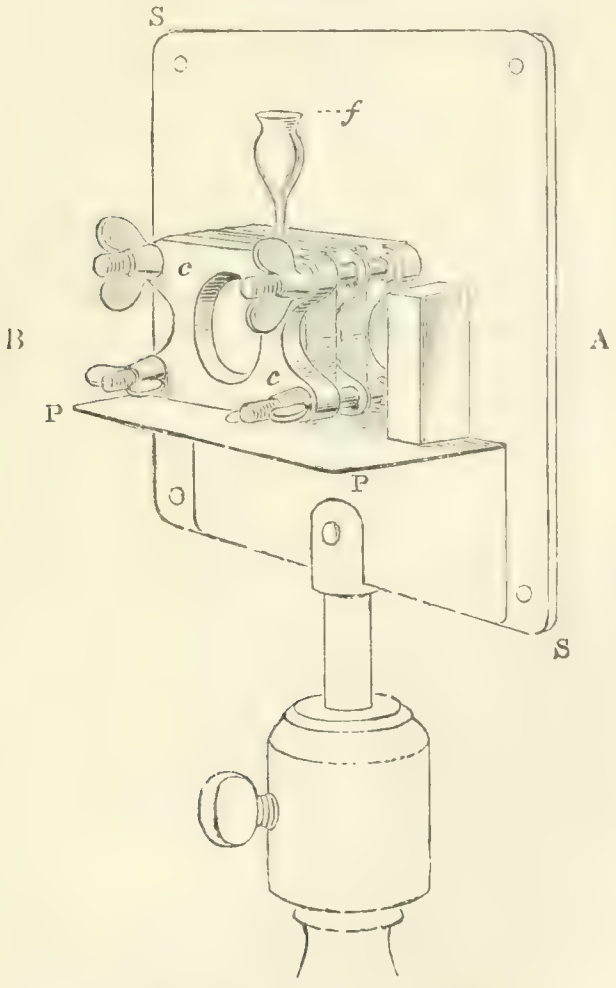
worked brass plate 1 millimetre thick, perforated like *c c*. A portion of this middle plate is cut away, opening a passage into the interior of the cell. Through this passage the cell is filled with liquid by means of the funnel *f*. The cell is placed on a platform P P riveted onto the double brass screen S S. The source was placed at A, and the thermopile with its blackened tube at B. It is not necessary to figure them.

The needle being brought accurately to zero by the compensating-cube when the cell was empty, liquid sulphuric ether was poured in. The consequent deflection was

42°.

Here vapour absorption and liquid absorption were so nearly alike as to stimulate me to further efforts. The entry in my

Fig. 5.



note-book on the 22nd ends with the remark :—"I purpose repeating this experiment on Monday, with the view of rendering the result secure."

On Monday, accordingly, the experiments were resumed; but they proved by no means so easy as I had hoped to find them. For five days I worked at the subject without coming to any satisfactory conclusion. The source, the mirror, the cell, the experimental tube, and the thermopile were all in turn objects of scrutiny; but there still remained a difference between the action of the liquid and that of its vapour sufficient to throw doubt on the assertion of their identity.

At the conclusion of many trials and precautions I found the absorption of the vapour still distinctly in excess of that of the liquid. I had reduced the spherical aberration to a minimum, by confining the reflection to a small central area of the silvered mirror. Still the image of the incandescent lime, formed at the end T′ of the experimental tube, was large enough to encroach a little on the annular space surrounding the aperture closed by the plate of rock-salt. Diaphragms of polished metal had also been used, to lessen the amount of heat falling upon the pile. I figured to myself the heat impinging on the annular space and diaphragm, reverberated back to the end T of the experimental tube, reflected from the annular space and diaphragm at that end, and thus in part sent back to the pile. Such heat, instead of passing once through the vapour, would pass through it three times; and if it formed a sensible part of the total heat, might make vapour absorption appear greater than liquid absorption. I had the tube dismounted, and the annular spaces and diaphragms carefully coated with lampblack. Remounting the tube and measuring once more the vapour absorption, it was found to be

32·4 per cent.

This was the mean of five concurrent series of observations, in which every care was taken to ensure exactitude. Lest the total heat should vary during the execution of a series, it was taken at the conclusion of every individual experiment.

The absorption of sulphuric ether, acting as a liquid, was next determined. The mean of three series of experiments, two of which yielded identical results, and the third of which differed only 0·7 per cent. from the others, made the absorption of the liquid ether to be

32·9 per cent.,

which, I need not say, is surprisingly close to the vapour absorption, differing therefrom by only 0·5 per cent.

Informed by experiment that the heat from the incandescent platinum spiral was more powerfully absorbed than that of the lime-light, I thought it worth while to inquire whether the

liquid followed its vapour in absorptive energy when the quality of the heat was changed. On Monday the 31st, accordingly, the rays from the spiral being rendered parallel by a rock-salt lens, the absorption of sulphuric ether was determined, and found to be

66·7 per cent.;

while the absorption of the liquid was

67·2 per cent.,

which again gives a difference of only 0·5 per cent.

On another occasion I found the absorption by sulphuric ether to be :—

Vapour	71	per cent.
Liquid	70	„

On the 1st November I checked the result obtained with the lime-light and mirror, by using the lime-light and rock-salt lens. Here are the absorptions of the vapour and liquid respectively :—

Vapour	33·3	per cent.
Liquid	33·3	„

The absorptions are identical ; while the result agrees closely with that obtained with the concave reflector.

When the requisite quantity of sulphuric-ether vapour, viz. 7·2 mercury inches, was in the experimental tube, I tried whether the radiation from a Leslie's cube, coated with lamp-black and filled with boiling water, could pass through the vapour. About 14 per cent. of the incident heat was transmitted. Had I been asked at the time whether a liquid layer of sulphuric ether 1 millimetre thick was pervious to the heat of the cube, I should have replied with some confidence in the negative. Hence, for the moment, I thought the experiment opposed to the law that vapour absorption and liquid absorption, when equal quantities of matter are compared, are the same. On actually testing a layer of the liquid ether 1 millimetre thick, the transmission of upwards of 6 per cent. of the incident heat was observed. So that in this case also we have harmony of deportment between liquid and vapour. The absorption of the vapour exceeds that of the liquid, because the heat from the cube radiated freely against the interior silvered surface of the experimental tube, and by its reflection from that surface had its path through the vapour augmented in length. This augmentation naturally carried with it an increase of the absorption.

The next substance examined was hydride of amyl, the boiling-point of which is 30° Fahr., or 5° lower than that of

sulphuric ether. When a large evaporating-surface is exposed, there is therefore no difficulty in obtaining, from this liquid, vapour of a pressure of 6·6 inches of mercury. This, in a tube 38 inches long, would, if squeezed to liquefaction, produce a layer 1 millimetre thick. Vapour absorption and liquid absorption being measured in succession, this is the behaviour of the hydride of amyl:—

Absorption by vapour	51 per cent.
Absorption by liquid	51 „

the two absorptions being absolutely identical*.

Combining this section and the last, their joint results may be thus summed up. Beginning with a column of sulphuric-ether vapour 38 inches long at 7·2 inches pressure, or with a column of hydride-of-amyl vapour 38 inches long and at 6·6 inches pressure, and gradually shortening the column without altering the quantity, the vapour would gradually augment in density and pass wholly, when reduced to a thickness of 1 millimetre, into the liquid state of aggregation. Suppose a beam of heat of constant value, after passing through the vapour, to impinge upon a thermopile and to produce a definite galvanometric deflection; this deflection would remain absolutely fixed during all the changes of density and aggregation which we have supposed the vapour to undergo. In other words—as regards the absorption of radiant heat, the vapour would pass, without breach of continuity, through all its stages of condensation into the liquid form of matter.

A general law of molecular physics is, I apprehend, here illustrated.

§ 8. *Rhythmic Absorption of Radiant Heat by Gases and Vapours.*

Conclusive as the foregoing experimental argument must appear as regards the action of free molecules upon radiant heat, I am nevertheless glad to supplement it by another of a totally different character. On the 29th of November, 1880, I had the pleasure of witnessing, in the laboratory of the

* When the rock-salt cell was empty, reflection of course occurred at its two interior surfaces. A perfectly diathermanous liquid, with the refractive index of rock-salt, would annul this reflection. And though the liquids actually employed had a smaller refractive index than rock-salt, and though they were far from being perfectly diathermanous, their introduction into the cell must nevertheless have diminished the reflection, and thus added to the transmitted heat. This addition, having been determined by calculation, was sensibly neutralized by the introduction of washers of thin note-paper, which slightly augmented the thickness of the liquid stratum traversed by the calorific rays.

Royal Institution, the remarkable experiments of Prof. Graham Bell, wherein musical sounds were evoked by causing an intermittent beam of light to impinge upon thin disks of various kinds of matter. I was soon convinced that the effects were due to the rhythmic gain and loss of heat. Being occupied with experiments on gases and vapours at the time, I thought that they might be invoked to decide the nature of the action revealed by Prof. Bell. The result was mentally clear before the experiment was made. I pictured a highly absorbent vapour, exposed to the shocks of the intermittent beam, suddenly expanding during the moment of exposure, and as suddenly contracting when the beam was intercepted. Pulses of an amplitude probably far greater than those obtainable with solids would, I thought, be thus produced; and these pulses, if caused to succeed each other with sufficient rapidity, would be sure to produce musical sounds.

This idea was tested and verified on the spot. The 'Journal of Telegraph Engineers' for December 8th, 1880, contains the following record of what occurred :—"When Professor Bell was good enough to show me his experiments, I happened to be myself experimenting on the action of vapours upon radiant heat. Old experiments had revealed, and new ones had confirmed, the fact that, as regards the absorption of heat, there existed vast differences between vapours. This is well illustrated by the deportment of bisulphide of carbon and of sulphuric ether, one of which is highly transparent, and the other highly opaque to radiant heat. It occurred to me that, if the action were due to the absorption of heat, we might possibly extract musical sounds from sulphuric-ether vapour; whereas bisulphide-of-carbon vapour being transparent to heat-rays, they would for the most part go through this vapour unabsorbed, and produce no sonorous effect. I think Prof. Bell will bear me witness as to the result. We placed a quantity of sulphuric-ether vapour in a test-tube, and allowed an intermittent beam of light to strike upon the vapour far above the liquid, and we heard distinctly a musical tone of a pitch corresponding to the rapidity of the flashes. We then took the bisulphide-of-carbon vapour, and tried it in a similar manner, but neither Prof. Bell nor myself could hear any trace of a musical sound" *.

* Mr. Bell has given a perfectly accurate account of this occurrence in the Philosophical Magazine, vol. xi. p. 519. With reference to what occurred on the 29th of November he writes thus :—"Professor Tyndall at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the

It was obvious, however, that the arrangement of Prof. Bell —a truly beautiful one—was not suited to bring out the maximum effect. He had employed a series of glass lenses to concentrate his beam; and these, however pure, would, in the case of transparent gases, absorb a large portion of the rays most influential in producing sound. This may be illustrated by comparing a rock-salt lens, in my collection, with a glass lens of the same focal length prepared in the workshop of M. Duboscq. Transmitted through the former, the radiation from an incandescent platinum spiral produced a galvanometric deflection of 55°, possessing, according to the table of calibration, a value of more than 100. Transmitted through the latter, the deflection fell to 10°, or to less than $\frac{1}{10}$ of the radiation transmitted by the rock-salt. The $\frac{9}{10}$ here shown to be intercepted by the transparent glass, consist of heat on which transparent gases and vapours would exert a specially absorbent power. Hence the desirability of maintaining this important factor, in the radiation employed to test the sonorous power of such substances.

It was with the view of preserving intact these powerful calorific rays that I employed in my experiments on calorescence* small concave mirrors silvered in front; and to these mirrors I now resorted. My more intense sources of heat comprised a Siemens lamp connected with a dynamo-machine; an ordinary electric lamp connected with a voltaic battery; and a lime-light, produced sometimes by the combustion of oxygen and hydrogen, and sometimes by oxygen and coal-gas. The lime-light (which was used by me in 1859) is so handy, steady, and otherwise effective, that I have applied it almost exclusively throughout this part of the inquiry. Sources of heat, however, of much lower temperature than the lime-light have proved competent to evoke musical sounds. A candle-flame, a red-hot coal, a red-hot poker, the same poker at the temperature of boiling water, and an incandescent platinum spiral have all been proved effective, though of course far less so than the concentrated lime-light†.

To produce the required intermittence, I first employed a circle of sheet zinc, 16 inches in diameter, provided with radial slits. This was afterwards exchanged for a second disk of the

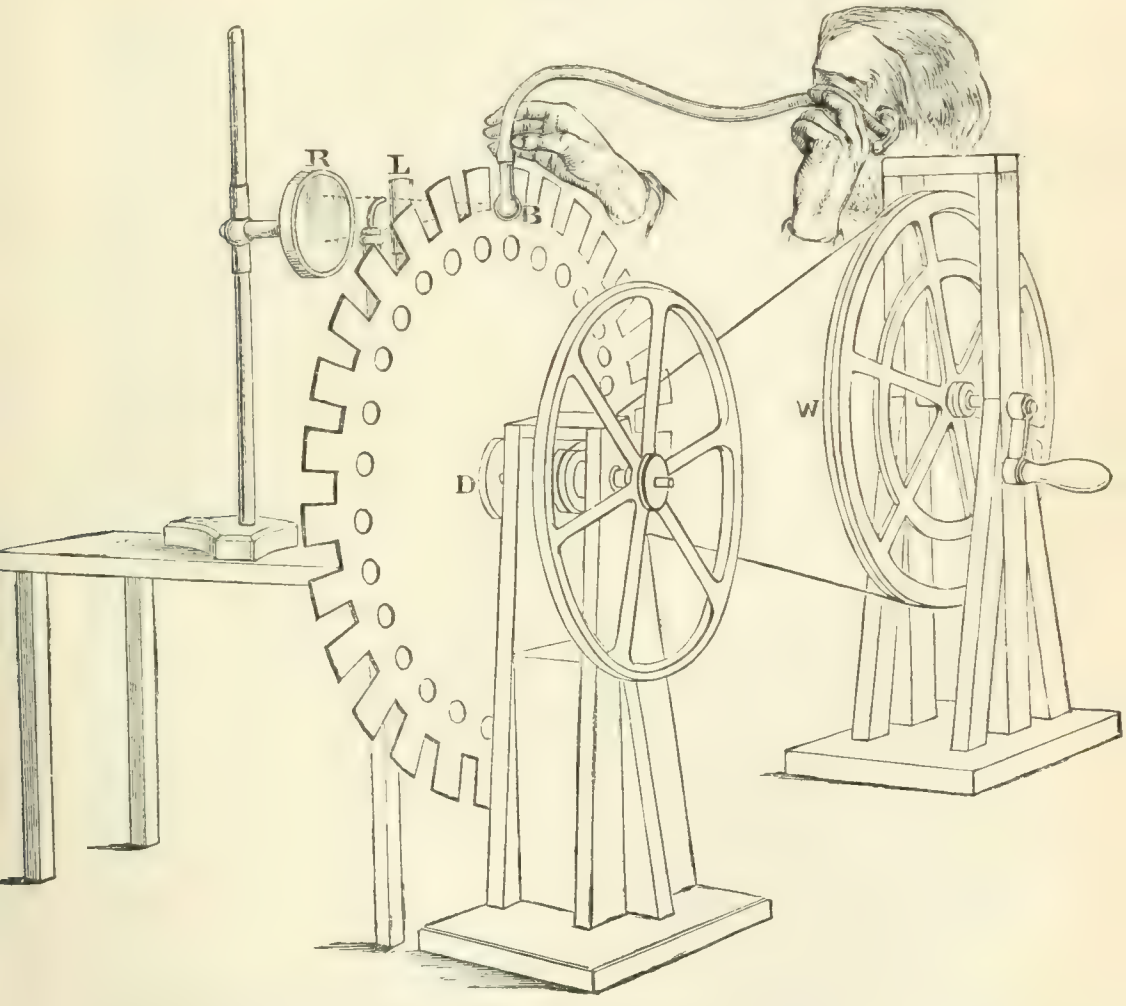
vapour of sulphuric ether (a good absorbent of heat), and another with the vapour of bisulphide of carbon (a poor absorbent); and he predicted that, if any sound was heard, it would be louder in the former case than in the latter. The experiment was immediately made; and the result verified the prediction."

* Philosophical Transactions, 1866, vol. clvi. p. 1.

† These earlier experiments will be found recorded in the Proceedings of the Royal Society, vol. xxxi. pp. 307, 478.

same diameter, but furnished with circumferential teeth and interspaces. The disk was mounted vertically on a whirling table, and caused to rotate across the beam near the focus of the concave mirror. Immediately behind the disk was placed the flask containing the gas or vapour to be examined, while an india-rubber tube, ending in a hollow cone of ivory or box-wood, connected the flask with the ear. With this arrangement, simple as it is, sounds of surprising intensity were obtained with all those gases and vapours which my previous experiments with the experimental tube and thermopile had proved to be powerful absorbers of radiant heat. The final arrangement was that shown in fig. 6.

Fig. 6.



The source of heat is the carefully worked and centred lime-cylinder L, heated by the oxyhydrogen flame. The rays from this source are received by the concave mirror R, and converged upon the bulb B, which contains the substance

to be tested. The bulb is connected with the ear by a piece of india-rubber tubing, ending in a tapering tube of boxwood or ivory. The intermittence of the calorific beam is effected by the disk D of strong cardboard, 2 feet in diameter, and provided at the circumference with 29 teeth and corresponding interspaces *. The disk is caused to rotate by the wheel W, with which it is connected by a band. The positions of the sonorous bulb and of its ear-tube are shown in the figure. In the case of gases lighter than air, the bulb B is turned upside down. With the heavier gases it is held erect. When vapours are tested, a small quantity of liquid is poured into the bulb, which is shaken so as to diffuse the vapour in the air above the liquid. The bulb is held so that the point of maximum concentration of the beam falls upon it.

With this apparatus I have tested more than once the sounding-power of ten gases and of about eighty vapours. As a sound-producer chloride of methyl is supreme. It is, however, closely followed by aldehyde, olefiant gas, and sulphuric ether, the two latter being very nearly equal to each other. The volatility of the liquid from which the vapour is derived is of course an important factor in the result. For however high the inherent capacity of the molecule as an absorber may be, if the molecules be scanty in number the effect is small. Feeble vapours, on the other hand, may to some extent atone by quantity for the inherent weakness of their molecules. A few examples will suffice to show how the specific action of the molecules overrides the effect of volatility. Bisulphide of carbon, with a boiling-point of 43° C., is less powerful than acetic ether, with a boiling-point of 74°. Tetrachloride of carbon boils at 77°; but its sound by no means equals that of acetal, which boils at 104°. Chloroform, with a boiling-point of 61°, is less powerful as a sound-producer than valeral, with a boiling-point of 100°, or even than valerianic ether, with a boiling-point of 144°. Cyanide of methyl boils at 82°—but produces less sound than acetate of propyl, with a boiling-point of 102°. In the experimental tube these vapours follow, as absorbers, the order of their sounds. When tested in liquid layers they follow the same order. I have examined about a score of liquids with boiling-points varying from 163° to 308°. At ordinary temperatures the vapours of these liquids were practically inaudible; the liquids being plunged in a bath of heated oil the vapours so produced emitted, for the most part, powerful sounds. The

* Intermittence is sometimes produced by the series of equidistant circular apertures shown in the figure.

measured absorptions of a sufficient number of substances, in relation to their sounding-power, shall be tabulated immediately.

The fact is worth a passing reference, that the chlorides of the elements appear one and all to be feeble sound-producers, because they are one and all feeble absorbers of radiant heat. Many years ago I had found them highly diathermanous, and accepted chloride of sodium as representative of the class. Silicium chloride, for example, though very volatile, is weak as a sound-producer. Tetrachloride of carbon and terchloride of phosphorus are also volatile, but not strong. Bichloride of tin, chloride of arsenic, chloride of titanium, and chloride of sulphur are all feeble sound-producers. In these three cases the boiling-points are high: but non-volatility is not the cause of the weakness; for when the vapours are raised, by heating their liquids, almost to the pressure of the atmosphere, they are still but feebly sonorous. Whatever, then, the condition may be which renders these substances pervious to radiant heat, it appears to be a condition common to them all.

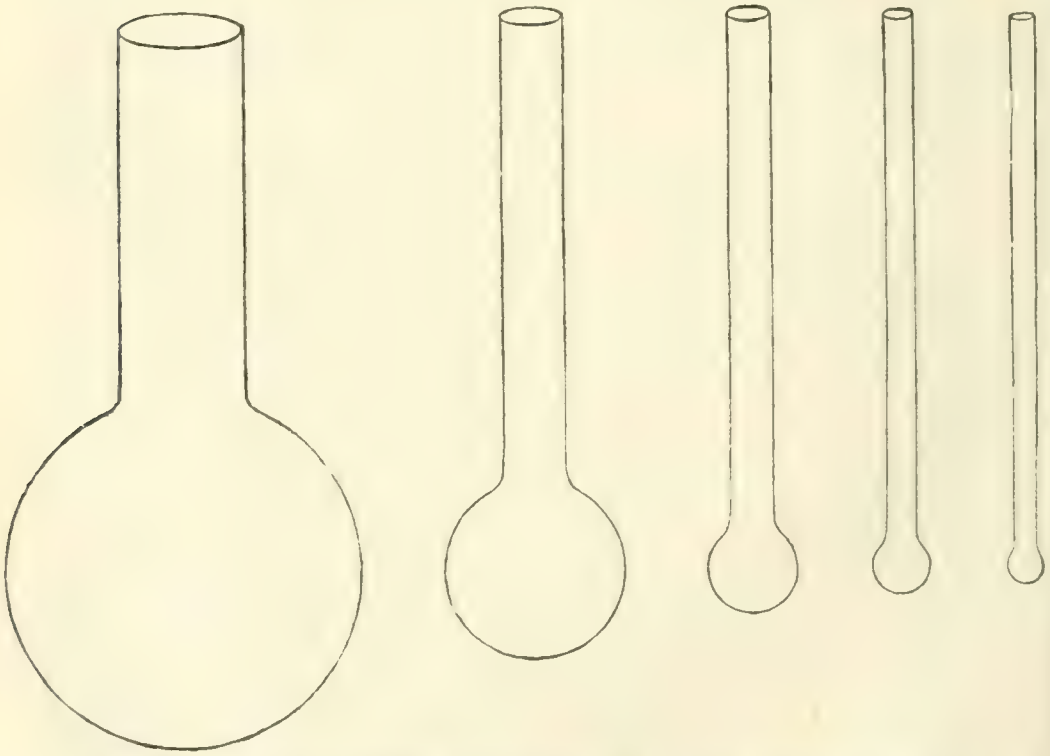
In experiments with the chlorides care must be taken to abolish all fumes. Bichloride of tin sounds loudly with fumes, but is weak without them. Simply heating the upper part of the flask frequently suffices to lower the sounds from loudness almost to silence.

The universality of its presence, and the discussions which it has provoked, rendered the action of water-vapour especially interesting to me. I did not imagine at the outset that the modicum of vapour diffused in atmospheric air at ordinary temperatures could produce sonorous pulses of sensible intensity. In my first experiment, therefore, I warmed water in a flask nearly to its boiling-point. I heated the flask above the water with the spirit-lamp flame, thus dissipating every trace of haze, and then exposed the clear vapour to the intermittent beam. The experiment was a virtual question put to the vapour as to whether I was right or wrong in ascribing to it the power of absorbing radiant heat. The vapour answered by emitting a musical note which, when properly converged upon the tympanum, seemed as loud as the peal of an organ. When the temperature was lowered from 100° C. to 10° C., the sound did not vanish, as I expected it would. It remained not only distinct but strong. The flasks employed in these experiments were dried in a variety of ways, of which I have already given some account, and which will suggest themselves to every experimenter in this field. Taken open from the laboratory, and exposed to the intermittent beam, the flasks are always to some extent sonorous. Placed beside

sulphuric acid underneath the receiver of an air-pump, and permitted to dry there, they are reduced to silence. The slightest invasion of humid air renders them again sonorous. Breathing for a moment into a dried and silent flask, a loud sounding-power is immediately manifested.

Flasks without lips have been specially blown for these experiments, the india-rubber tubing being readily pushed over their necks. Large flasks are not the most suitable. To produce effective pulses, sudden and intense expansions and contractions are required; and these are best obtained when the beam, at its place of maximum concentration, covers a large portion of the matter in the flask. Thin bulbs about a cubic inch in volume are both handy and effective; but the bulb may be reduced to $\frac{1}{50}$, or even $\frac{1}{100}$, of a cubic inch without rendering the sound insensible. A speck of water introduced into such tiny bulbs, when vaporized by heat, produces sounds which are not only sensible, but loud. A series of bulbs which I have actually employed in my experiments are represented in their natural dimensions in fig. 7.

Fig. 7.



It cannot be necessary to state that the absorption which produces the pulses is direct and immediate, being the act of the gaseous molecules. The pulses are not due to the heating of the glass envelope and the communication of its heat to

what it contains; for, were this the mode of heating, air would be as sonorous as olefiant gas. Nor are the pulses due to the sudden vaporization of a liquid layer which might be supposed to overspread the interior of the flask. When water at a low temperature is purposely caused to cover the interior surface, exposure to the beam produces sound of a certain intensity. When the flask is so heated in a spirit-flame as to chase away every trace of the adherent liquid, the exposure of the pure vapour, then within the flask, to the beam, generates a sound far louder than that produced when the liquid film was there. Holding the bulb containing the hot vapour for a little time in the intermittent beam, its temperature falls, the quantity of vapour diminishes, and the sound sinks in intensity. On quitting the spirit-flame, the bulb in some cases must have been near a red heat; but even at this temperature the vapour sounded loud.

It has, I think, been amply shown that when the molecules of a liquid are rendered free by vaporization they carry with them their absorbent power, liquids and vapours being pervious and impervious to the same quality of heat. Hence the inference, that prior transmission through a liquid of sufficient thickness ought so to sift a calorific beam as to render it powerless to act on the vapour of that liquid. Even with the loudest-sounding vapours this proves to be the case, a layer of liquid $\frac{1}{8}$ of an inch thick being found generally sufficient to deprive the beam of its efficient rays, and the vapour of its sounding-power.

In transparent liquids, the visible rays have free transmission; the destruction of sounding-power by such liquids must therefore be due to the absorption of the invisible calorific rays. This induction, which hardly needs verification, is nevertheless capable thereof. Many years ago I pointed out the astonishing transparency of dissolved iodine to the invisible heat-rays. Placed in the path of the intermittent beam, a layer of this substance, perfectly opaque to light, does not sensibly diminish the sound of transparent gases and vapours. To such substances the iodine is exactly complementary, arresting the rays which they transmit, transmitting the rays which they absorb, and therefore not interfering with the sounding-power.

That sounds may also be produced by the absorption of the visible rays is well exemplified by the deportment of iodine and bromine vapours, both of which yield with the lime-light forcible sounds. Here the intervention of a transparent liquid, however adiathermanous it may be, produces no sensible effect upon the sound, the reason being that it permits the particular

rays which act upon the coloured vapours to pass freely through it. A layer of dissolved iodine, on the other hand, deprives the beam of its power of evoking sounds from either iodine or bromine vapour.

The rotation which produces the maximum effect is soon ascertained by experiment. The sound is loudest when the pulses succeed each other in periods which invoke the resonance of the flask. I possess a hollow cone with a well-polished rock-salt base, by which this point is well illustrated. Filling this cone with chloride of methyl, while the base is turned towards the source of heat, the apex of the cone being connected by a tube with the ear, sounds of extraordinary intensity are produced. Abandoning the ear-tube, the sound can be heard at a distance. But to obtain this effect the speed of rotation must be definite and constant. The maximum sound once obtained, either the lowering or the heightening of the speed rapidly enfeebles it. It is difficult by hand-turning to keep the rate of rotation constant. Hence the desirability of a mechanical arrangement which would ensure the proper rapidity and necessary uniformity. One or two motive powers have been tried, including a small steam-engine and an electro-magnetic engine; but the arrangement has not yet been brought to perfection.

§ 9. *Manometric measurements.*

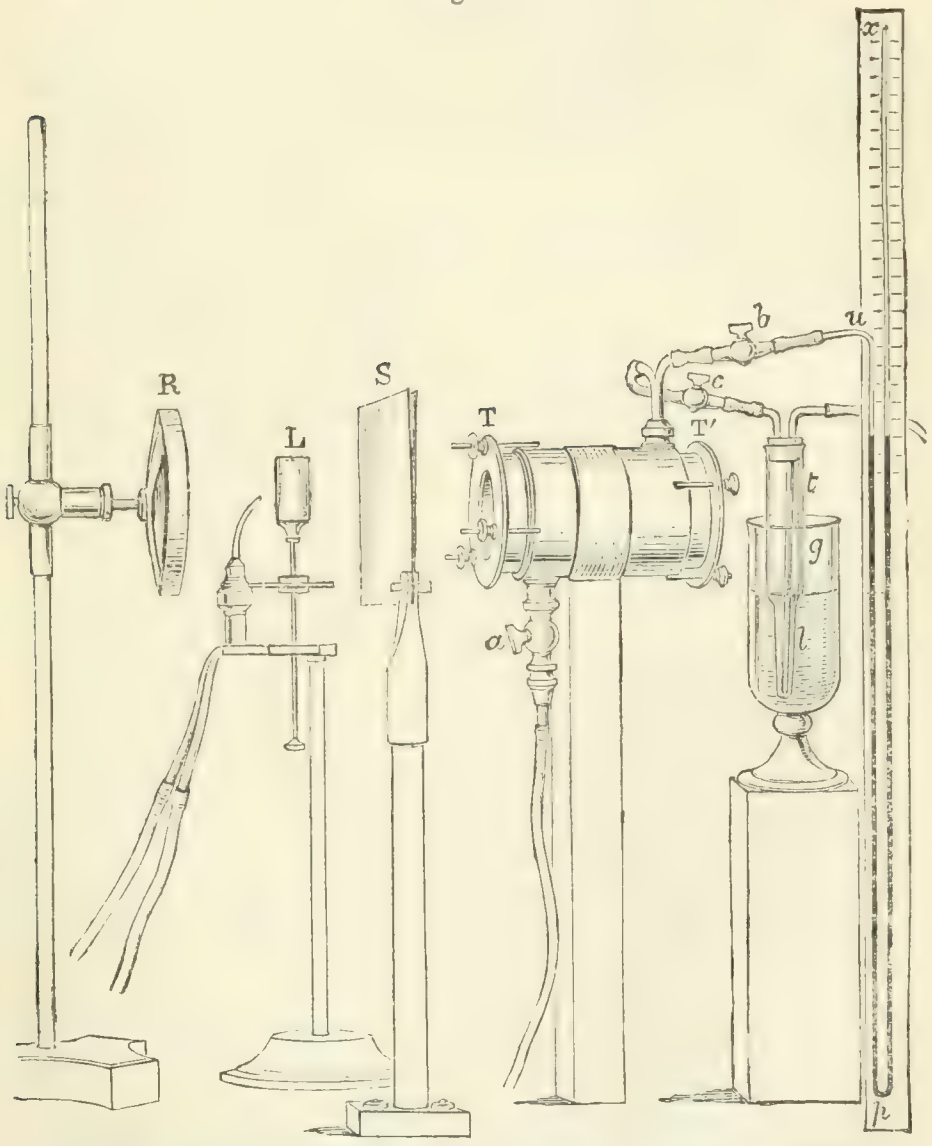
Some time before the visit of Prof. Graham Bell in November, 1880, I had inserted into my old experimental brass cylinder a narrow tube of glass, which, being bent at a right angle a few inches above the cylinder, could hold an index of coloured liquid in its horizontal portion. I had long known that the absorption of radiant heat must be accompanied by the expansion of the absorbing body, but thought that such expansion would furnish only a rough measure of the absorption. With ordinary sources of heat I found the expansion small, even when sulphuric ether occupied the experimental tube; but when a pair of stout carbons, rendered incandescent by a Siemens machine, were employed as a source, the liquid index was driven forcibly out of the narrow glass tube.

The experimental tube, however, was but a rude manometer; and I therefore sketched and described to my assistant at the time, with a view to its construction, a handier instrument. The apparatus was to consist of a short tube with rock-salt ends, capable of being exhausted and filled with any required gas or vapour. Through this tube it was proposed to send a concentrated calorific beam, whose action on an absorbent gas or vapour should be declared by the depression of a liquid column in one leg of a U tube, and its elevation in

the other. Two rock-salt plates were to be employed, with the view of allowing the beam free escape from the tube after it had done its work upon the gas or vapour. The warming of the apparatus by the reverberation of the heat would be thus avoided. The point aimed at was to effect the expansion of the gaseous body purely by radiant heat, and undisturbed as far as possible by heat derived from the envelope*.

A number of manometric tubes of different lengths and materials were constructed on this principle, some being of glass and some of metal. The instrument with which the measurements now to be recorded were executed is represented in fig. 8. T T′ is a glass tube 4 inches long and 3 inches in diameter. It is provided with brass flanges at the ends which

Fig. 8.



* Professor Röntgen was, I believe, the first to turn the expansion of gases to account in demonstrating the absorption of radiant heat. The

reduce the diameter to 2·5 inches. Against these flanges, transparent plates of rock-salt are fixed air-tight. The tightness of the tube was secured, sometimes by india-rubber washers properly greased, and sometimes by cement*. A stop-cock *a* near one end of T T′ was connected with a barometer-tube and an air-pump. A T piece at the other end was connected on the one side with a purifying-apparatus (not shown), consisting of two U tubes, one containing fragments of Carrara marble wetted with caustic potash, the other containing fragments of glass wetted with sulphuric acid. Before entering these U tubes the air was freed from suspended matter by a plug of cotton-wool. On the other side, the T piece was connected with a quill tube of glass bent into the shape of a U, in the two legs of which a coloured liquid stood at the same level. The liquid column when standing at the same level in both arms of the U was 350 millim. high in each, while the free leg of the U rose to a height of about 500 millim. above the surface of the liquid (shortened in the figure). The source of heat was the lime-cylinder L, rendered incandescent by a flame of coal-gas and oxygen. The rays from the lime-cylinder were received by a concave mirror R silvered in front, and sent by it in a convergent beam through the manometer-tube. The focus of the beam was within the tube and near its most distant end. The gas and oxygen were supplied from gas-holders specially constructed for these and similar experiments—long experience of the futility of gas from the public mains, or compressed in iron bottles, having shown independent gas-holders which could be kept at an unalterable pressure to be essential.

The experiments were conducted thus :—A test-tube *t*, plunged in water held by the glass *g*, contained the liquid whose vapour was to be examined. Through a cork which stopped the test-tube passed a narrow tube of glass, ending in a small orifice near the bottom of the test-tube, and at a considerable depth below the surface of the liquid. To augment this depth, and to economise the liquid, the lower part of the test-tube was drawn out to half the diameter of its upper part. A second narrow tube passed also air-tight through the cork, and ended immediately beneath it. Both tubes were bent at a right angle above the cork. The manometric tube being exhausted, air freed from its carbonic acid, its moisture, and

---

very day, moreover, on which I made my communication to the Society of Telegraphic Engineers, viz. the 8th of December, 1880, he forwarded to a scientific journal the announcement of his having obtained sounds from coal-gas and ammonia (see Wiedemann's *Annalen*, Jan. 1881). His subsequent experiments with aqueous vapour &c. agree with mine.

* The absorption of the vapours by india-rubber (which was in some cases extraordinary) caused the washers to be abandoned.

its suspended matter was allowed to bubble through the liquid in the test-tube, and to pass thence into the manometric tube. To spare the oxygen in the gas-holder it was cut off during the interval between two consecutive experiments, the coal-gas being kept continually alight. When the manometric tube was filled, which was always accomplished through an orifice of fixed dimensions, the oxygen was turned on, and the cylinder was allowed to remain for one minute under the action of the intensified flame. During this time a double silver screen S intercepted the radiation. At the end of a minute this screen, which moved on an hinge, was withdrawn, the beam then passing through the mixed air and vapour. The liquid standing in the adjacent leg of the narrow U tube was immediately depressed, that in the opposite leg being equally elevated. The rise of this latter column above its starting-point, marked zero on a millimetre-scale, was accurately measured. Double this rise gave the difference of level in the two legs of the U; and this "water-pressure" expressed the augmentation of elastic force by the absorption of radiant heat.

Here follow a certain number of the measurements which have been thus made. They do not comprise the whole of the substances examined.

TABLE V.—Vapours.

Increase of Elastic Force by Radiant Heat.

Name of Liquid.	Boiling-point.	Mean water-pressure. millim.	Character of sound.
1. Sulphuric ether . . .	35°	300	Very strong.
2. Hydride of amyl . . .	30	279	,,
3. Acetone	58	267	,,
4. Bromide of ethyl . . .	39	264	,,
5. Formic ether	55	261	,,
6. Acetic ether	74	248	,,
7. Acetal	104	237	,,
8. Chloride of allyl . . .	46	235	,,
9. Iodide of methyl . . .	45	233	,,
10. Dichloride of ethidene .	57	217	Strong.
11. Nitrate of ethyl . . .	86	208	,,
12. Nitrite of amyl . . .	99	205	,,
13. Chloride of butyl . . .	69	185	,,
14. Butyric ether	121	183	,,
15. Formic acid	99	180	,,
16. Valeral	100	172	,,
17. Valerianic ether . . .	144	168	,,
18. Acetate of propyl . . .	102	166	,,
19. Methylic alcohol . . .	—	162	,,

Table V. (*continued*).

Name of Liquid.	Boiling-point.	Mean water-pressure. millim.	Character of sound.
	°		
20. Iodide of ethyl	72	148	Moderate.
21. Bromide of butyl	92	134	,,
22. Dutch liquid	85	127	,,
23. Acetate of butyl	114	120	,,
24. Benzol	81	117	,,
25. Carbonic ether	126	108	,,
26. Chloride of amyl	102	105	,,
27. Chloropicrin	112	94	,,
28. Iodide of allyl	101	92	,,
29. Chloroform	61	89	,,
30. Iodide of butyl	121	88	,,
31. Allylic alcohol	97	84	,,
32. Bisulphide of carbon	43	81	,,
33. Bromide of amyl	119	78	,,
34. Cyanide of ethyl	98	77	,,
35. Butyl alcohol	110	72	Weak.
36. Nitrate of amyl	147	67	,,
37. Oxalate of ethyl	—	66	,,
38. Cyanide of methyl	82	64	,,
39. Tetrachloride of carbon	77	58	,,
40. Bromoform	150	44	,,
41. Xylol	140	44	,,
42. Amylic alcohol	130	42	,,
43. Iodide of amyl	146	42	,,
44. Terebine	160	39	,,
45. Cymole	175	38	,,
46. Butyric acid	163		
47. Butyrate of amyl	176		
48. Caprylic alcohol	180		
49. Valerianic acid	175		
50. Pure aniline	184		
51. Œnanthic ether	188	Very small absorptions and very feeble sounds at ordinary temperatures. Sounds in most cases very strong when liquid is heated to its boiling-point.	
52. Valerianate of amyl	196		
53. Salicylous acid	196		
54. Caproic acid	205		
55. Nitro-benzol	205		
56. Kreosote	210		
57. Menthol	213		
58. Chinoline	238		
59. Eugenol	247		
60. Nicotine	250		
61. Monobromnaphthaline	277		
62. Sebacic ether	308		

TABLE VI.—Gases.

Name of Gas.	Water-pressure. millim.
Chloride of methyl	350
Aldehyde	325
Olefiant gas	315
Sulphuric ether	300
Nitrous oxide	198
Marsh-gas	164
Carbonic acid	144
Carbonic oxide	116
Oxygen	5
Hydrogen	5
Nitrogen	5
Dry air	5
Humid air at 50° C.	130

Sulphuric ether is here inserted with the view of connecting this Table with the last. Of all gaseous bodies hitherto examined, chloride of methyl is the most energetic absorber and the most powerful sound-producer. After it comes aldehyde, with a boiling-point of 21° C. The figure 5 attached to the elementary gases and to dry air expresses, not absorption of radiant heat, but expansion, due to contact with the slightly warmed apparatus. The nitrous oxide employed was derived from an iron bottle, in which it was preserved for medical purposes. In some of my experiments marsh-gas showed itself a better absorber than nitrous oxide. This, for instance, was the case in experiments made in the spring of 1880 with the manometer. The sample of marsh-gas wherewith the foregoing result was obtained was very carefully prepared in our chemical laboratory.

The temperature of 50° C. in the case of humid air was obtained in a wooden shed erected in our laboratory. The shed is traversed by two tubes of sheet iron 4 inches in diameter, which carry the heated air and products of combustion from two large ring burners. It is 8′ 6″ long, 4′ 3″ wide, and 7 feet high. The temperature of the air within it can be readily raised to 60° C. In the experiment recorded in Table VI. the air was taken from the outside laboratory through a tube passing through the wooden wall of the shed. It was caused to bubble through water contained in a large flask which had been permitted to remain for some time in the warm shed. The mixed air and vapour entered the manometer-tube at a temperature some degrees lower than that of the tube itself. Closely examined, all parts of this tube were bright and dry when the vapour-laden air was within it. On

permitting the beam from the lime-light (produced by coal-gas and oxygen) to pass through the mixture, a prompt rise of 65 millim. was the consequence. Cutting the beam off, the column rapidly returned to zero. The double of 65, or 130 millim., gives the difference of level in the two legs of the U tube.

I have done my best to render these determinations correct. They have been repeated both by myself and my assistant* a great number of times. The first measurements were made in the early part of last year, and were made known in the Royal Institution on the 8th of April, 1881. Difficulties were encountered in obtaining a powerful, and at the same time constant, source of heat. The mixture of coal-gas and oxygen issuing from independent holders was finally resorted to. The sounds are classified into "very strong," strong," &c.; but it is, of course, impossible to say where one class ends and another begins. They shade gradually into each other. But if the middle members of any class be compared with the corresponding members of another class, the difference of sounding-power will appear.

§ 10. *Application of Results to Meteorology.*

If it be at length conceded that aqueous vapour exerts upon radiant heat the action which I so long ago ascribed to it, I think the knowledge of this action will prove of importance to the scientific meteorologist. Meteorology, as connected with heat, seems to me to abound in facts which it has hitherto been incompetent to explain. This, for example, I hold to be the case as regards the celebrated observations of Patrick Wilson of Glasgow, made a century ago. Wilson brought strongly into light the great differences which sometimes exist between the temperature of the earth's surface and of the air at a small elevation above the surface. His letter to Dr. Maskelyne on this subject is published in the Philosophical Transactions for 1780, under the title:—"An Account of a most extraordinary degree of Cold at Glasgow, together with some new Experiments and Observations on the comparative Temperature of Hoar Frost and the Air near it." On the afternoon of the 13th of January, 1780, the cold was intense, a thermometer at the high window of the observatory pointing, at 7 P.M., to 0° Fahr. At 8 P.M., Wilson and Dr. Irvine laid two thermometers upon the snow, and hung up two others in the air 2 feet above the snow. Here follow the temperatures observed on the evening of the 13th and on the morning of the 14th of January:—

* Who has aided me in this investigation with his usual zeal and intelligence.

Time of observation.	Snow-temperature.	Air-temperature.
$8\frac{1}{2}$ P.M.	-12°	-0°
9 „	-14	-2
10 „	-14	-4
11 „	-17	-6
$11\frac{1}{2}$ „	-18	-6
$\frac{1}{2}$ A.M.	-20	-8
1 „	-23	-7

The sign — signifies that the temperatures were all below zero Fahrenheit. These temperatures amply justify Wilson's statement that the cold was "extraordinary." Coexistent moreover with the general cold, we have a difference of 16° between the temperature of the surface and that of the air 2 feet above it. Had the air-thermometer been hung 10 feet instead of 2 feet above the surface, the difference would have been still greater. The thermometer, moreover, must have been chilled, not only by its immersion in cold air, but also by its own radiation against the intensely cold snow. The chilling of the superficial snow was purely an effect of radiation. Beneath the surface its temperature reached +14°. Wilson filled a bread-basket with this warm snow at $2\frac{1}{2}$ A.M. on the 14th. Within half an hour it had fallen 24°, and in two hours 32°.

I venture to predict that if Wilson's experiment be repeated during the cold of a Canadian winter the same result will be obtained; and it seems to me that until the action of water-vapour upon radiant heat had been discovered, no explanation of the phenomenon could have been given. It was accepted, but not accounted for. On the night of Wilson's observations "a light air was felt coming from the east." With such an "air" and such a temperature the quantity of water-vapour in the atmosphere must have been infinitesimal. Dry air being a practical vacuum to the rays of heat, were the vapour-screen entirely removed, the earth would find itself exchanging temperatures with celestial space, and the superficial chill would be commensurate. In Wilson's case, though vapour was not abolished, it was so far diminished as to produce the observed refrigeration. Meteorologists, I am informed, sometimes say that laboratory experiments, however well performed, have but little application to their field of observation*. I,

* Mr. Hill, the Meteorological reporter for the North-western Provinces of India, writes thus:—"There is even, on the part of some, an evident reluctance to accept the decision of laboratory experiments on the question of atmospheric absorption as final, however ingenious, varied, and consistent with one another the experiments may be" (Proc. Roy. Soc. vol. xxxiii. p. 216).

on the other hand, submit that such experiments are necessary to rescue their science from empiricism. What could Wells have done with dew had he not been preceded by Leslie and Rumford? His whole theory is an application of results obtained in the laboratory *.

What I have stated regarding Wilson applies also to Six, who concluded from his experiments "that the greatest differences at night in point of temperature, between bodies on the surface of the earth and the atmosphere near it, are those which take place in very cold weather." This is quoted from Wells †, who, in his essay on Dew, recurs more than once to the subject. He signalizes, but does not explain, "the greater difference which takes place in very cold weather, if it be calm and clear, between the temperatures of the air and of bodies on the earth at night, than in equally calm and clear weather in summer"‡. A considerable number of observations bearing upon this point are scattered through the essay. The radiant power of the air being practically *nil*, it retains for a considerable time the warmth imparted to it during the day, while, when it is dry, the rays from the surface of the earth pass unimpeded through it. Hence the relative refrigeration of the surface §.

In regard to the action of water-vapour Magnus considered experiment superfluous, as the phenomenon of dew sufficed to prove me wrong. If the vapour possessed the power which I ascribed to it, he contended that dew could not be formed. It is not difficult to dispose of this objection. The formation of dew and superficial refrigeration are connected, not by coincidence, but by opposition. I would venture to predict that where the one is great the other, in general, will be small. "Very little dew," says Wells, "appeared on the two nights of the greatest cold I have ever observed on the surface of

* "Its complete theory," says Wells, "could not possibly, in my opinion, have been attained, before the discoveries on heat were made, which are contained in the works of Mr. Leslie and Count Rumford" (Essays, p. 191).

† Essays, p. 176. Wells thus generously refers to the labours of Wilson:—"Indeed, several of my experiments upon dew were only imitations of some which had been previously made on hoar frost, by that ingenious and worthy man" (Essays, p. 151).

‡ Ibid. p. 188.

§ It ought to be stated that, contrary to Six and to Wells, Mr. Glaisher has found that "the differences between the temperature of the air and of bodies on the earth at night, in equally clear and calm weather, were the same at every period of the year" (Phil. Trans. 1847, p. 126). He moreover records differences considerably in excess of those observed by Wilson and by Wells. Keeping the action of aqueous vapour in view, the elaborate paper of Mr. Glaisher might repay further discussion.

the earth, relatively to the temperature of the air, both of them having occurred after a long tract of dry weather"*. This evidence is specially valuable in view of the fact that Wells knew nothing of the action of water-vapour on radiant heat. On another occasion he observed a difference of $9\frac{1}{2}$ degrees between the temperature of the air and that of wool placed upon the earth, without any deposition of dew whatever upon the chilled wool †. He supplements these observations by one equally important in the opposite direction. "On the night," he writes, "which afforded the most copious dew ever observed by me, the cold possessed by the grass beyond that of the air was for the most part only 3° and 4°"‡. The smallness of the refrigeration in this instance, and the copiousness of the dew, I refer to one and the same cause, namely the abundance of vapour. Heavy dew implies this abundance; abundant vapour, if not too local, implies checked radiation; and checked radiation tends to abolish the difference of temperature between air and soil.

Wells had a theory of his own to account for the association of moderate refrigeration and heavy dew. The heat rendered free by the condensation of the vapour to liquid "prevented" the cold. He tried to determine the effect of condensation by the following experiment §. To 10 grains of wool he added 21 grains of water, this being the quantity of dew deposited on wool in one of his observations. He placed the moistened wool in a saucer on a feather-bed in a room, and determined the chill produced by its evaporation. After eight hours, while the wool still retained $2\frac{1}{2}$ grains of moisture, its temperature was 4° lower than that of a dry saucer placed near it on the same feather-bed. When the process is reversed, condensation instead of evaporation coming into play, the foregoing amount of heat, Wells contended, would be liberated on the grass, and thus prevent inordinate refrigeration.

In thus reasoning Wells went to the limit of the knowledge of his time; and the explanation here given is a philosophical one. But I do not think it a sufficient explanation. The grass is exposed to the open atmosphere; and the heat deve-

* Essays, p. 186.

† Ibid. p. 183. Wells sometimes found wetted wool to lose weight, while dry wool gained no weight though lowered many degrees below the temperature of the air (p. 184).

‡ Ibid. p. 169. From a remark occurring at page 135, it may be inferred that the night here referred to was that common to the 29th and 30th of July, 1813. On the two occasions first mentioned, when there was but little dew, the grass was in one instance 12°, and in the other 14° colder than the air.

§ Essays, p. 187.

loped by every successive film of moisture condensed upon its blades is instantly wasted by radiation. Those who are accustomed to work with the thermopile know how rapidly the associated galvanometer-needle falls from a high deflection to zero when the heat incident upon the pile is suddenly cut off. A similar rapidity of waste would assuredly occur during the slow formation of dew. The heat of condensation could not, for this reason, be housed in the manner supposed by Wells *. The true explanation I hold to be that already indicated—the checking of radiation by vapour, the abundance of which was indicated by the copious deposition of dew.

If the experiments of Wilson could be made in an atmosphere still colder than that in which he worked—on a larger plain, for instance, and in a country remarkable for the dryness of its air—Wells considered that a difference of at least 30° would be observed on serene nights between the air and a downy substance placed on the earth. And as Six had found the air-temperature at an elevation of 220 feet to be 10° higher than at 7 feet, these 10° being added to the 30° would make the surface at least 40° colder than the air at the height of 200 or 300 feet. With all this I agree. I would go even further, and reiterate here a statement made by me nineteen years ago, that the withdrawal of the aqueous vapour of our atmosphere, for a single calm night, would kill every plant in England capable of being killed by a freezing temperature.

Pictet, I believe, was the first to notice that the temperature of the air near the earth's surface on serene nights diminished as the surface was approached, the sequence of the day temperatures being thus inverted. To account for the chilling of the air, say, at 10 feet above the earth's surface beyond that

* Wells himself observed in grass a fall of temperature of 7° in twenty minutes. This gives us some notion of the rapidity with which a radiant so powerful as water would dispose of its heat (Essays, p. 157).

At the instance of my friend Mr. Francis Galton, and with the kind sanction of the Meteorological Council, the following instructive observations, showing the temperatures recorded by two thermometers—the one placed on cotton-wool resting on the earth, and the other hung at a height of four feet in the air—were recently made by Mr. Whipple at Kew:—

Time.	Air.	Wool.
	°	°
4.20 P.M.	34·8	33·2
4.25 „	32·5	27·6
4.30 „	32·4	25·7
4.35 „	32·4	23·4
4.40 „	32·2	21·7
4.45 „	32·2	20·7

The rapidity of radiation is well shown by these observations, an exposure of twenty-five minutes sufficing to establish a difference of 11°·5.

at 100 feet above the surface, Wells invoked the radiant power of the air itself. It is chilled, he thought, by its own emission against the cold earth underneath. Wells takes great pains to prove that the air possesses this power; and if not the air, the floating matter of the air will, he contends, exert the necessary radiation. Difficulties of this nature not unfrequently crop up in works on meteorology; but they disappear in presence of the fact that mixed with the air is a gaseous constituent, small in quantity, but capable of producing the effects needing explanation.

As an example of such difficulties, I have already referred to Sir John Leslie's paper, "On certain Impressions of Cold transmitted from the Higher Atmosphere"*. He there describes the Æthrioscope, an instrument used to measure these impressions. "The sensibility," he says, "of the instrument is very striking; for the liquor incessantly falls and rises in the stem with every passing cloud. Under a fine blue sky, it will sometimes indicate a cold of 50 millesimal degrees; yet on the other days, when the air seems equally bright, the effect is only 30°. The causes of these variations are not quite ascertained." He might have said, not at all ascertained. The causes, I submit, are the variations of the quantity of transparent aqueous vapour in the atmosphere, which, without affecting the visual brightness of the air, is competent to arrest radiation from the earth. Precisely of the same character is the difficulty noticed by Lieutenant Hennessey in his paper on "Actinometrical Observations in India." Like Leslie, he speaks of variations the causes of which are not ascertained. "Again," he says, "there is a change of intensity from day to day apparently not due to alterations in the sun's declination so that the average daily curve (about noon) is higher or lower without any visible reason"†. The reason here is that applicable in Leslie's case, namely the variations of the invisible atmospheric vapour.

In 1866 my friend Professor Soret, of Geneva, favoured me with a letter from which the following is an extract:—"In two comparative experiments, made within a few days at Geneva and Bologna, the most powerful radiation was obtained at Geneva, although at Bologna the heavens were visibly *purer*. The result appears to me to support your views regarding the aqueous vapour of the air; for the tension of aqueous vapour at Bologna was 10·7, while at Geneva it was only 6·33."

Cautiously abstaining from drawing a general conclusion

* Transactions Roy. Soc. of Edinburgh, vol. viii. p. 483.

† Proc. Roy. Soc. vol. xix. p. 228.

from a single fact, M. Soret, in 1868, made some further experiments on solar radiation. The intensity was measured by first allowing the rays to fall directly on the thermometer of the actinometer, and then by allowing them, prior to meeting the thermometer, to pass through 5 centimetres of water. Calling the first temperature T and the second t, the ratio $\frac{t}{T}$ will be obviously greatest when the absorption by the water is least. And as we know that water exerts its chief absorbent power on the ultra-red rays of the spectrum, the variations in this ratio observed at different atmospheric thicknesses will enable us to infer the nature of the heat "arrested" by the atmosphere.

M. Soret found the ratio to be greater in the middle of the day than when the sun is near the horizon. At 12.30, for example, on the 9th of March the ratio was 0·594, while at 5.10, on the same day, it was only 0·409. A smaller fraction of the total heat was absorbed by the water near midday than near sunset. At midday, therefore, the solar heat was more thoroughly sifted of its calorific rays, and more transmissible by water, than it was when the atmospheric thickness was much greater. It would seem difficult to reconcile this result with the notion that aqueous vapour is the absorbing constituent of the atmosphere.

A year subsequently MM. Desains and Branley found, both at Paris and at Lucerne, that the sun's heat was always more transmissible through water and alum in the morning than at midday. I have too much confidence in the able experimenters here named to think any of them wrong. How, then, is the discrepancy between them to be accounted for? I think in the following way. What is called the glow of the Alps varies greatly with the quantity of suspended matter in the air. When pronounced, it shows that the more refrangible constituents have been in great part removed from the sun's rays. The proportion of the less-refrangible rays in the total radiation is augmented in this way, the relative transmissibility of the heat being diminished. It was, I would suggest, heat that had its character impressed upon it in this way by scattering, and not by absorption, that yielded the result obtained by M. Soret.

Whatever may be the value of this explanation, one result of great interest to me was established by the two French experimenters. Simultaneous observations were made by them on the summit of the Rigi and at Lucerne, the vertical distance between both stations being 4756 feet. Within this stratum 17·1 per cent. of the solar rays was absorbed.

Experiments were made at the same time at both stations on the perviousness of water to the solar rays. If, as I contend, a vapour and its liquid absorb the same rays, the withdrawal of 17 per cent. of the radiation by aqueous vapour must render the residual heat more transmissible by water. This is precisely what the French experimenters found it to be. "Through a glass trough 0·08 of a metre in length, and full of water, the rays on the Rigi passed in the proportion of 685, and at Lucerne in the proportion of 730, per 1000 of the incident heat."

Magnus was so convinced of the impotency of aqueous vapour to arrest radiant heat, that in reference to various meteorological phenomena, where the action I had ascribed to it offered a satisfactory explanation of the facts, he put in its place mist or haze, the existence of which he assumed, even when neither mist nor haze was visible. There are various passages in the Essay on Dew which it would be difficult to reconcile with this assumption; for they show that even visible atmospheric turbidity has by no means the influence which Magnus ascribed to it.

Thus, on the 7th of January, 1814, Wells observed, "a little after sunset," a refrigeration of 8°, at a time when some parts of the sky were covered with clouds, and the lower atmosphere a little obscure *. On another evening, "when the atmosphere was neither very clear nor very still," a difference of $14\frac{1}{2}°$ was observed between the temperatures of air and swandown. Wells also observed a refrigeration of 5° when the sky was thickly covered with high clouds. A very definite observation in regard to haze was made on the 21st of January, 1814. The air at this time was "a good deal hazy"†. Notwithstanding this, the temperature of swandown placed on snow was $13\frac{1}{2}°$ lower than that of the air 4 feet above it. Thus, if other circumstances be favourable—that is to say, if the air be dry—even a visible haze does not prevent powerful refrigeration. I close these references to mist and haze by mentioning a most striking observation made by Wells on the 1st of January, 1814. "I found," he says, "during a dense fog, while the weather was very calm, a thermometer lying on grass thickly covered with hoar frost 9° lower than another suspended in the air 4 feet above the former"‡. Here, as before, low temperature implies scanty vapour, the absence of which enabled the grass to pour its heat even through the interstices of a dense fog §.

* Essays, p. 174. † Ibid. p. 176. ‡ Ibid. p. 158.

§ Mr. Glaisher moreover has found differences of from 10° to 12° between grass and air "at times when the sky has been free from clouds but not bright, haze and vapour being prevalent" (Phil. Trans. 1847, p. 145).

I could draw still further on this admirable Essay in illustration of the thesis which I have so long defended. As a repertory of valuable facts and penetrative arguments it probably stands unrivalled in the literature of meteorology. One point remains which cannot be passed over. It has reference to the part played by clouds in arresting and returning the radiation from the earth. "No direct experiments," says Wells, "can be made to ascertain the manner in which clouds prevent, or occasion to be small, the appearance of a cold at night, upon the surface of the earth, greater than that of the atmosphere; but it may, I think, be firmly [fairly?] concluded from what has been said in the preceding article, that they produce this effect, almost entirely, by radiating heat to the earth, in return for that which they intercept in its progress from the earth towards the heavens"*. Wells had the strongest analogies to adduce in favour of this view. He placed boards and sheets of paper above his thermometer, thus screening them from the clear sky; and in that beautiful passage where he speaks of "the pride of self-knowledge," and refers to the simple devices which experience had taught gardeners to apply for the safety of their plants, he mentions the protection which even a thin cambric handkerchief can afford to thermometers over which it is spread. He was irresistibly led to conclude that clouds acted in the same fashion, and that when they occupied the firmament, they sent back to the earth the heat incident upon them, exactly as the board, and the paper, and the cambric sent it back in experiments made close to the surface of the earth.

But in the enunciation of this hypothesis his knowledge and penetration as an observer came, as usual, into play. He is careful to distinguish between high clouds and low clouds. "Dense clouds," he says, "near the earth must possess the heat of the lower atmosphere, and will therefore send to the earth as much, or nearly as much, heat as they receive from it by radiation. But similarly dense clouds, if very high, though they equally intercept the communication of the earth with the sky, yet being from this elevated situation colder than the earth, will radiate to it less heat than they receive from it, and may consequently admit of bodies on its surface becoming several degrees colder than the air"†.

Magnus urged this point against me; and I may be per-

* Essays, p. 205.

† Ibid. p. 206. "If," says Wells, in another place, "the clouds were high and the weather calm, I have sometimes seen on grass, though the sky was entirely hidden, no very inconsiderable quantity of dew" (ibid. p. 128).

mitted to say that I always considered it one of his strongest points—my holding of this opinion being, however, dependent on the views which I entertained, and which were opposed to those of Magnus, regarding the relation of liquid to vapour. If, as I believe, the absorbent power is not enhanced by condensation—if in this respect water behaves like hydride of amyl and sulphuric ether,—then I do not think that such a process of reverberation, between earth and clouds, as that assumed by Wells is possible. The aqueous vapour, in a very few thousand feet of air, of average humidity, would, if condensed, form a layer of water 0·5 of an inch in thickness; and through such a layer, or even through a thinner layer, the earth's radiation could not pass. If the earth's radiation reach the clouds, it must be by a process similar to that of handing buckets from man to man in the case of a fire. The heat must be taken up and re-radiated, we know not how many times, before the clouds are reached. I do not, however, think this mechanism of discharge necessary. Low clouds will not form above exposed thermometers, in weather previously serene, unless some change has occurred in the atmosphere; and change may occur where no cloud reveals it. It may extend, and in most cases probably does extend, from the low clouds to the earth. I think it in the highest degree probable that in most, if not in all, the cases cited by Wells of thermometers rising when clouds were formed overhead, the precipitation was due to the intrusion of humid air, the humidity extending invisibly from the clouds downwards. To this, I believe, rather than to any immediate exchange of temperature with the clouds, the rapid and considerable changes of temperature referred to by him at pp. 156 and 157 of the Essay are to be ascribed. Future observations will, doubtless, bring this view to an experimental test.

I here recur with renewed pleasure to a paper published by General Strachey in the Philosophical Magazine for July 1866. It was probably intended as a reply to the strictures of Magnus; and to me it appears cogent in the highest degree. General Strachey calculated the fall of temperature from $6^h\ 40^m$ P.M., Madras time, to $5^h\ 40^m$ next morning, for a certain number of days, selected as sufficiently clear. He also calculated the mean vapour-tension during the nights, and tabulated the results according to the quantity of vapour, for the years 1841, 1842, 1843, and 1844. In such observations, as pointed out by Strachey, discrepancies are to be expected; but the general result is unmistakable, that the fall of temperature by radiation is greatest when the air is driest, and least when the air is most humid. A series of observations

made at Madras between the 4th and the 25th of March, 1850, are particularly suitable for the illustration of this law of action. During the period referred to "the sky remained remarkably clear, while great variations of the quantity of vapour took place." Here are the results as tabulated by General Strachey:—

Tension of vapour.	·888.	·849.	·805.	·749.	·708.
Fall of temperature from $6^h\ 40^m$ P.M. to $5^h\ 40$ A.M.	6°·0	7°·1	8°·3	8°·5	10°·3

Tension of vapour.	·659.	·605.	·554.	·435.
Fall of temperature from $6^h\ 40^m$ P.M. to $5^h\ 40^m$ A.M.	12°·6	12°·1	13°·1	16·5.

These results, if correct (and I am not aware that they have ever been questioned), show in the most impressive manner the influence of the aqueous vapour of our atmosphere on our planet's radiation. As the vapour diminishes, the door opens, which permits the escape of the earth's heat. The halving of the vapour-tension nearly trebles the refrigeration of the thermometer.

Equally clear is the evidence given by General Strachey as to the action of aqueous vapour upon the radiation of the sun. Here are the results:—

Tension of vapour.	·824.	·737.	·670.	·576.	·511.	·394.
Rise of temperature from $5^h\ 40^m$ A.M. to $1^h\ 40^m$ P.M.	12°·4	15°·1	19°·3	22°·2	24°·3	27°·0

This table is the exact complement of the last. *There* the *fall* of temperature was powerfully promoted by the withdrawal of the vapour. *Here* the *rise* of temperature is powerfully promoted by the same cause*.

* Mr. Hill, Meteorological Reporter for the North-west Provinces of India, in a paper recently presented to the Royal Society, describes an attempt to determine the "Constituent of the Atmosphere which absorbs Radiant Heat." He uses for this purpose the careful observations made by Messrs. J. B. N. Hennessey and W. H. Cole, at Mussoree and Dehra respectively. From the absence of symmetry in the quantities of heat received by the actinometer on both sides of noon at Mussoree, and from the existence of this symmetry at Dehra, he infers the periodic lifting and lowering of the absorbing constituent above and below the higher station. He finds the variation of the absorption-coefficient to follow the variations

But the most impressive illustration of the action of aqueous vapour is now to be referred to. In 1865 I subjected to examination the radiation from the electric light produced by a battery of 50 of Grove's cells, and found, by prismatic analysis, the invisible calorific radiation to be 7·7 times the visible. The determination was afterwards made by the method of filtration, whereby the one class of rays was detached with great sharpness from the other, and both of them rendered measurable. By this method the invisible radiation was found to be 8 times the visible. A close agreement was therefore established between the results of the two methods. Computed from the diagram of Müller, the invisible radiation of the sun is twice the visible. This smaller ratio might, of course, be referred to the original quality of the solar emission, the ratio holding good up to the surface of the sun. But having placed, as I thought beyond doubt, the action of aqueous vapour on radiant heat, and believing the action of the vapour to be substantially the same as that of water, I reasoned and experimented as follows in 1865:—"The sun's rays, before reaching our earth, have to pass through the atmosphere, where they encounter the atmospheric vapour, which exercises a powerful absorption on the invisible calorific rays. From this, apart from other considerations, it would follow that the ratio of the invisible to the visible radiation in the case of the sun must be less than in the case of the electric light. Experiment, we see, justifies this conclusion. If we cause the beam from the electric lamp to pass through a layer of water of suitable thickness, we place its radiation in approximately the same condition as that of the sun; and on decomposing the beam after it has been thus sifted, we obtain a distribution of heat closely resembling that observed in the solar spectrum."

If, therefore, we could get above the vapour-screen which swathes the earth, the "powerful absorption" referred to in the paragraph just quoted would disappear, the ratio of the invisible to the visible solar rays being augmented correspondingly. That such would be the case I have long taken for granted; but I hardly hoped for a corroboration so impressive as that furnished by the recent observations of Pro-

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of vapour-tension. From this and from a similar result obtained by a second method of calculation, he draws the conclusion "that there can be very little error in agreeing with Dr. Tyndall that the absorptive power of dry air is sensibly nothing, and that the total absorptive power of the atmosphere is due to the water-vapour it contains." A most interesting discourse on Solar Heat, by M. Violle, of Grenoble, will be found in the *Revue Scientifique* for 1878, p. 944. I guard myself against saying that the diathermancy of dry air is *perfect*.

fessor Langley, in the Sierra Nevada Mountains of California. Professor Langley is known to have highly distinguished himself by researches on radiant heat, with instruments of his own invention. He writes to me thus from Mount Whitney, California, September 10, 1881 :—

"I received your letter just as I was starting on the expedition to this point of which I wrote. I much regretted that I had not time to provide myself with your mercury-pyrheliometer; so I have been obliged to use the old form, with its many disadvantages.

"Our route here has led us through the dryest parts of this continent, and across rainless deserts to this mountain, where the air is perhaps drier than at any other equal altitude ever used for scientific investigation. I write from an altitude of 12,000 feet, while the 'Peak' rises nearly 3000 more above me. I have been successful in bringing up, and using here, the rather complex and delicate apparatus for investigating the absorption of the atmosphere on homogeneous rays, throughout the visible and invisible spectrum.

"You may be interested in knowing that the result indicates a great difference in the *distribution* of the solar energy here from that to which we are accustomed in regions of ordinary humidity; and that while the evidence of the effect of water-vapour on the more refrangible rays is feeble, there is, on the other hand, a systematic effect due to its absence, which shows by contrast its power on the red and ultra-red in a striking light.

"These experiments also indicate an enormous extension of the ultra-red spectrum beyond the point to which it has been followed below, and, being made on a scale different from that of the laboratory—on one indeed as grand as nature can furnish—and by means wholly independent of those usually applied to the research, must, I think, when published, put an end to every doubt as to the accuracy of the statements so long since made by you, as to the absorbent power of this agent over the greater part of the spectrum, and as to its predominant importance in modifying to us the solar energy.

"I am, with much regard,

"Very truly yours,

"S. P. Langley."

LXIV. *On Rhabdophane, a new Mineral.*

By W. G. Lettsom, *Esq.**

HAVING ascertained from Monsieur Lecoq de Boisbaudran, who had had the kindness to favour me with a specimen of his new metal gallium, that it would be agreeable to him to examine our British blendes for that metal, I applied to various dealers for specimens thereof, and I also asked a few friends to give me their assistance in the matter. Among the latter was Mr. Ludlam, who with his usual liberality sent me three or four specimens.

Among them was one which in its appearance differed from any British blende that I am familiar with. It was in small mamillated globules, brown, with a peculiar greasy lustre. Having a suspicion that possibly it might not be a blende, I submitted it for examination to that Grand Inquisitor the spectroscope.

Letting a bright light fall on the specimen, I looked at it with a pocket-spectroscope, when immediately the bands due to the presence of didymium became apparent. I had thus come across what, at least, was no ordinary blende.

Having submitted the specimen to Mr. Maskelyne, that gentleman was so good as to cause a preliminary chemical examination of the mineral to be made, from which it appears that it contains neither zinc nor sulphur, and is consequently no blende, but that it is *essentially* a phosphate of didymium.

With some pure fragments of the mineral which I have placed in the skilful hands of Mr. Walter Noel Hartley, that gentleman has kindly undertaken to make a complete analysis†

* Communicated by the Crystallological Society, having been read November 23, 1878.

[This paper was withheld from publication with the intention that it and the analysis should appear simultaneously. Through some misapprehension this intention has not been carried out, and the analysis has already appeared in the Journal of the Chemical Society for May.—Sec. Cryst. Soc.]

† [Professor Hartley's analysis shows it to be a hydrated phosphate of cerium, didymium, and yttrium, capable of representation by the formula $R_2O_3 . P_2O_5 . 2H_2O$, the percentage composition found being :—

Combined water	7·97
P_2O_5	26·26
Ce_2O_3, Di_2O_3, Yt_2O_3 ..	65·75

$Ce_2O_3 : Di_2O_3 : Yt_2O_3 = 23·19 : 34·77 : 2·39$ nearly.

This seems to be the first time that yttrium has been found in a British mineral.]

of the substance ; but as he informs me he has no leisure to go into the matter at present, I am not as yet able to state to the Society what is the precise constitution of the mineral.

Within a few days after showing the original specimen to Mr. Maskelyne, I had the pleasure of learning from him that he had been enabled, I believe by means of the spectroscope, to recognize two other specimens of the mineral in the old collection at Oxford, where they are simply labelled "Blende from Cornwall"*.

On my showing Mr. Ludlam the specimen I had received from him, he informed me that it came from "the Turner Collection," which is now in his possession—and that it is described in the third volume of M. Lévy's Catalogue of that Collection, under the heading of "*Zinc sulfuré No. 75. Mamelonné, brun, fibreux dans la cassure, ressemblant au plomb-gomme, avec plomb phosphaté, Cornouailles.*" I should not say the specimen resembles plomb-gomme much; neither have I observed any phosphate of lead upon it.

There were two specimens of the mineral in the Turner Collection as well as in the collection at Oxford, the locality attributed to them all being simply "Cornwall;" and as M. Lévy's Catalogue was published in 1837 (that is to say, more than forty years ago), I fear there is but little hope of ascertaining at what mine they were raised: even, however, if that were made out, it seems improbable the workings there should be still carried on.

Neither in the collection at the British Museum, free access to which has been kindly granted to me, nor in that at King's College, nor in the old collection belonging to Mr. Vicary at Exeter, nor in that of the late Sir John Aubyn at Devonport have I been able to find a specimen of the mineral in question.

The name Rhabdophane, which this species has received, was selected as one indicating the stripes or bands which it exhibits in the spectroscope.

For the information of such of our chemists as may feel disposed to work upon British blendes for gallium, I here subjoin the results that were obtained by M. Lecoq de Boisbaudran in operating upon some of those specimens that I forwarded to him. The quantity of material used for the examination was, I believe, in every case 20 grammes.

No. 1. Redruth . .	Poor enough.
No. 2. Cumberland. .	Rich enough.

* There is a small specimen without label in the Brooke Collection at Cambridge.

No. 3. Derbyshire . .	No gallium.
No. 4. Cornwall (?) .	Rich enough.
No. 5. Weardale . .	Rich in gallium. Seems to be a little richer than the preceding.
No. 6. Fowey Consols.	Rich. Seems to be somewhat richer still than the preceding.

M. Lecoq de Boisbaudran adds that some of the English blendes thus appear to him to be at least as rich in gallium as the Bensberg blende is, and that, judging from the intensity of the gallium-lines shown, he is inclined to think the Fowey-Consols blende is even superior to that from Bensberg.

I should add that the Fowey-Consols blende here spoken of is the well-known mamillated nodular material of that locality.

Should any member of the Society wish to see a portion of the original specimen, or to examine with the spectroscope sections thereof or of the Oxford mineral, I am prepared to submit them to him.

LXV. *On the Dichroism of two European Andalusites.* *By* W. G. Lettsom, *Esq.**

TWO years ago or so there was received in London from Brazil a batch of Andalusites, the transparency of which allowed of their remarkable dichroism being well observed. This induced me to make trial as to the amount of dichroism which Andalusites from European localities might exhibit if suitably cut by a lapidary; for none of our Andalusites that I am acquainted with are capable of being examined, as is the case with Brazilian specimens, in their natural state.

I beg leave to lay before the Society a few sections of Andalusite from a locality in Germany the name of which I have not at hand, but which I hope to obtain in a day or two. Those sections show in one image a rich chocolate-brown colour, the other being all but colourless.

Other sections, from Goldenstein in Moravia, exhibit, in one image a deep blood-red colour, the other image in this case too being almost colourless.

* Communicated by the Crystallological Society, having been read November 23, 1878.

LXVI. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* *By* C. K. Wead*.

IN the May number of this Magazine, p. 376, Dr. Everett presents Clausius's deduction of the dimensions of a magnetic pole in the electrostatic system, finding them to be $P_e = M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-2}$, instead of $M^{\frac{1}{2}} L^{\frac{1}{2}}$, as Maxwell gave it without explicit statement of the method of derivation. In Maxwell's treatise this unit is obtained, along with all the rest, from fifteen simultaneous equations involving twelve unknown quantities; the substitution of the final results in the original equations shows that the suggestion of a misprint may be set aside at once.

In the smaller books that use the clearer method of deriving the dimensions step by step, the development does not usually include magnetic quantities under the electrostatic system: they are not found in the British-Association Reports on Electrical Standards, Reprint, p. 80, nor in Kohlrausch's 'Physical Measurements,' nor in Everett's 'Units and Physical Constants.' Only in Herwig's *Physikalische Begriffe*, p. 78, do I find any values for intensity of a magnetic field, moment of a magnet (strength of pole is involved in this, but not given separately), and magnetic potential in this system. An examination of his method, which leads to Maxwell's results, shows at once that the whole question turns on the step, or steps, between current-strength and magnetic moment. Using so far as possible Everett's notation, but with the subscript letter e or m to distinguish the two systems when necessary, we shall have to consider Strength of current C, Intensity of a magnetic field I, Moment of a magnet μ or PL, Strength of magnetic pole P, Quantity of electricity Q.

Herwig gives $I^e = \frac{2\pi r^2 C^e}{L^3}$; and since $C_e = M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2}$ (agreeing with Everett), $I_e = \frac{C_e}{L} = M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-2}$.

Then, since a magnet whose moment μ in the field I is subject to a couple, $I_e \mu_e =$ a couple,

$$\mu_e, \text{ or } P_e L = \frac{M L^2 T^{-2}}{M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-2}} = M^{\frac{1}{2}} L^{\frac{3}{2}};$$

$$P_e = M^{\frac{1}{2}} L^{\frac{1}{2}};$$

$$V_e = \frac{\text{work}}{P_e} = \frac{M L^{\frac{1}{2}} T^{-2}}{M^{\frac{1}{2}} L^{\frac{1}{2}}} = M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1} = \text{magnetic potential.}$$

* Communicated by the Author.

Clausius gives "$C \times L^2 = P \times L$ in any consistent system;" so

$$P_e = \frac{C_e L^2}{L} = \frac{L^2 \times M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2}}{L} = M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-2}.$$

The equation $C \times L^2 = P \times L$ leads to $\frac{P}{L^2} = \frac{C}{L}$; which would in the electromagnetic system be consistent, since each member $= I$. Again, $PL = \mu$; that is, PL must represent a magnet; consequently $C \times L^2$ is put equal to a magnet. But the passage of the current ordinarily produces effects, such as the movement of a galvanometer-needle, which we explain more naturally by saying that the circular current produces a magnetic field at its centre, than by saying that the current is, or makes, or even is equivalent to, a magnet. Herwig's derivation, therefore, in which a magnet placed in a field experiences a couple, conforms to the ordinary way of thinking better than the way of Clausius, and is the way used in the derivation of the electromagnetic system. Again, if

$$P_e = M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-2},$$

then

$$I_e P_e L = M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-2} . M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-2} . L = ML^4 T^{-4} = \text{a couple} \times L^2 T^{-2}.$$

How can this result be explained, consistently with the known effect of a circular current on a magnet? It may be noted that, in all discussions except that of Clausius, the magnet pole P is introduced into a field. Clausius produces a field by the pole P. Also that the ratio of the two values of P is the square of a velocity.

Herwig says, with regard to the step from I to μ in the electrostatic system:—"It may be remarked that for this purpose we cannot use the formula of § 64, $\mu = \pi r^2 C$, which expresses the relation between the magnetic moment μ and the current-strength C; for the validity of this formula is dependent (*geknüpft*) on the use of the magnetic system" (p. 78).

A comparison of the derivation of the same units in the two systems will strengthen our belief in the view, that Clausius has proposed a new way, for which, rather than for the older one, a justification is needed. Taking the least number of steps that will lead from P to Q, or *vice versâ*, we have these equations in each system:—

Magnetic,

$$\frac{PP}{L^2} = F(=\text{a force}). \quad PL = \mu; \; I\mu = \text{a couple}; \; \frac{CL}{L^2} = I;$$

$$Q = CT. \quad . \quad . \quad (1)$$

Electrostatic,

$$\frac{QQ}{L^2}=F.\quad \frac{Q}{T}=C;\quad \frac{CL}{L^2}=I;\quad I\mu=\text{a couple};\quad \mu=PL. \quad (2)$$

Electrostatic (Clausius),

$$\frac{QQ}{L^2}=F.\quad \frac{Q}{T}=C;\quad CL^2=\mu=PL. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

It is obvious at a glance that, after the fundamental operation has been performed of finding P_m or Q_e, the remaining operations take exactly the opposite order in the two systems; the corresponding equations in the two are the same; while this is not true of series (1) and (3). In (1) we may find the dimensions of Q in terms of P, and from (2) and (3) of P in terms of Q. The results are:—

$$Q_m=ML^2T^{-1}\div P_m, \quad . \quad . \quad . \quad . \quad . \quad (1')$$

$$P_e=ML^2T^{-1}\div Q_e, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2')$$

$$P_e=LT^{-1}\div Q_e. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3')$$

Since in the two systems P and Q play similar parts, are we not justified in expecting such a symmetry as is shown by (1′) and (2′), rather than the want of it shown in (1′) and (3′)? If the values of P_m and Q_e, $M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1}$ are substituted in the above, we may group the results thus:—

$$P_m=M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1},\quad Q_e=M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1};$$

$$Q_m=M^{\frac{1}{2}}L^{\frac{1}{2}},\qquad P_e=M^{\frac{1}{2}}L^{\frac{1}{2}}\ (2'),\ \text{or}\ M^{\frac{1}{2}}L^{\frac{5}{2}}T^{-2}\ (3').$$

Of course these are not presented as three independent lines of argument in favour of Maxwell's view, as they are all involved in the series of operations (1), (2), and (3).

It will now be shown that to write "$C\times L^2=PL$ in any consistent system" is simply begging the whole question.

No physical formulæ are better established than those used in finding with the magnetometer M and H, the moment of a magnet and the horizontal component of the earth's magnetism:—

$$MH=\frac{\pi K}{t^2(1+\theta)};\quad \frac{M}{H}=\frac{1}{2}\frac{\gamma^5\tan\phi-\gamma'^5\tan\phi'}{\gamma^2-\gamma'^2};$$

in which K = a moment of inertia = ML^2, t = a time = T, γ and γ' are lengths = L, and the other quantities in the second members are numerical. In the usual notation, therefore, $\mu I=ML^2T^{-2}$, $\mu\div I=L^3$; whence we find the dimensions of $\mu=M^{\frac{1}{2}}L^{\frac{5}{2}}T^{-1}$, and of $I=M^{\frac{1}{2}}L^{-\frac{1}{2}}T^{-1}$, the same as in the magnetic system. But one has the same right to declare that "in any consistent system" both the above formulæ must be

true as that $C \times L^2 = P \times L$. In the magnetic system both are true; in the electrostatic system with Maxwell's value the first is true, the second is not; with Clausius's values the second, and not the first. Maxwell (or at least Herwig) openly uses the first; Clausius impliedly uses the second; for if $CL^2 = PL = \mu$, and $C \div L = I$,

$$(C \div L) \times L^3 = \mu, \ \therefore \ \mu \div I = L^3.$$

In deducing the dimensions of physical quantities, there is much that is as arbitrary as the order in which several numbers shall be multiplied together*. Thus, the familiar equation $I = k\frac{2\pi r^2 C}{(l^2 + d_2)^{\frac{3}{2}}}$ is true in any conceivable system of units, where r is the radius of the current-circle, and d the distance from the centre on a normal to the plane. In the electrostatic and electromagnetic systems, the dimensions of either C or I being given to find the other, r, l, and d being lengths, k is arbitrarily made equal to 1, and then I or C is found: if I and C had both been given, ordinarily its value would not be 1, nor its dimensions $= L^0 M^0 T^0$. To pass from I to P or to $PL = \mu$, either of two equations may be used:—

$$I = kP \div l^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\mu I = k' \times \text{a couple}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Clausius arbitrarily makes $k = 1$ in (1), letting k' assume whatever concrete value will satisfy (2). Herwig, to obtain Maxwell's result, as arbitrarily makes $k' = 1$, paying no attention to k. If arbitrarily we make $k = k' = 1$, μ and I must come out as in the magnetic system.

In making $k' = 1$ rather than k, there is the advantage of introducing a mechanical unit; and we use the equation (2) that is both more familiar in experimental work, and the one used in the derivation of the magnetic system. Further, if P be changed, three other quantities of the twelve that Maxwell discusses must have their dimensions changed, and confusion would be introduced into his system, that is based on fifteen equations, in each of which the second member is some simple mechanical quantity, as work, time, &c. Until it has been clearly shown how this system will be affected by the proposed change, and why the new expression is to be preferred to the older one, that has been "unimpeached" for some twenty years, is it not clearly better to write $P_e = M^{\frac{1}{2}} L^{\frac{1}{2}}$? It is not a question merely of correctness, but of consistency, simplicity, and usefulness; and on all these grounds Maxwell's expression seems to the writer to deserve the preference.

University of Michigan,
Ann Arbor, May 29, 1882.

* On this point, compare Everett's 'Deschanel,' p. 783.

LXVII. *Notices respecting New Books.*

Magnetism and Electricity; an elementary Text-book for Students. By Richard Wormell, *D.Sc.*, *M.A.* London: Thomas Murby.

THIS book is one of a series—the High School Science series. The author informs us that he has aimed at embodying two characteristic features—*first*, to make the work progressive in several respects from beginning to end, and, *secondly*, to introduce the explanation of the latest discoveries and applications of electricity. The latter feature will be commended by every one, but the former will appear to many to be of doubtful utility. Probably it is on that account that the book is thrown into the form of lectures. But are not the functions of a text-book and of lectures different?

A text-book ought to be clear in statement, exact in information, and free from error. We cannot say that these conditions are completely satisfied by Dr. Wormell's book. As regards the first point, there is frequently an ambiguous use of the pronoun *it*, which is not only inelegant, but causes the reader some pains to find out what is meant. For example, "But after the pith-ball has been in contact with the glass rod, it no longer comes towards it, but flies away" (p. 75). "If we rub the glass with silk, then balance it and present the finger to it, it is attracted" (p. 78). "On presenting the knuckle to the cover A, after it is lifted off, a spark of positive electricity passes to it, and the plate is discharged" (p. 88). "If a bar magnet is brought near this loop, it will be made to turn, following the bar magnet, or being repelled from it" (p. 31). The *Carrier* is described as follows:—"This consists of a metallic ball, a wooden ball covered with tin-foil, a gold-leaf, or some similarly conducting material, attached either to a glass handle or suspended by a silk thread, in order to insulate it." A tyro in electricity is not likely to discern the meaning of this description at first sight.

Owing, probably, to the progressive character of the book, many of the statements are not exact enough. Is there any advantage in making a science simpler than it really is? For example, it is said of the Carrier (*loc. cit.*):—"The charge of electricity brought by the carrier is of the same kind as that of the body to be tested, and affects the electroscope exactly in the same way as if the body itself were brought near it." At pp. 52 and 71 magnets and magnetic poles are mixed up in a manner which will give the student some trouble to unravel. What is meant by "placing two magnets at unit distance apart"? We have the analogy between *potential* and *level* carried a little further than usual, thus:—"The electrical capacity of any body may be measured by the quantity of electricity required to charge it to a given potential: just as the capacity of a cistern for water may be measured by the quantity of water required to fill it to a given level" (p. 111).

As regards the third point, we have at p. 51 a contradiction within the compass of a paragraph. "If we could increase the weight of the bob without altering the amount of substance composing it, we should lessen the number of oscillations per minute:"

and "If we wish to double the number of oscillations per minute we must multiply the force by four, etc."

As regards the woodcuts, we notice one very commendable feature—namely the introduction of a symbolical figure to represent a battery, electrometer, or other instrument frequently occurring in experimental arrangements. In the description of Winter's electrical machine, reference is made more than once to a knob P; but in the woodcut no P is discernible.

The Calendar of the Departments of Law, Science, and Literature of the Tokio Dai-Gaku (University of Tokio), 1880–81.

The scientific papers of the Tokio University have been previously noticed (Phil. Mag. vol. xiii. p. 367, May 1882); and the above work will show the interest and importance which the Japanese government attach to the progress of education by the establishment of the Tokio Dai-Gaku or University, which could not have attained to its present state of progress had it not been favoured by the generosity of the Government, in devoting a large amount of money for instruction in the higher branches of study; and it is the intention of the authorities connected with it, while pursuing its original object, to still further make such possible improvements that the standard of scholarship attained in it may equal that of the Universities of Western countries.

The historical summary contains a brief sketch of the rise and progress of education from the introduction of Western learning in the early part of the last century, when medical science and astronomy became gradually known to the people: and subsequently to 1744 three observatories were established, two at Asakusa and one on the hill at Kudan, in which astronomical observations were made, and an almanac was compiled.

In the beginning of the present century, about 1811, it was considered a pressing necessity to understand the condition of foreign nations, and to adopt such of the better arts as they might have. Accordingly Dutch works were first translated and the Dutch language taught, to which, in 1857, English, French, German, and Russian were added, and courses of Mathematics, Chemistry, and Botany successively established.

After this time, in 1862, various modifications and improvements in the educational department took place; instruction in German, French, and English was given; but in 1875 all this was altered, and the latter language was only to be used. Students, however, were sent to other countries to continue their studies.

Since the first permanent establishment of the Tokio Dai-Gaku various modifications have been made, until it has arrived at its present state, which embraces the departments of the Law, Science, Literature, and Medicine.

Although the special courses of instruction are mainly given in the English language, and the student is taught either French or German, it is the intention ultimately to use the Japanese language in all the Departments.

LXVIII. *Intelligence and Miscellaneous Articles.*

ON THE EFFECTS PRODUCED IN A VACUUM BY THE CURRENT OF THE GRAMME MACHINES. BY M. JAMIN.

AS soon as Davy had discovered the electric arc he hastened to reproduce it in an enclosure void of air, in order to avoid the combustion of the carbons. He saw that it could be enlarged, that the carbonaceous material was carried from the positive to the negative pole, that the former was more luminous than the latter, but that the general aspect of the phenomenon did not change. Despretz repeated the experiment with a more energetic battery, and ascertained that the carbon volatilized under the electric action was deposited on the sides of the glass balloon as soon as the intensity of the current exceeded a certain limit. It is probable that this deposition of carbon takes place in all cases, but does not begin to be ascertainable until the intensity is very great.

Things happen quite otherwise when instead of a battery a Ruhmkorff induction-coil is employed. It is known that it gives rise to successive induction-currents in opposite directions: the first currents, inverse, have too little tension to clear the distance separating the points, and produce nothing; the second, direct, acquire at last an enormous tension, but have little duration and transport but a slight quantity of electricity. The whole therefore reduces to successive almost instantaneous discharges separated by comparatively prolonged intervals of rest, to sparks, and *in vacuo* to emanations of light; but there is never an electric arc, because the quantity of electricity is insufficient.

Gramme machines with alternating currents participate at the same time in the properties of batteries and the induction-coil, while presenting some special characters: they give two alternately contrary currents; but these are equal, last the same time, are separated by only a brief interval, and transport, on account of their duration, a large sum of electricity. They could be replaced by a battery the direction of which changed periodically and without interruption; but they differ from a battery by the great intensity of their currents: when one of these is ending it is augmented by the extra current; when commencing, it profits by the inverse electromotive force of the preceding current, as I have demonstrated*. In short, the effects will be those of batteries with an aggravation due to the enormous tension, and those of the induction-coil with the aggravation resulting from a larger quantity of electricity. This is verified by experiment. In air, instead of a single arc, several can be lighted; and the number of them is multiplied by increasing the tension—that is, by diminishing the section and increasing the length of the induced wire as well as the velocity of the rotation. This has permitted me to maintain sixty

* *Comptes Rendus*, t. xcii. p. 1201.

lights by a machine which originally supported only eight. The results in the imperfect vacuum of an air-pump might yet have been in part foreseen; and they are very curious, as you will see, and as I have just observed with the cooperation of one of my pupils, M. G. Maneuvrier.

Taking an electric egg, I placed in it, face to face, two carbons of 15 centim. length, 4 millim. diameter, separated by an interval of 4 millim. When the exhaustion reached about 12 millim. the light began to spirt out spontaneously, not in the form of a brilliant arc confined between the points, but starting from all parts of the carbons, with the ordinary appearance of the emanations in Geissler's tubes. Each of them showed, at the same time, the appearances which, with the induction-coil, characterize the two poles—that is to say, the blue aureole enveloping the surface of the two cylinders, which belongs to the negative pole, and, besides, the paler stratified light due to the positive pole. It shot forth normal to the outline of the carbons; and the whole balloon was filled with light. The experiment is one of the most beautiful to be seen.

Thus the two currents contribute each an equal share to the production of the phenomenon, which is that of the Geissler tubes, but which assumes an incomparable splendour because of the considerable quantity of electricity that passes and rapidly modifies the observed appearances. In fact the carbons become heated, redden, and arrive at a pale whiteness, not only at their extremity, but throughout their length; they are then rapidly volatilized by the combined effect of the heating and the currents. Whatever may be the cause of this volatilization, it is certain that a carbonaceous material is diffused in the state of vapour. The balloon becomes filled with a blue gas very similar to iodine vapour, which deepens more and more in colour to indigo. After this the vapours condense abundantly upon the sides of the balloon, which become opaque; and this terminates the experiment. The deposit collected resembles finely divided carbon, and dissolves with effervescence and incandescence in nitric acid. Is it pure carbon, or a hydrogenated compound of carbon? That is a question which I reserve for ulterior examination.

To avoid this forced termination of the experiment, I have replaced each carbon by two equal bundles of crayons fitted to the two rheophores, and diverging in the direction of the generatrices of two cones with their bases opposite. In this case the currents divide into a great number of emanations less intense than the single one; all the carbons become luminous at once; and the more numerous they are, the less heated do they become. There is almost no volatilization; and the effect is prolonged as much as one pleases.

I have replaced the carbons by copper rods of 3 millim. diameter and diverging from the rheophore. The same effect is produced with still greater splendour. If the current is too intense, the copper melts. It is always in part volatilized, and is deposited in a thin coat upon the balloon. It is evident that the experiment

will be varied by employing different metals, different gases, and working under different pressures; I shall have the honour of making known to the Academy the results.—*Comptes Rendus de l'Académie des Sciences*, May 8, 1882, t. xciv. pp. 1271–1273.

ON THE EMPLOYMENT OF ROTATING DISKS FOR THE STUDY OF COLOURED SENSATIONS. THE RELATIVE INTENSITY OF COLOURS. BY A. ROSENSTIEHL.

In continuing my researches upon the coloured sensations, I have been led to examine a problem which has hitherto appeared inaccessible—that of the relative intensity of colours. What common measure can there be between a red and a yellow, a blue and a violet? It seems at first sight impossible to set up a numerical comparison between values which appear so different in kind; and yet this comparison is possible, thanks to a well-known special property of the eye.

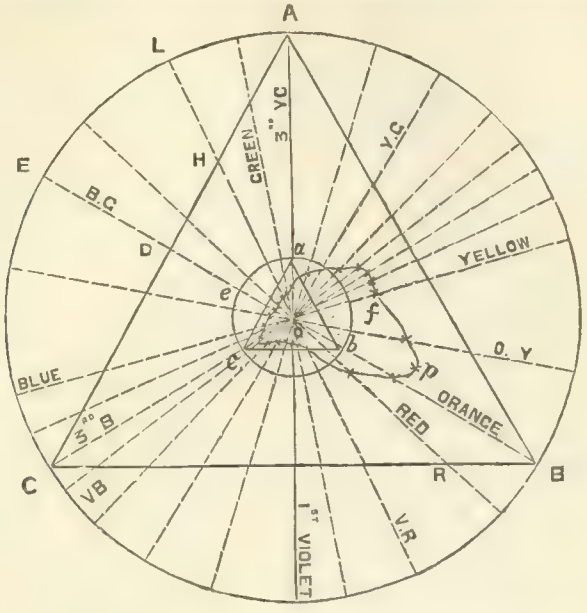
Between the different colours which have received names an infinity can be intercalated which form the insensible transition from the one to the other without a break. The series is continuous, and closes upon itself like the perimeter of a polygon. Besides (and I cannot sufficiently insist upon the peculiarity), this whole includes an infinity of sensations which, associated in twos, produce a sensation identical for all, *that of white*. We find nothing analogous in the sensations of the ear; and it is this highly characteristic circumstance that gives a common measure between the different colours.

In my previous Notes, adopting and precisely defining Young's theory, I laid it down as a principle that the sensation of white results from the *equal* excitation of three primary sensations. Consequently every pair of complementary colours represents in its combination the mixture of the three primary colours *at equal intensity*. This notion of equal intensity was introduced into a graphic construction, which was discussed; and I verified the principles on which it is based.

It is necessary to distinguish between intensity of coloration and total luminous intensity. By adding the sensation of white in different proportions to a coloured sensation of constant intensity, colours of the same intensity of coloration, but of different total luminous intensity, are obtained. These colours have for their common measure the angle of the sector of the complementary necessary to extinguish all coloured sensation; and, generally, *the angle of the sector of the complementary colour is in the inverse ratio of its coloration-intensity. The angle of the white sector* * *is in the direct ratio of the total luminous intensity.*

* It will be remembered that, in all my experiments, the intensity of the sensation of white is measured by the angle of a sector painted white with sulphate of barium, which is set in rapid rotation in front of an absolutely black orifice.

When the colours have not the same complementary, the graphic construction is indispensable for the determination of the intensity.



From the point O, I draw three lines OA=OB=OC, the length of which is proportional to the intensity of the colours A, B, C, which I regard as primary sensations, and which, for that reason, are placed at an angular distance of 120° from one another. The places of the colours resulting from the mixture, in twos, of the three primary sensations are necessarily upon the sides of the triangle ABC. Their distances from the vertices are in the inverse ratio of the coloured sectors necessary to produce them; and their distances from the point O represent their coloration-intensities. In this conception the point O is not regarded as the centre of gravity of the triangle; it represents not the place of the white, but the *absolute black*.

The intensity of the binary colours being given by the construction, it is easy, with the aid of rotating disks, to compare with it the coloured surfaces the intensity of which we wish to know.

Let there be required to find the intensity of coloration of a red surface S. I mix C and B so as to obtain red, which I reproduce, in another part, with a sector S, of which the angle α is given by experiment. Let R (in the figure) be the point occupied by the red; we have

$$\frac{\mathrm{OR}}{\mathrm{OB}} = \mathrm{I}\,\frac{\alpha}{360},$$

from which we get for the intensity of coloration I of the surface S

$$\mathrm{I} = \frac{\mathrm{OR} \times 360}{\alpha \times \mathrm{OB}}.$$

This method is susceptible of verification. Knowing the in-

tensities of two complementary colours, we know the ratio of their intensities. From this ratio we deduce the angles of the sectors which ought to reproduce the sensation of white—a condition easily verified by direct experiment. The following Table contains the result of this measurement for ten pairs of complementary colours copied upon the chromolithographed circle by M. Digeon, from the chromatic circle of M. Chevreul, whose nomenclature I have retained.

	Ratio of the coloration-intensities, given	
	by the construction.	by direct experiment.
3rd yellow-green and violet	1·17	1·25
4th green and red	3·72	3·73
Blue-green and orange	6·80	6·70
1st blue-green and orange-yellow ..	6·28	6·20
Blue and yellow	3·74	3·80
2nd blue and 1st yellow.....	3·13	3·18
3rd blue and complementary yellow	3·07	3·00
4th blue and 2nd yellow.....	2·16	2·21
Violet-blue and 4th yellow.....	2·16	2·13
2nd violet-blue and yellow-green ..	2·18	2·00

The accordance between experiment and construction is one of the most complete, so that the latter can be regarded as summing up exactly the state of our knowledge respecting the mixing of colours. It gives for each the proportion of the primary sensations CR : RB, the coloration-intensity OR, the complementary V, the ratio of the sectors of a pair of complementary colours $\frac{OR}{OV}$ necessary to produce the sensation of white, the intensity of that sensation, the total luminous intensity, the triads, &c.

Having obtained all these concordant numerical results, I could not resist the desire to advance a step further and examine if it was possible to express the intensity of a colour by its ratio to that of white, and thereby to learn what is the distance which still separates the finest colours obtained with colouring-matters from those which the eye is susceptible of perceiving.

In regard to the total luminous intensity that ratio has been exactly determined. The finest aniline-blue represents only one fifteenth, and the chromates of lead one seventh, of the intensity of the white obtained, for one and the same illumination, with barium sulphate. For the coloration-intensity the difference would be still greater; and hence it is evident that the chemistry of colouring-matters owes us yet much progress before it can realize what light can give us and what the eye is capable of perceiving.—*Comptes Rendus de l'Académie des Sciences*, May 22, 1882, t. xciv. pp. 1411–1414.

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WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta: contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATA.

Page 217, line 10, for $\frac{dy^2}{dx^2}$ put $\frac{d^2y}{dx^2}$.

— 221, line 4, *for* $\cos(2\omega+2\delta)$ *put* $\sin(2\omega+2\delta)$.

— 222, line 4, *for* $\cos^2\delta$ *put* $\cos^2 2\delta$.

— 223, line 30, for η read $\frac{g}{a\mu}\eta$ bis.

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II. Illustrative of Mr. F. D. Brown's Notes on Thermometry.

III. Illustrative of Mr. W. J. Lewis's Crystallographic Notes.

IV. Illustrative of MM. Elster and Geitel's Paper on the Electricity of Flame, and M. H. Brongersma's on Double Refraction, produced by Electrical Influence, in Glass and Bisulphide of Carbon.

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[FIFTH SERIES.]

JULY 1882.

I. *Researches on Melting-point.* By EDMUND J. MILLS, *D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College Glasgow*.*

IF we desire to ascertain the purity of a chemical substance —in other words, to establish its species—two lines of investigation are open to us. We may (1) by analytical means determine the composition, or (2) measure by physical methods some natural property of the body. Both modes of research require that what is determined—whether composition or natural property—shall be constant over a fair range of genetic condition. Both modes also involve a considerable amount of inference ; but, of the two, far less is demanded by the methods of physical science, which are, as a rule, distinguished by their greater certainty because of their experimental directness. To the property of melting, these characteristics are preeminently applicable.

The accurate ascertainment of melting-point, in terms of the air-thermometer, supplies us with physical constants of considerable importance. While the readings of the mercurial thermometer are subject to grave correction—its zero, in particular, being never stationary,—it is most unlikely that the melting-point of a substance will vary in any ordinary interval of time, under any common change of pressure, or with transition to a distant latitude. Actuated chiefly by these considerations, I undertook the researches of which an account is comprised in the following sections.

* Communicated by the Author.

A. *Benzol Derivatives.*

1. *Dinitrobenzol.*—The benzol from which specimens A and C were prepared had been purified by myself, first by fractional distillation, then by fractional distillation after treatment with bromine, and, lastly, by freezing and pressure. The preparation of the dinitro-compound was effected with the aid of hydric sulphate. The crude product was washed with water, alkaline water, and water successively, and then submitted to a process of purification repeatedly adopted in these experiments.

[The process in question consists in crystallization from *distinct successive solvents*, followed each time by pressure. It is well known that small quantities of impurity are prone to cling to substances with much tenacity; but the observation has most frequently been made in connexion with some single solvent. One can readily conceive that the tenacity with which a given trace of a foreign body is held may, under such circumstances, be in effect constant. If, however, we now transfer the mixture to a new solvent, then, we may fairly presume, the trace will be in a condition of altered adhesiveness, and may be much more readily separable. It is of course not easy to decide whether this result is always attained, or whether the later solvent makes any impression at all; but it will probably be granted that the method of multiple successive solvents is in general expedient to adopt.

After each crystallization the crystals were in all cases submitted to powerful pressure between folds of carefully cleansed linen or, occasionally, of silk tissue. The surface-tension of the dissolved impurities appears to be for the most part considerable; and this, aided by mechanical compression, greatly economizes the time required in preparing a pure substance.

The pressed crystals were next reduced to a very fine powder, and dried for eleven days over oil of vitriol in the dark. Care was taken never to make determinations with substances previously melted; for it not unfrequently happens that a second melting takes place at an appreciably different temperature.]

Specimens A and C were crystallized twice from naphtha, and thrice &c. (A_3 &c., C_3 &c.) from purified alcohol. Specimen B was given me by Mr. C. E. Groves, who had prepared it from thrice frozen and pressed benzol. It was crystallized once from water, once from naphtha, once &c. (B_1 &c.) from pure alcohol. The results are contained in Table I.

Dinitrobenzol melts with some sharpness after a decided pasty stage. It strongly resists pulverization—a character more especially observable in specimen B.

TABLE I.

	A_3.	A_4.	B_1.	B_2.	B_3.	C_3.	C_4.
	89°86	89°71	89°79	89°75	89°64	89°73	89°67
	·75	·71	·82	·78	·71	·62	·59
	·75	·74	·71	·70	·66	·73	·62
	·78	·68	·71	·70	·64	·76	·67
	·73	·84	·66	·70	·66	·73	·64
	·75	·76	·68	·70	·74	·70	·73
	·81	·82	·79	·67	·71	·76	·64
	·78	·79	·66	·80	·69	·73	·62
	·81	·84	·71	·78	·71	·65	·75
	·67	·84	·68	·70	·74	·78	·70
	·75	·79	·79	·70	·71	·76	·62
	·59	·82	·76	·83	·62	·73	·78
	·81	·82	·66	·72	·64	·68	·70
	·83	·71	·74	·72	.....	·65	·59
Mean	89·76	89·78	89·73	89·73	89·68	89·71	89·67
Probable error ...	·012	·010	·010	·008	·007	·008	·010
Thermometer ...	2	2	2	2	3	2	2

2. *Dichlorobenzol.*—For this specimen I was indebted to Dr. Hugo Müller, who prepared it by his well-known process of chlorination, which consists in treating hydrogenated bodies with chlorine in presence of iodine. It was crystallized twice from naphtha, and four times &c. (A_4 &c.) from alcohol. Dichlorobenzol melts very suddenly.

TABLE II.

	A_1.	A_5.	A_6.	A_7.	A_8.	A_9.	A_{10}.
	52°61	52°72	52°66	52°74	52°74	52°74	52°83
	·66	·72	·71	·72	·72	·76	·75
	·74	·70	·71	·69	·69	·72	·75
	·71	·67	·74	·69	·74	·66	·75
	·74	·75	·79	·72	·74	·72	·78
	·79	·72	·74	·69	·72	·72	·73
	·74	·75	·76	·69	·69	·64	·73
	·71	·72	·79	·72	·74	·76	·70
	·76	·72	·71	·66	·76	·76	·75
	·71	·75	·71	·69	·69	·76	·73
	·69	·77	·68	·69	·76	·76	·73
	·76	·75	·74	·72	·72	·74	·67
	·74	·72	·74	·69	·74	·72	·75
	·79	·75	·74	·66	·76	·74	·73
Mean	52·72	52·73	52·73	52·70	52·73	52·73	52·74
Probable error ...	·008	·004	·006	·004	·004	·006	·006
Thermometer ...	3	3	3	3	3	3	2

3. *Dibromobenzol.*—A was a sample prepared by heating

benzol (permanently reddened by bromine) for a short time nearly to boiling, washing and rectifying the product. The benzol thus purified was used in preparing benzol by Couper's process*; and from this product, on distillation, a residue of dibromobenzol was obtained. This was crystallized once from spirit, twice from naphtha, and thrice from spirit; at the last crystallization the substance was only partly dissolved; and the crystals obtained from the solution were alone examined. C was prepared from the monobromobenzol above referred to. The product of Couper's process always contains a little dibromobenzol, which cannot be removed in the ordinary way. This was allowed to remain. Traces of another solid impurity were precipitated by exposure to sunlight for some weeks in presence of solid potassic hydrate, followed by filtration and distillation. Finally the purified monobromobenzol was mixed with bromine in the proportion $C_6H_5Br : Br_2$, and left for some weeks in the laboratory, during which it was once heated in the water-bath. The product was washed with aqueous caustic soda, some unaltered monobromobenzol removed by partial distillation with water, and the remainder crystallized once from alcohol, once from naphtha, and once from alcohol in presence of charcoal (C_1): successive fractions (C_2 &c.) from alcohol were then taken. $C_{6\alpha}$ was obtained from a hot liquid which had deposited about half its contents, which I have named $C_{6\beta}$.

Dibromobenzol melts nearly as sharply as dichlorobenzol. It is hardly possible to trace a pasty stage.

Table III.

	A.	A.	C_1.	C_2.	C_3.	C_4.	C_5.	$C_{6\alpha}$.	$C_{6\beta}$.
	87·08	87·05	87·10	86·97	87·06	87·10	87·04	87·06	87·10
	87·03	87·07	86·99	87·08	87·01	87·05	87·01	87·06	87·05
	86·87	87·03	87·12	86·97	87·03	87·10	87·01	87·03	86·94
	86·97	87·05	86·99	87·16	87·03	87·10	87·01	86·98	87·07
	87·00	87·01	87·10	87·13	87·11	87·05	87·01	87·09	87·07
	86·97	86·96	87·07	86·99	87·01	87·07	87·01	87·06	87·07
	86·95	87·01	86·97	87·08	87·01	87·04	86·96	87·06	87·05
	87·03	87·01	87·12	87·13	86·98	87·06	87·01	87·06	87·05
	87·03	87·05	86·99	87·05	87·11	87·15	86·99	87·06	87·02
	87·08	87·10	87·02	87·05	87·03	87·07	87·04	87·11	87·02
	87·00	87·01	86·99	87·10	87·01	87·13	86·99	87·03	87·02
	87·03	87·03	87·12	87·08	87·03	87·04	86·99	87·10	87·02
	86·97	87·12							
	87·03	87·09							
Mean	87·00	87·04	87·05	87·06	87·04	87·08	87·01	87·06	87·04
Probable error.	·010	·007	·011	·012	·007	·007	·004	·006	·008
Thermometer ..	2	4	2	2	2	2	2	2	2

* *Ann. Chim. Phys.* [3] lii. p. 309.

4. *Dinitrobromobenzol.*—Benzol purified by bromine distillation, and freezing, was brominated by the Couper process, and freed from dibromobenzol by heating to the boiling-point with a mixture of Nordhausen and common oil of vitriol; this treatment was followed by washing and distillation.

Sample A was prepared from the above bromobenzol by Kekulé's method*. The crude product amounted to 148 per cent., theory requiring 157 per cent.: it contained a small quantity of an oily body. This was crystallized twice from naphtha and five times &c. (A_5 &c.) from alcohol.

The crystals of dinitrobromobenzol are, as observed by Kekulé, remarkably large and well defined. They powder harshly, like rosin. The concentrated alcoholic solution emits sound as it crystallizes.

Dinitrobromobenzol melts with considerable sharpness.

Table IV.

	A_1.	A_2.	A_4.	A_6.	A_9.	A_{10}.	A_{16}.
	70°53	70°54	70°56	70°68	70°67	70°68	70°56
	·53	·51	·64	·55	·54	·71	·59
	·50	·65	·61	·55	·64	·55	·53
	·53	·49	·64	·55	·67	·71	·53
	·48	·51	·56	·57	·54	·55	·56
	·53	·57	·67	·68	·70	·60	·66
	·61	·51	·59	·57	·56	·60	·66
	·58	·65	·59	·68	·64	·63	·56
	·66	·59	·48	·57	·59	·68	·46
	·53	·57	·67	·68	·59	·60	·69
	·64	·54	·69	·63	·67	·57	·71
	·48	·54	·67	·65	·62	·68	
	·50	·67	·64	·63	·67	·57	
	·56	·62	·67	·63	·67	·57	
Mean	70·55	70·57	70·62	70·61	70·62	70·62	70·59
Probable error ...	·010	·010	·010	·009	·009	·010	·015
Thermometer ...	2	2	2	2	2	2	3

5. *Nitrodibromobenzol.*—Sample A, from (D) dibromobenzol (q. v.) which had been crystallized once from alcohol, once from naphtha, and once from alcohol. 25 grm. of substance were gently heated with 250 cub. cent. of "fuming nitric acid," and allowed to cool during a rather longer period. The product, purified by means of water and ammonia, contained a little oily impurity, and weighed 28·5 grm. (=114 per cent., theory requiring 119·1 per cent.): it was crystallized twice from naphtha, and thrice &c. (A_3 &c.) from alcohol.

* *Ann. Chem. Pharm.* cxxxvii. p. 167.

B was prepared from another portion of (D) dibromobenzol. 25 grm. were gently heated for two hours with 250 cub. cent. nitrate as before, and allowed to cool during twenty-one hours. The pressed product, which had at first contained an oily body, weighed 17 grm. (=68 per cent.): it was crystallized twice from naphtha, and thrice &c. (B_3 &c.) from alcohol.

The melting-point of nitrodibromobenzol is satisfactorily sharp.

TABLE V.

	A_3.	A_1.	A_5.	B_3.	B_4.	B_1.	B_5.
	83°·49	83°·43	83°·46	83°·46	83°·49	83°·50	83°·32
	·53	·48	·46	·49	·49	·52	·49
	·47	·46	·51	·46	·44	·50	·44
	·55	·54	·43	·43	·54	·45	·47
	·47	·46	·49	·46	·49	·52	·49
	·55	·43	·43	·44	·46	·48	·55
	·53	·48	·51	·39	·57	·52	·57
	·53	·54	·49	·49	·52	·50	·49
	·47	·54	·49	·46	·52	·50	·41
	·49	·48	·46	·52	·52	·60	·52
	·53	·51	·49	·44	·41	·50	·52
	·55	·51	·49	·46	·49	·52	·49
Mean	83·51	83·49	83·48	83·46	83·49	83·51	83·50
Probable error ...	·006	·007	·005	·006	·008	·006	·008
Thermometer ...	2	2	2	2	2	3	2

B. *Aniline Derivatives.*

1. *Monochloraniline.*—Sample A was prepared from chlorinated acetanilide*, free from toluol. It was purified by solution in aqueous hydric chloride, evaporating thrice to dryness after filtration, precipitating with ammonia, and distilling in a current of steam. The product was crystallized twice from naphtha, and thrice &c. (A_3 &c.) from alcohol.

B was prepared more directly from aniline by the following method:—Half a pound of aniline, purified by cohobation for several hours with one sixteenth of its weight of mercuric chloride, was mixed with a pound of glacial acetate, and chlorine led slowly over its surface. Considerable heat was evolved. When this sign of action ceased to be manifested the mixture was allowed to cool, and the now solid product heated with water and caustic soda : the oily layer thus formed was withdrawn, and cohobated for a few hours with alcoholic potash. Water was next added to this solution and its residue; and the

* For the details of the methods of obtaining aniline derivatives from acetanilide see Proc. Roy. Soc. x. p. 589, and Phil. Mag. 1875, xlix. p. 21.

united amines thereby precipitated were separated by appropriate treatment with aqueous hydric chloride*. B was crystallized twice from naphtha, and thrice &c. (B_3 &c.) from alcohol.

The chlorination of acetanilide yields but little dichloraniline; the chlorination of the acetate still less.

Monochloraniline produces great cold when dissolved in alcohol. On the application of heat to its powder, it melts with great sharpness.

TABLE VI.

	A_3.	A_1.	A_5.	A_6.	B_1.	B_7.	B_8.	B_3.	B.
	69°·66	69°·68	69°·69	69°·66	69°·75	69°·68	69°·69	69°·69	69°·66
	·74	·76	·69	·63	·64	·63	·66	·66	·56
	·74	·71	·74	·63	·64	·63	·72	·63	·56
	·58	·71	·74	·66	·70	·65	·74	·66	·48
	·74	·65	·66	·79	·64	·65	·72	·71	·51
	·71	·76	·69	·69	·62	·65	·69	·66	·56
	·66	·71	·64	·71	·64	·71	·72	·58	·53
	·66	·65	·64	·66	·70	·71	·66	·61	·53
	·71	·63	·64	·63	·64	·73	·69	·66	·53
	·69	·73	·64	·66	·64	·63	·64	·66	·48
	·69	·68	·74	·71	·64	·73	·66	·63	·48
	·79	·76	·66	·77	·67	·63	·69	·71	·58
	·61	·68	·64	·77					
	·77	·63	·72	·71					
Mean	69·69	69·69	69·68	69·69	69·66	69·67	69·69	69·65	69·54
Probable error.	·010	·008	·007	·009	·007	·008	·006	·007	·010
Thermometer. .	2	2	2	2	2	2	2	2	3

2. *Trichloraniline.*—A was obtained by chlorinating a solution of aniline in glacial acetate, as described under *Monochloraniline.* The product was melted under aqueous potash, twice distilled in a current of steam, and crystallized twice from naphtha and thrice &c. (A_3 &c.) from alcohol.

The chlorination of acetanilide suspended in water yields little or no trichloraniline.

Lesimple† describes a modification of trichloraniline which he prepared from nitrotrichlorobenzol by reduction. It has, he states, a very unpleasant and persistent smell, and melts at 96°·5. On these two points my derivative differs from his: it has a faint but not unpleasant odour, and melts at about 77°. In all the other reactions and characters mentioned by Lesimple the two bodies exhibit a complete agreement. Beilstein (who,

* *Loc. cit.* † *Ann. Chem. Pharm.* cxxxvii. pp. 126, 127.

subsequently to myself, published an account of the derivative having the lower melting-point) confirms my determination. The substance with which I have dealt seems, then, to be isomeric with Lesimple's. Trichloraniline yields a non-electric powder, which, in consequence of the woolliness of this body, is rather difficult to obtain. The powder cakes somewhat on drying. The melting-point is exceedingly sharp.

TABLE VII.

	A_2.	A_4.	A_5.	A_6.	A_7.	A_8.	A_9.	A_{10}.
	77°·06	77°·05	77°·08	77°·06	77°·07	77°·03	77°·02	77°·00
	·03	·05	·05	·03	·07	·03	77·02	·02
	·01	·05	·05	·03	·07	·03	77·05	·02
	·06	·08	·08	·09	·04	·06	77·07	·02
	·06	·05	·08	·06	·07	·06	77·02	·02
	·09	·08	·08	·06	·10	·03	77·02	·05
	·03	·08	·08	·01	·12	·06	76·99	·02
	·03	·02	·05	·06	·10	·03	76·99	·02
	·03	·05	·05	·06	·10	·03	77·07	·05
	·06	·08	·05	·09	·10	·06	77·05	·07
	·11	·08	·05	·03	·07	·03	77·10	·00
	·06	·08	·05	·03	·10	·06	77·02	·02
Mean	77·05	77·06	77·06	77·05	77·08	77·04	77·04	77·03
Probable error ...	·005	·004	·003	·005	·004	·003	·006	·004
Thermometer ...	2	2	2	2	2	2	2	3

3. *Monobromaniline.*—In order to prepare this substance, the best commercial aniline was heated to 100° for three hours with $\frac{1}{32}$ part of mercuric bromide, which gave rise to the formation of a trace of rosaniline. The liquid was for the most part distilled off, cohobated for twelve hours with an equal weight of glacial acetate, and then distilled to 130°. The residue, after treatment with hot water, and pressure, was powdered finely, and rapidly stirred in a large bulk of water; into this, 1·75 part of bromine was gradually introduced. The brominated acetanilides thus produced were decomposed with alcoholic potash, and the resulting bromanilines separated by treatment with aqueous hydric chloride. The principal product was dibromaniline.

Specimens A and B were obtained by partially attacking with alcoholic potash the mixed bromacetanilides; the unattacked residue yielded no more on renewing the treatment. They were crystallized several times from naphtha and spirit. The remainders of A and B were further crystallized thrice from spirit (A B). The mother-liquids of these preparations

were united, and their bromaniline crystallized four times from naphtha and nine times from alcohol, in the presence, during the last two operations, of animal charcoal (C). Q was obtained by directly treating aniline with bromine. It was purified by crystallization—once from naphtha and thrice from alcohol with the aid of animal charcoal (Q_1), also once more from alcohol (Q_2).

Monobromaniline may be dried over oil of vitriol; but I have preferred to use calcic chloride.

The melting-point of bromaniline is very sharp; A B proved exceptionally sudden in this respect.

Table VIII.

	A.	B.	A B.	C_1.	C_1.	Q_1.	Q_2.
	61°·78	61°·78	61°·76	61°·82	61°·65	61°·93	61°·94
	·78	·82	·78	·86	·81	·85	·78
	·74	·91	·76	·76	·68	·74	·97
	·89	·80	·83	·80	·89	·88	·97
	·84	·80	·83	·80	·68	·90	·97
	·85	·74	·76	·82	·70	·88	·94
	·91	·71	·85	·75	·84	·72	·83
	·80	·82	·76	·82	·78	·85	·81
	·84	·76	·76	·78	·81	·77	·83
	·87	·84	·76	·78	·84	·88	·83
	·87	·78	·76	·76	·76	·79	·81
	·84	·89	·74	·82	·84	·79	·81
	·78	·84	·74	·84	·92	·88	·78
	·84	.....	·78	·76	·84	·85	·83
	.....	.....	.....	.....	·84		
Mean	61·83	61·81	61·78	61·80	61·79	61·84	61·86
Probable error ...	·008	·010	·006	·006	·014	·011	·013
Thermometer ...	4	4	4	4	2	2	2

4. *Dibromaniline* [see *Monobromaniline*].—The substance was dissolved in water containing one tenth vol. of common aqueous hydric chloride, the solution filtered cold, and mixed with sufficient ammonia.

Specimens A and B were obtained by the partial action of alcoholic potash on bromacetanilides. A was crystallized four times from naphtha and five times from alcohol; B three times from naphtha, once from spirit, twice from alcohol. C was similarly obtained; it was separated in an evaporation for monobromaniline, having been held in solution by the hydrochloride of that body: this sample was crystallized three times from naphtha and three times from alcohol, and animal charcoal was used. F and G were a result of the further action of alcoholic potash on bromacetanilides. F was sepa-

rated like C; it was crystallized three times from naphtha, four times from alcohol, and animal charcoal was used. G was treated on five successive occasions with a quantity, insufficient to dissolve it, of the dilute aqueous hydric chloride already named; the cold filtered solution was precipitated with ammonia. Of the successive precipitates (G_1, G_2, G_3, G_4, G_5), G_1, G_3, and G_5 were used for the determination of melting-point; G_1 and G_3 were crystallized twice from naphtha and thrice from alcohol, animal charcoal being present at the last operation; G_5 was crystallized twice from naphtha, and twice from alcohol in presence of animal charcoal.

The hydrochloric solution of dibromaniline already mentioned sometimes shows supersaturation.

TABLE IX.

	B.	A.	C.	F.	G_5.	G_1.	G_2.	G_3.
	78°·91	78°·88	78°·74	78°·82	78°·84	78°·68	78°·79	78°·92
	·94	·91	·85	·85	·73	·73	·79	·85
	·88	·85	·85	·66	·73	·71	·79	·92
	·83	·80	·71	·82	·79	·79	·76	·81
	·88	·91	·93	·79	·76	·76	·82	·81
	·91	·77	·87	·87	·68	·87	·90	·73
	·88	·72	·90	·85	·76	·73	·74	·73
	·80	·88	·87	·69	·76	·81	·82	·83
	·94	·83	·77	·93	·79	·89	·92	·94
	·78	·77	·71	·71	·87	·92	·90	·85
	·91	·69	·85	·71	·71	·84	·82	·79
	·78	·83	·90	·90	·81	·84	·74	·85
	·91	·77	·82	·87	·87	.....	·92	·79
	·78	·91	·77	·90	·81	.....	·82	·92
Mean	78·87	78·82	78·82	78·81	78·78	78·80	78·82	78·84
Probable error ...	·010	·012	·013	·015	·010	·014	·011	·012
Thermometer ...	2	2	2	2	2	2	2	4

5. *Tribromaniline* [see *Monobromaniline*].—Sample A was prepared from brominated phenylacetamide by distilling the crude product of the action of alcoholic potash thereon from strong aqueous hydric chloride, and adding water to the distillate.

Z was precipitated by water from some strongly hydrochloric washings, and crystallized four times from naphtha, four times from alcohol. B was crystallized from a hot mixture of ordinary aqueous hydric chloride diluted with two vols. of water, and washed with the same liquid diluted with six vols. of water. It was crystallized twice from naphtha and twice from alcohol, animal charcoal being present. Q was prepared by directly acting with bromine on aniline dissolved

in a large volume of dilute sulphuric acid. It is the characteristic product of the reaction; but a little bromaniline is also formed, with traces perhaps of dibromaniline. It was purified by extraction with aqueous hydric chloride diluted with nine vols. of water, by distillation, and by crystallization from spirit with the aid of animal charcoal. Q_1 was crystallized thrice from spirit, Q_2 four times. In the preparation of tribromaniline, whether from aniline directly or from phenylacetamide, the product is accompanied with a considerable amount of a black substance, non-volatile, and insoluble even in boiling spirit, which evolves much hydric bromide on distillation, and then carbonizes.

The determinations headed A, Z, and B were made with a thermometer protected by two glass cylinders; in the other cases the thermometer was bare.

The exact observation of the melting-point of tribromaniline is very difficult, inasmuch as the substance becomes transparent only at the edges of a bead which is for the most part dim and pasty.

TABLE X.

	A.	Z.	B.	Q_1.	Q_2.	Q_1.
	116°·42	116°·07	116°·05	116°·20	116°·14	116°·31
	·26	·12	·21	·31	·17	·31
	·28	·26	·24	·23	·28	·24
	·17	·32	·21	·15	·22	·24
	·31	·18	·19	·26	·30	·26
	·23	·23	·16	·31	·22	·19
	·27	·23	·13	·26	·19	·11
	·23	·34	·19	·28	·22	·31
	·28	·23	·13	·31	·36	·31
	·26	·12	·21	·23	·36	·26
	.....	.....	.....	·28	·33	·29
	.....	.....	.....	·28	·28	·24
	.....	.....	.....	·31	·36	
	.....	.....	.....	·31	·30	
	.....	.....	.....	·28	·39	
Mean	116·27	116·21	116·17	116·27	116·27	116·26
Probable error ...	·013	·018	·011	·008	·013	·011
Thermometer ...	2	2	2	2	2	3

C. *Toluol Derivatives.*

1. *Nitrotoluol.*—Nitrotoluol was prepared from coal-tar toluol, which had been purified by agitation with oil of vitriol and potash successively, and by distillation. According to Beilstein's recommendation, hydric nitrate of spec. grav. 1·48 was dropped into toluol; a stream of air was kept passing through the flask, and a stream of water round it. Through

the washed mixture of liquid and solid nitrotoluol with toluol thus obtained, a current of steam was passed, to remove toluol first; then a mixture of the two nitrotoluols came over; and from this the solid was almost completely extracted by solidification in a freezing-mixture and filtering out suddenly by atmospheric pressure. For the success of this operation, distillation in a current of steam is essential.

Sample A was purified by melting with a little Nordhausen, washing with water, and crystallizing from spirit: the mother-liquids were evaporated for deposits, d_1, d_2, d_3. The results were:—

d_1.	d_2.	d_3.	
51°·45	51°·18	51°·22	Each result is a mean.
51·11	50·83 (thermometer 2)		
51·12 (recrystallized)			
51·13–51·52 (sublimed)			

It is clear that neither crystallization from spirit (even after Nordhausen) nor sublimation gives a satisfactory result.

Sample B was purified like A, twice crystallized from spirit, once from pure high-boiling-point (132°) naphtha, twice from spirit, once from spirit of wine.

Sample C was similarly purified: four successive extracts (x) were taken in a mixture of 1 spirit, 2 water. Therm. 2. The mean results were:—

x_1.	x_2.	x_3.	x_4.
44°·5	44°·9	43°·8	50°·7

This shows that the method of extracts is also a failure. x_1, x_2, x_3 were united as C_a; x_4 was set aside as C_b; C_a was twice crystallized from naphtha, once from spirit of wine. Four similar extracts were then made, united, crystallized from naphtha, C_b added, and the mixture was again crystallized, once from naphtha, once from spirit of wine: this product is termed C_c.

Sample D was thrice crystallized from naphtha, once from spirit of wine; twice, thrice, and four times from spirit of wine. Part of the crude substance of D had not been distilled. The fractions are marked D_4, D_5, &c., according to the times of crystallization.

M was prepared by dropping purified toluol into twice its bulk of nitrous or "fuming" nitrate of spec. grav. 1·48; 50 cub. cent. of toluol were added to 100 cub. cent. nitrate in each operation, which lasted 1·6 hour. It was crystallized twice from naphtha and thrice from alcohol.

TABLE XI.

	C.	C_α.	C_β.	D_1.	D_5.	D_6.	D_7.	B.
	51°·43	51°·39	51°·22	51°·22	51°·27	51°·34	51°·34	51°·31
	·29	·39	·37	·32	·10	·39	·39	·34
	·40	·46	·30	·35	·10	·17	·24	·29
	·31	·37	·40	·25	·35	·32	·39	·34
	·29	·34	·32	·37	·37	·14	·39	·21
	·31	·34	·35	·37	·35	·17	·34	·34
	·34	·24	·37	·35	·35	·19	·39	·34
	·47	·24	·48	·09	·27	·32	·39	·37
	·31	·32	·24	·35	·39	·29	·30	·16
	·24	·21	·25	·27	·39	·17	·32	·39
	·31	·26	·19	·22	·27	·27	·36	·29
	·21	·41	·14	·35	·39	·14	·30	·29
	·31	·26	·26	·14	.....	·32	·22	·29
	·24	·37	.....	.....	.....	.....	.....	·31
Mean	51·32	51·33	51·30	51·27	51·27	51·35	51·34	51·30
Probable error ...	·011	·013	·017	·016	·016	·016	·010	·010
Thermometer ...	2	3	2	2	3	3	3	2

2. *Dinitrotoluol.*—I have investigated the melting-point of this substance as prepared (1) directly from toluol, (2) from liquid nitrotoluol ("metanitrotoluol"), (3) solid nitrotoluol when the active masses are relatively small, and (4) solid nitrotoluol when the active masses are relatively large.

[Case I.] Dinitrotoluol was prepared from coal-tar toluol, which had been purified by agitation with oil of vitriol and potash successively, and distillation: a mixture of oil of vitriol with hydric nitrate was used. The product was pressed to free it from oily matter, and crystallized from spirit. The portions that fell successively in the act of crystallization were called A, B, C, D, and many experiments were made with them; but the results were not very accordant. The four samples were fractionally crystallized from spirit: the first deposits being called A_1, B_1, &c.; the second, A_2, B_2, &c.

Fractions of F (an old preparation) were also employed.

Melting-points having been taken, C_1, F_4, and F_3 were found to coincide, and mixed together. Four successive fractions of the mixture were taken; and after some further fractionation,

$(F_2)_2$, $(F_1)_1$, $[(F_3F_4C_1)_2]_2$, $[(F_3F_4C_1)_3]_2$, $(F_3F_4C_1)_1$ yielded *a*;

A_1, B_1, $(F_2)_1$, $[(F_3F_4C_1)_4]_2$, $[F_3F_4C_1)_4]_1$, $[(F_3F_4C_1)_3]_1$ yielded *b*;

A_2, B_2, C_2, $(F_1)_2$, $[(F_3F_4C_1)_2]_1$ yielded *c*;

D yielded *d*.

The following results were then obtained :—

TABLE XII.

	a.	*a.*	*b.*	*b.*	*d.*	*b.*	G.	G.	*a.*
	69°·23	69°·12	69°·22	69°·20	69°·13	69°·23	69°·15	69°·29	69°·33
	·23	·20	·19	·20	·16	·20	·20	·31	·25
	·09	·23	·22	·20	·16	·28	·42	·16	·40
	·33	·20	·22	·20	·18	·25	·29	·09	·22
	·14	·23	·09	·25	·21	·25	·34	·31	·38
	·06	·12	.....	·12	·10	·12	·29	·31	·28
	.....	.....	.....	·23	·16	·12	·15	·06	·33
	.....	.....	.....	·23	·29	·25	·26	·06	·33
	.....	.....	.....	·23	·24	·20	·34	·31	·38
	.....	.....	.....	·27	·21	.....	·29	·29	·17
	.....	.....	.....	.....	·16	.....	·34	·14	·17
	.....	.....	.....	.....	.....	.....	.....	·29	·40
	.....	.....	.....	.....	.....	.....	.....	·31	
	.....	.....	.....	.....	.....	.....	.....	·24	
Mean	69·18	69·18	69·19	69·21	69·18	69·21	69·28	69·23	69·30
Probable error .	·025	·013	·015	·007	·010	·012	·016	·017	·016
Thermometer ...	3	3	3	3	3	2	2	3	2

[Case II.] The mixture of nitrotoluols obtained by nitrating coal-tar toluol was cooled to −17°, and the solid modification (which separated in crystals) removed by sudden application of the filter-pump and subsequent fractional distillation. The product boiled at 219°–223°. 6·5 cub. cent. of substance dissolved completely in 20 cub. cent. nitrous nitrate, with

TABLE XIII.

	I.	II.
	68°·99	69°·24
	69·15	·19
	·12	·32
	·23	·24
	·20	·21
	·26	·13
	·23	·21
	·20	·16
	·15	·16
	·23	·00
	·26	·11
	·23	·19
	·15	·03
	·15	·08
	·18	·13
Mean	69·18	69·16
Probable error	·012	·014
Thermometer	2	2

slight evolution of heat; after four hours' contact, 150 cub. cent. water were added. The precipitate, which was solid on the following morning, was washed with warm aqueous sodic carbonate, and then weighed about 8·5 grm., or 112·5 per cent., the theoretical yield being 132·9 per cent. I will call this specimen J dinitrotoluol. It was crystallized four times from naphtha and three times from spirit. (See Table XIII.)

[Case III.] The whole of the remainder of D nitrotoluol (p. 12) was treated with nitrous hydric nitrate in the cold; 2·5 grm. of the nitrotoluol being dissolved in 39 grm. of the nitrate, with which it remained in contact 2½ hours. The powder was very electric. I will call this specimen L. It was crystallized four times from naphtha and thrice from spirit.

The following numbers were obtained with Therm. 2:—

TABLE XIII A.

	69°·28
	·17
	·20
	·17
	·14
	·12
	·12
	·17
	·22
	·17
	·14
	·03
	·22
	·22
	·22
Mean	69·17
Probable error	·010
Thermometer	2

[Case IV.] When rather larger quantities are taken, though the same proportions between the reagents and time of action be observed, a different result is obtained. Thus Y was prepared from 4·5 grm. M mononitrotoluol; the crude product (122 per cent.) was purified by warming with weak aqueous potash containing ammonia, and was crystallized twice from naphtha, eight times from alcohol. Z was made from 20·5 grm. of a new stock of solid mononitrotoluol (which had been distilled in steam, frozen, pressed, and crystallized twice from naphtha and four times from alcohol); the crude product (120 per cent.) was crystallized twice from naphtha, and eight to nine times (Z_8, Z_9) from alcohol. Both samples had a faint yellowish tinge.

TABLE XIV.

	Y.	Z_3.	Z_4.
	69°·54	69°·60	69°·43
	·57	·60	·46
	·54	·57	·51
	·62	·63	·51
	·57	·57	·38
	·59	·55	·51
	·57	·57	·51
	·57	·57	·56
	·51	·65	·56
	·51	·63	·62
	·51	·60	·59
	·59	·57	·59
Mean	69·56	69·59	69·52
Probable error ...	·007	·006	·013
Thermometer ...	2	2	2

3. *Trinitrotoluol.*—A. Some crude coal-tar dinitrotoluol was heated with about five times its weight of "fuming" nitrate for more than thirty hours, but not so as to cause ebullition of the liquid. It had lost about 15 per cent. of its weight, but, as shown by its melting-point, had not been converted into trinitrotoluol.—B was lost in the process of crystallization.—C. The deposit from the nitric mother-liquid of the preparation fused at about 59°·7. The substance itself fused at about 80°·8 when crystallized nine times from spirit; and then, when kept a few days, its melting-point rose to about 181°·8. A small residue from the entire preparation, crystallized thrice from spirit, thrice from naphtha, and thrice from spirit of wine, fused at 182°·8 nearly. This body is soluble in naphtha, insoluble in cold and very sparingly soluble in hot spirit, insoluble in water or aqueous ammonia. Quantitative experiment seems to indicate that it is a compound of dinitrotoluol with trinitrotoluol. In appearance it closely resembles trinitrotoluol. Subsequent attempts to prepare it, both directly and in the nascent way, did not succeed.—D. The result of A was confirmed.

E. A specimen of coal-tar trinitrotoluol about 3 years old, yellow at the top; the melting-point of the lower and colourless part was taken. (This result will be noticed separately.) The remainder from this first determination (part being yellow) was crystallized twice from naphtha and once from spirit of wine. The results are given under the first E column; the

result of a further crystallization from spirit under the second E column; and so on.

Table XV.

	F_1.	F_1.	F_1.	F_2.	F_2.	E.	E.	E.	E.	K.
	78°·88	78°·86	78°·89	78°·78	78°·85	78°·79	78°·83	78°·76	78°·88	78°·87
	·91	·91	·79	·88	·85	·76	·81	·86	·78	·95
	79·05	·81	·87	·80	·83	·81	·86	·73	·80	·95
	78·78	·86	·94	·88	·93	·86	·89	·62	·70	·90
	·81	·73	·89	·88	·80	·83	·83	·78	·88	·90
	·81	·78	·84	·90	·87	·89	·89	·78	·82	·95
	·78	·78	·78	·93	·93	·65	·76	·78	·85	·84
	·81	·84	·84	·93	·93	·83	·81	·88	·80	·90
	·81	·73	·86	·80	·92	·89	·86	·78	·82	·84
	·81	·81	·92	·88	·92	·71	·97	·83	·80	·78
	·78	·81	·83	·93	·83	·86	·73	·78	·90	·84
	.....	.....	.....	.....	.....	79·02	·91	·86	·82	
Mean	78·84	78·81	78·86	78·87	78·88	78·82	78·84	78·78	78·82	78·88
Probable error.	·016	·011	·010	·011	·009	·018	·012	·013	·010	·011
Thermometer...	2	2	3	3	3	2	2	2	2	2

F. Coal-tar toluol boiling at 109°·5–112°·0 was purified with oil of vitriol in the cold; it then boiled at 110°–113°. Equal volumes (300 cub. cent.) of nitrate (sp. gr. 1·48) and this substance were mixed, the latter being run in gradually. 51 cub. cent. of toluol were recovered by distillation; hence benzol was absent. The product was washed, added to 300 cub. cent. "fuming" nitrate (sp. gr. 1·5); washed, added to 4 vols. "fuming" nitrate (the latter), left 24 hours, and 1 vol. Nordhausen poured in—this being done in two operations, on account of the violent action in the cold. The yield was 190·7 per cent.; theory, 246·7 per cent. Half of the yield was crystallized from naphtha and spirit, and termed F_1. The other half was boiled for 6×6 hours with "fuming" nitrate, and termed F_2; the product was very dark, contained scarcely any acid bodies, and showed little loss when weighed. The fusion-points of F_1 and F_2 are the same.

K. Prepared from liquid coal-tar nitrotoluol which had been twice distilled, cooled to −17°, filtered, again distilled (under 224°); when cooled as mentioned, only a trace of crystals was deposited. 19 cub. cent. of this liquid, 150 cub. cent. "fuming" nitrate, 100 cub. cent. Nordhausen, yielded 31·5 grm. trinitrotoluol, = 142·6 per cent., theory requiring 165·7 per cent. The product was crystallized four times from

naphtha, five times from spirit; its fusion-point then agreed with that of F_1 and F_2.

Such trinitrotoluol is found with great ease, an hour's ebullition with a mixture of equal volumes of oil of vitriol and common nitrate sufficing to produce it. Now the presence of oil of vitriol is a great hindrance to making ordinary *tri*-nitrotoluol*.

M. M nitrotoluol was treated with Nordhausen and nitrous nitrate, and yielded 157·1 per cent. of product. The melting-point of this preparation having been found not quite regular, the substance was again treated with the nitrating-mixture, and crystallized twice from naphtha, and thrice (M_1), four times (M_2), &c. (&c.) from spirit.

The crystalline form of this modification of trinitrotoluol is distinct from that of F_1 or K_1, being much more prismatic and less platy.

A small preparation (L) of trinitrotoluol made from L dinitrotoluol (itself made from D nitrotoluol) was treated like M, and the melting-point also determined before complete nitration had been effected. This point was $78°{\cdot}91 \pm {\cdot}008$, after one crystallization from naphtha and two from spirit; and the prismatic character of the crystals was apparent; but the amount of substance was too little to go on with.

TABLE XVI.

	M_1.	M_2.	M_3.	M_4.	M_5.
	80°·49	80°·53	80°·50	80°·58	80°·55
	·36	·56	·47	·53	·52
	·52	·58	·55	·47	·55
	·49	·42	·55	·50	·47
	·52	·56	·50	·53	·60
	·46	·42	·47	·53	·58
	·49	·55	·58	·50	·55
	·49	·53	·55	·50	·55
	·54	·50	·50	·47	·52
	·54	·48	·53	·53	·58
	·49	·61	·61	·53	·58
	·49	·58	·61	·55	·55
	·52	·42	·53	·53	·60
	·54	·42	·50	·53	·58
Mean	80·49	80·51	80·53	80·52	80·55
Probable error ...	·008	·012	·008	·005	·006
Thermometer ...	2	2	2	2	2

* *Zeit. Chem.* xiii. p. 539.

Thus it is evident that trinitrotoluol prepared from the solid modification of nitrotoluol is distinct both in form and melting-point from the others ; it is also more difficult in making.

It deserves to be mentioned that the melting-point of trinitrotoluol appears to undergo a slight change under certain conditions. Thus, a perfectly colourless specimen three years old melted at 78°·76—the number of observations being 13, and the probable error 0·012. A few grammes of the specimen F_2, after exposure to light, with frequent agitation, for 13 days, during which it became mustard-yellow on the surfaces, melted at 78°·78 ; the number of observations being 14, and the probable error 0·013. This result was obtained after two crystallizations from naphtha and two from spirit, when the substance had become brilliantly white.

D. *Toluidine Derivatives.*

Toluidine.—The modification examined is solid at the ordinary temperature, and obtainable by the reduction of mononitrotoluol.

Sample H was purchased from Messrs. Hopkin and Williams. It was purified by conversion into oxalate, which salt was thrice extracted by ether and then decomposed by potash, distillation in a current of steam, and crystallization thrice from naphtha and four times &c. (H_4 &c.) from alcohol.

S was given me by Mr. Spiller. It was pressed and crystallized twice from naphtha and four to five times (S_{4-5}) from alcohol.

G was purchased from Dr. Schuchardt, of Görlitz. It was pressed, and crystallized twice from naphtha and three to four times from alcohol (G_{3-4}).

The above were ascertained to be all different preparations.

The melting-point of toluidine is difficult to observe. The substance remains for some time in the pasty stage, and then conducts heat very badly ; from this cause the melting-point may easily be overestimated. On the other hand, when the solid substance is plunged into a bath which is hotter than the real melting-point, it melts with great readiness and sharpness. Good numbers can only be obtained with finely-powdered material ; the capillary tubes must be introduced into the bath at 4° or 5° below the melting-point; and the mercury in the thermometer must rise very slowly towards the last.

TABLE XVII.

	H_4.	H_5.	H_6.	H_6.	S_4.	S_5.	S_6.	G_3.	G_4.
	42°·78	42°·79	42°·78	42°·69	42°·82	42°·81	42°·80	42°·67	42°·83
	·88	·74	·67	·85	·82	·86	·76	·80	·78
	·73	·61	·73	·75	·82	·83	·74	·72	·75
	·76	·77	·76	·73	·82	·76	·76	·83	·80
	·78	·71	·70	·73	·72	·79	·71	·75	·83
	·65	·66	·83	·69	·74	·83	·68	·77	·93
	·83	·69	·70	·75	·87	·76	·86	·85	·78
	·78	·77	·73	·81	·82	·83	·86	·75	·80
	·83	·71	·78	·69	·74	·79	·81		
	·83	·69	·83	·73	·84	·68	·76		
	·70	·63	·65	·79	·74	·71	·78		
	·73	·79	·70	·77	·72	·83	·66		
	.....	·63	·76	·73	·69	·81	·67		
	.....	·77	·62	·81	·82	·79	·76		
Mean	42·77	42·71	42·73	42·76	42·78	42·79	42·76	42·77	42·81
Probable error.	·012	·011	·011	·009	·010	·009	·011	·013	·012
Thermometer ...	2	2	2	4	2	2	2	3	3

E. *Phenol Derivatives.*

1. *α Mononitrophenol.*—By α mononitrophenol I mean the more volatile mononitrophenol of the two produced by treating phenol with water and nitrate according to Fritsche's method (*Journ. prak. Chem.* lxxv. p. 257). The crude stock was several times distilled.

A was crystallized from water and dried over oil of vitriol (as the rest were): it had been exposed to light (A_1); it was further and continuously exposed to light (A_2). B_1 was a partial precipitate from spirit by water, dried in the shade; the mother-liquid gave with more water a precipitate which turned brown when dried like B_1, and then melted about 0°·6 below B_1. C_1 was made like B_1, except that scarcely any thing was left in the mother-liquid. E was a total precipitate from a sodic salt which contained, as a mean of two experiments, 14·28 per cent. of sodium (theoretically 14·31): it was dried in the shade. D was twice crystallized, probably from weak spirit, and dried in shade twenty days, *i. e.* very much longer than either of the others.

The powder of α mononitrophenol has a tendency to cake together on keeping.

TABLE XVIII.

	D.	B_1.	C_1.	E.	A_1.	A_2.
	44°31	44°29	44°29	44°22	44°28	44°31
	·29	·29	·24	·25	·25	·31
	·24	·24	·18	·25	·31	·23
	·33	·18	·10	·22	·28	·17
	·24	·32	·29	·27	·28	·25
	·29	·26	·29	·27	·23	·39
	·37	·29	·29	·30	·28	·31
	·33	·32	·21	·22	·31	
	·24	·21	·29	·27	·31	
	·33	·32	·29	·25	·28	
	.....	·21	·29	.....	·28	
Mean	44·30	44·27	44·25	44·25	44·28	44·28
Probable error ...	·009	·010	·012	·005	·005	·017
Thermometer ...	2	3	3	2	3	3

2. *β Mononitrophenol.*—β mononitrophenol is the less volatile mononitrophenol of the two produced by Fritsche's process (*Journ. prak. Chem.* lxxv. p. 257). The crude stock was converted into sodic salt, washed with aqueous sodic hydrate, dissolved in water, and precipitated by hydric chloride: a sodic salt prepared from this contained 14·17 instead of 14·31 per cent. of sodium, the preparation being effected with but very little loss.

A_1 was crystallized thrice from naphtha and once from water; A_2 was the same, twice crystallized from water; B_1, B_2, B_3 were all crystallized together thrice from naphtha, in one quantity; and this was crystallized thrice from water. All the specimens were dried over oil of vitriol in the dark for about two months.

β mononitrophenol becomes pale dirty yellow on exposure to light for a day, especially if a part of it has been melted. As the coloured product probably melts below the temperature at which the uncoloured substance does; and as a broad gas-flame must tend somewhat to produce the same effect as daylight, the long stage of incipient fusion and the uncertainty of the limit are accounted for.

Table XIX.

	A_1.	A_2.	B_1.	B_2.	B_3.
	111°·29	111°·50	111°·37	111°·53	111°·40
	·29	·34	·60	·41	·35
	·35	·53	·51	·32	·40
	·40	·50	·41	·34	·40
	·38	·53	·41	·39	·47
	·45	·50	·48	·46	·51
	·40	·27	·37	·41	·51
	·31	·42	·51	·41	·51
	·26	·42	·58	·44	·49
	·24	·45	·46	·37	·38
	.....	.....	·51	.....	·31
	.....	.....	·32		
Mean	111·34	111·45	111·46	111·41	111·43
Probable error ...	·014	·017	·016	·012	·014
Thermometer ...	3	2	3	3	3

3. *α Dinitrophenol.*—α dinitrophenol appears to have been first observed by Armstrong, who, however, did not obtain it in a state of purity; the exacter definition of the substance is due to Hübner and Schneider (*Zeit. Chem.* xiv. p. 524). In preparing it, I followed the method recommended by the last-named chemists, depending more especially on the insolubility of the baric derivative in boiling spirit of 90 per cent. When the baric derivative was dissolved in water and treated with aqueous hydric chloride, the hydric salt was precipitated: this was afterwards dissolved and crystallized.

α dinitrophenol has an extremely pale yellow colour when crystallized from naphtha or spirit; the powder has a deeper shade; and the solution imparts a dark-orange tint to a tissue on which it has been dried.

A, B, C, D, E were distinct preparations. A and B had been crystallized thrice from naphtha and thrice from spirit; C had been crystallized from water, aqueous potassic chloride, and twice from spirit; D had been made from a baric salt twice extracted with spirit, after which it was crystallized once from naphtha and four times from spirit*; E was formed by uniting the remainders of A, B, and C, extracting the baric salt thrice with alcohol, and crystallizing the resulting α dinitrophenol once from naphtha and once from spirit.

* The hot alcoholic solution was poured off from a red, quite insoluble foreign substance.

α dinitrophenol melts sharply, the pasty stage being short. According to Hübner and Schneider its melting-point is 63–64°.

TABLE XX.

	A.	A.	B.	B.	C.	D.	E.
	61°·89	61°·79	61°·85	61°·82	61°·78	61°·82	61°·79
	·80	·79	·77	·77	·75	·85	·74
	·82	·84	·77	·78	·70	·87	·77
	·80	·82	·80	·75	·73	·74	·75
	·74	·74	·82	·67	·67	·77	·72
	·89	·87	·80	·78	·81	·71	·83
	·87	·87	·77	·80	·70	·74	·85
	·74	·84	·74	·77	·75	·79	·79
	·74	·87	·82	·82	·75	·85	·77
	·73	·92	·74	·78	·75	·74	·77
	·70	·74	·85	·71	·78	·74	·75
	·73	·79	·80	·65	·70	·85	·75
	.....	.....	·74	·78	·78	·82	·70
	.....	.....	·77	·80	·81	·82	·77
Mean	61·79	61·82	61·79	61·76	61·75	61·79	61·77
Probable error ...	·013	·010	·006	·009	·007	·009	·007
Thermometer ...	4	2	2	4	2	2	4

4. *β Dinitrophenol.*—Z. The crude compound was prepared by Grüner's method, from crystalline phenol. A considerable amount of its baric salt was crystallized repeatedly from a large volume of water, and the cooled and filtered mother-liquid precipitated with hydric chloride.

Z_ϵ was the tenth precipitate; it was crystallized thrice from water.

Z_ζ was the eleventh precipitate; it was crystallized once from water.

Z_η was the twelfth precipitate; it was crystallized once from water.

Preliminary determinations of melting-point were made with the nine preceding fractions; but the numbers were not sufficiently satisfactory to warrant proceeding with purification.

Y was made by Dr. Armstrong from trinitrophenol, by way of amido-dinitrophenol; it was crystallized thrice from water.

T. For this also I am indebted to Dr. Armstrong: he had prepared it by acting with ordinary hydric nitrate on phenol. It was crystallized once from naphtha, once from alcohol, and four times from water.

In determining the melting-points of Z_η, Z_ζ, and Y, the thermometers were protected by two glass cylinders; in the other three cases the cylinders were not used.

Crystallization, powdering, desiccation, and filling of capillary tubes had to be effected either in total darkness or in a deep shade. β dinitrophenol is very nearly white; but by two hours' exposure to a somewhat gloomy atmosphere it becomes deep turmeric yellow.

β dinitrophenol melts with moderate sharpness.

TABLE XXI.

	Z_η.	Z_ζ.	Y.	T.	Z_ϵ.	Z_ϵ.
	116·60	111·63	111·46	111·57	111·66	111·65
	·60	·63	·57	·57	·64	·55
	·46	·49	·63	·49	·61	·65
	·52	·58	·68	·60	·61	·58
	·55	·63	·82	·57	·64	·53
	·63	·60	·60	·60	·80	·63
	·46	·58	·49	·57	·61	·60
	·43	·68	·57	·52	·66	·58
	·52	·52	·60	·54	·53	·53
	·63	·55	·57	·54	·66	·65
Mean	111·54	111·59	111·60	111·56	111·64	111·59
Probable error ...	·015	·012	·020	·007	·014	·010
Thermometer ...	2	2	2	2	2	3

5. *Trinitrophenol.*—Sample A was a commercial specimen. It was crystallized twice from water, once from alcohol, and again from water.

Z was prepared from dinitrophenol made by Grüner's process (*v. suprà*); the material employed was very pure, having been precipitated from a 12th (Z_η) cold aqueous extract of the crude basic salt. This was evaporated to dryness on the water-bath with a very large excess of hydric nitrate, and crystallized once from naphtha, once from ethylic alcohol, and twice from water.

O was made from phenol, containing minute amounts of the two modifications of dinitrophenol, by evaporation, as with Z. It was crystallized thrice from water, once from alcohol, and a first (O_1), second (O_2), and third (O_3) time from water.

M was similarly prepared from sodic α nitrophenate (E under α nitrophenol), and was crystallized thrice from water.

F was derived from sodic β nitrophenate (the analysis is

given under β mononitrophenol); it was crystallized once from water, once from alcohol, and again from water.

N was prepared from some α (Hübner's) dinitrophenol, for which I am indebted to Prof. Armstrong. This was purified by Hübner's two processes, and can have contained at most mere traces of its isomer. The trinitro-compound was crystallized thrice from water.

Trinitrophenol is a nearly white substance when in crystals; if very finely divided by any means, it appears a pale yellow. The powdered crystals, if exposed for two hours to indirect light on a dull morning, acquire a deep mustard-yellow colour. The crystals of the substance termed N were almost perfectly white.

The melting-point of trinitrophenol is, on the whole, not difficult to observe.

TABLE XXII.

	A.	Z.	O_1.	O_2.	O_3.	M.	F.	N.	N.
	121°·11	120°·95	121°·04	121°·14	121°·03	120°·99	121°·20	121°·20	121°·09
	·06	120·92	·07	121·11	120·98	121·13	121·17	120·98	120·89
	·08	121·03	·01	120·97	120·98	·16	120·98	121·09	121·09
	·11	120·95	·15	121·14	121·08	·18	121·14	·12	120·94
	·03	121·06	·18	120·97	·00	·10	·09	·12	121·11
	·06	121·00	·12	121·06	·08	·16	·01	·15	121·11
	·03	120·98	·23	·14	·06	·19	·12	·12	121·14
	·06	121·20	·09	·09	·14	·07	·04	·12	120·94
	·24	121·00	·12	·09	·11	·19	·14	·15	120·99
	120·97	121·00	·09	·11	·06	·16	120·98	·17	121·07
Mean	121·07	121·01	121·11	121·08	121·05	121·13	121·09	121·12	121·04
Probable error.	·014	·016	·013	·013	·011	·013	·016	·012	·018
Thermometer...	2	2	2	2	2	2	2	2	3

F. *Naphthalin Derivatives.*

Naphthalin.—Commercial pure naphthalin, which had been twice sublimed, was digested for a few hours with strong oil of vitriol in the water-bath. After washing with water, it was distilled from caustic soda in a current of steam. The first portion of the distillate is termed A, the second B. These portions were crystallized twice from naphtha and thrice (A_3 &c.) from alcohol. Before naphthalin melts it exhibits a decided pasty stage; yet the melting-point is sufficiently sharp.

The powdered substance is highly electric.

TABLE XXIII.

	A_3.	A_4.	A_5	A_6.	B_4.	B_5.	B_6.
	80°·06	80°·03	80°·01	80°·03	80°·05	80°·05	80°·06
	79·98	·06	·06	·08	·05	·08	·14
	80·01	·08	·09	·00	·05	·05	·09
	79·95	·00	·06	·08	·05	·02	·09
	80·03	·00	·09	·08	·05	·08	·09
	80·06	·00	·06	·08	·07	·05	·09
	80·09	·08	·12	·08	·07	·08	·03
	80·06	·06	·12	·11	·05	·08	·09
	80·03	·11	·06	·11	·07	·05	·09
	79·98	·03	·09	·06	·07	·05	·09
	80·06	·11	·06	·06	·07	·10	·11
	80·03	·00	·01	·08	·07	·05	·03
Mean	80·03	80·05	80·07	80·07	80·06	80·06	80·08
Probable error ...	·008	·008	·007	·006	·002	·004	·006
Thermometer ...	2	2	2	2	2	2	2

Nitronaphthalin.—A was a sample which had been in my collection for some years, and the details of its preparation are unknown. It was distilled in steam, and crystallized twice from naphtha and thrice &c. (A_3 &c.) from alcohol.

B was a similar sample from the same source. It was purified and fractionated exactly in the same way as A.

Nitronaphthalin becomes sensibly yellowed by continued exposure to light. It melts with considerable sharpness.

TABLE XXIV.

	A_3.	A_4.	A_5.	A_6.	A_7.	B_4.	B_5.	B_5.
	56°·17	56°·16	56°·17	56°·21	56°·15	56°·24	56°·17	56°·10
	·20	·13	·14	·21	·15	·24	·17	·20
	·14	·21	·12	·16	·18	·19	·20	·15
	·20	·16	·17	·16	·20	·19	·12	·12
	·17	·16	·22	·18	·07	·11	·14	·00
	·20	·21	·22	·26	·15	·16	·09	·15
	·17	·16	·17	·21	·12	·19	·14	·12
	·17	·16	·17	·16	·26	·16	·09	·10
	·17	·16	·14	·13	·14	·19	·09	·10
	·20	·16	·14	·16	·10	·21	·14	·18
	·22	·21	·16	·24	·18	·24	·12	·05
	·21	·21	·22	·16	·23	·16	·20	·15
	·20	·25	·25	·16	·18	·24	·14	·15
	·20	·21	·22	·18	·20	·16	·12	·02
Mean	56·19	56·18	56·18	56·18	56·16	56·19	56·14	56·11
Probable error ...	·004	·006	·007	·006	·009	·007	·006	·010
Thermometer ...	2	2	2	2	2	2	2	3

TABLE XXV.

Summary of Results.

Substance.	Weighted mean.	Probable error.	After Poggendorff's correction.	Air-thermometer.
Toluidine	42°·765	·004	42°·700	42°·890
Nitrophenol (*a*)	44·270	·003	44·205	44·392
Nitrotoluol	51·305	·005	51·239	51·407
Dichlorobenzol	52·723	·002	52·657	52·821
Nitronaphthalin	56·175	·002	56·110	56·261
Dinitrophenol (*a*)	61·778	·003	61·714	61·843
Monobromaniline	61·806	·003	61·742	61·871
Dinitrotoluol (*a*)	69·211	·004	69·154	69·252
„ „ (*b*)	69·571	·004	69·514	69·610
Monochloraniline	69·667	·003	69·610	69·706
Dinitrobromobenzol	70·598	·004	70·542	70·634
Trichloraniline	77·052	·001	77·004	77·068
Dibromaniline	78·821	·004	78·776	78·833
Trinitrotoluol	78·841	·004	78·796	78·853
Naphthalin	80·061	·002	80·018	80·070
Trinitrotoluol (M)	80·524	·003	80·481	80·532
Nitrodibromobenzol	83·490	·002	83·452	83·492
Dibromobenzol	87·037	·002	87·007	87·035
Dinitrobenzol	89·718	·003	89·693	89·712
Nitrophenol	111·413	·006	111·448	111·455
Dinitrophenol	111·579	·004	111·614	111·621
Tribromaniline	116·247	·005	116·298	116·319
Trinitrophenol	121·082	·005	121·151	121·194

DISCUSSION.

The determinations of melting-point which have been recorded in the preceding tables, and the results of which are summarized in Table XXV., show a very small probable error in connexion with their weighted means. The probable error of a weighted mean has ranged from 0°·001 to 0°·006, its average value being less than 0°·004. So far, then, as regards the actual process of ascertaining melting-point, considerable accuracy has doubtless been attained.

The preliminary operation of calibrating the thermometers was so conducted as not to have introduced material error, as indeed is obvious from a comparison of the results obtained with different thermometers on melting the same substance*. Regnault was of opinion that the height of the barometer cannot be ascertained with a less error than about 0·1 millim. Such an error would correspond to about 0°·0037 on 100°; and the error would be still less on the mean, as in the present case, of several readings. The small errors in the determination of the exposure-corrections could not sensibly affect the

* See, for instance, Table VII.

final results. It is to comparison with the air-thermometer that we have to look for any important source of error. All observers who have made an extended range of such comparisons have found noteworthy errors, though they have in no case stated probable error. The probable error of the result of my own comparisons of thermometer 2 with the air-thermometer is 0°·085 for a single set of comparisons, or 0°·085 $\div\sqrt{33}=0°{\cdot}015$ for the results of the thirty-three sets. This number is the measure of probable error of the equation employed in the final reductions. Compounding, then, the mean probable error of the melting-point (·004) with that of the comparison (·015) with the air-thermometer, we may consider the melting-points in Table XXV. ascertained, in terms of the air-thermometer, with a probable error of

$$\sqrt{(\cdot004)^2+(\cdot015)^2}=0{\cdot}015.$$

The relation of the chemical symbol to the physical properties of a substance is a matter of such great interest that I have sought for it in melting-point, although other investigators of the general subject, working with less definite data, have not arrived at very encouraging results.

It is very easy to show that, in some cases, there is a very simple connexion between the formula and the melting-point of a substance in the centigrade scale. Thus, dichlorobenzol, bromaniline, and trinitrotoluol form a group in which melting-point $=\phi\times$ numerical value of formula.

Substance.	Formula.	Melting-point.	ϕ.
Dichlorobenzol .	$C_6H_4Cl_2$ =147	52°·821	·35933
Bromaniline . .	C_6H_6BrN =172	61·742	·35971
Trinitrotoluol .	$C_7H_5N_3O_6$ =227	80·532	·35477

In the first of these two instances the values of ϕ are almost exactly the same; in the last, however, the limits of probable error are exceeded, though a close approximation is very evident.

The following comparison furnishes another practical identity:—

Melting-point.		Melting-point.	
Trinitrotoluol . 78°·853	— Dinitrotoluol .	69°·252 = 9·601	}
Trinitrophenol. 121·194	— Dinitrophenol.	111·621 = 9·573	}

In the next instance there is an approximation:—

Melting-point.		Melting-point.	
Dinitrotoluol . 69°·252	— Nitrotoluol .	51°·407 = 17·845	}
Dinitrophenol. 61·813	— Nitrophenol.	44·392 = 17·451	}

Such illustrations may be of service in enabling us to detect, with more or less probability, the parallelism of chemical series, and to enable us to decide whether a function—nitration, for example—has or has not the same value in different parts of a series. Other groups in which a similar but less intimate relation prevails, might be adduced from the list; and a glance at Table XXV. will show that, on the whole, melting-point and formula grow together. It may not improbably prove to be the case that, when the whole subject of melting-point has been successfully investigated, this simple relation is the limiting condition of the real law. The data, however, hitherto adduced are far from adequate to a discussion of numerical relations among melting-points: for such an object it would be a fruitless task to examine them further.

Some negative results of this investigation are worthy of attention. Thus α and β nitrophenol have the same additive formula, and yet differ by 67°·063 in their melting-point. It is clear then, as already well known, that melting-point may, in cases of isomerism, be related to something else than formula. The melting-point of naphthalin is actually lowered in the first stage of nitration. Considerations such as these may perhaps serve as suggestions for future work.

An accurate method of determining melting-point places it within our power to detect far more delicate shades of isomeric differences than have hitherto been regarded as possible. Thus strong presumptive evidence has been adduced (p. 14) that dinitrotoluol, when prepared directly from toluol or from liquid (meta-) nitrotoluol, or from solid (para-) nitrotoluol by gentle nitration, melts at 69°·252; but that when paranitrotoluol is energeticaly nitrated, the product melts at 69°·610. There are consequently two modifications of dinitrotoluol obtainable very directly from toluol—the melting-point of these substances differing by 0°·358, a quantity far beyond the range of error of the method. In like manner, it can be shown that two parallel modifications of trinitrotoluol exist—one of which melts at 78°·853, the other at 80°·532 *.

[For a complete account of the thermometers referred to in this memoir the reader is referred to the Transactions of the Royal Society of Edinburgh, 1881, p. 567; for the method of determining melting-point, to the Proceedings of the Royal Society, vol. xxxiii. p. 203.]

* Compare Phil. Mag. 1875. I. p. 17.

II. *Measurement of Curvature and Refractive Index.* By C. Vernon Boys, *A.R.S.M., Demonstrator of Physics at the Normal School of Science, South Kensington**.

[Plate I.]

OF the methods best known for measuring the curvature of surfaces, that depending on the spherometer is both accurate and convenient in the case of surfaces of sufficient diameter and where the curvature is not too small. The reflection-test, depending on the observation with a telescope of two images projected on a scale, is certainly not convenient, nor is it capable of giving very accurate results. While endeavouring to find some more satisfactory way of examining the curvature of the surfaces of lenses that would be both accurate and simple, I discovered the method which I am about to describe. Though, from its great simplicity, I can hardly expect it to be new to every one, yet I have never heard of its being employed, nor do those to whom I have shown it remember to have seen it before. Therefore, even if it should be shown that this method is not new, it is certainly so little known that I think it worthy of attention.

The centre-of-curvature test known as Foucault's test, which is used to examine the figure of the mirrors of reflecting telescopes, gives, perhaps, the most delicate means of examining form that exists. By its means the expansion by heat of a portion of the surface produced by touching it with the finger is rendered evident, as an apparent mountain standing out of the glass, which takes from five to fifteen minutes to disappear; and the warm air leaving a hand held between the centre of curvature and the surface has the appearance of flames of fire. In each of these cases no other system of observation could show in so striking and conspicuous a manner effects depending on so slight a cause.

The examination at the centre of curvature is carried out in this way:—The mirror is placed in a convenient support so that its surface is vertical; and in front of it is placed a lamp with an opaque chimney, through which some pin-holes have been made. If one of these holes is near the centre of curvature, the light leaving it and reflected by the surface is brought to a focus on the other side of the centre. This focus is then found; and the lamp is moved till the focus is as near the chimney as will allow of its observation. A piece of thin sheet metal, with a straight edge, is then placed so that it may be moved to or from the mirror or laterally. Now, if the edge of this is sufficiently on one side to let the light

* Communicated by the Author.

Fig. 1.

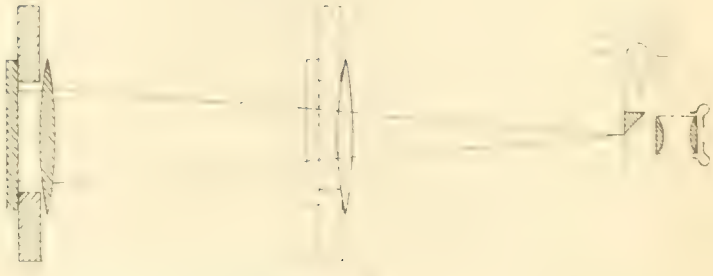


Fig. 2.

Fig. 3.

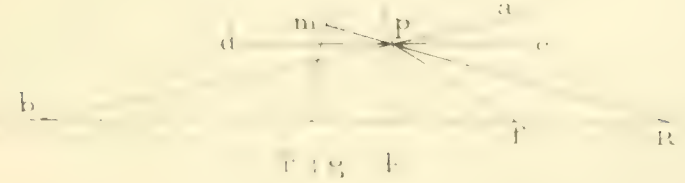


Fig. 4.

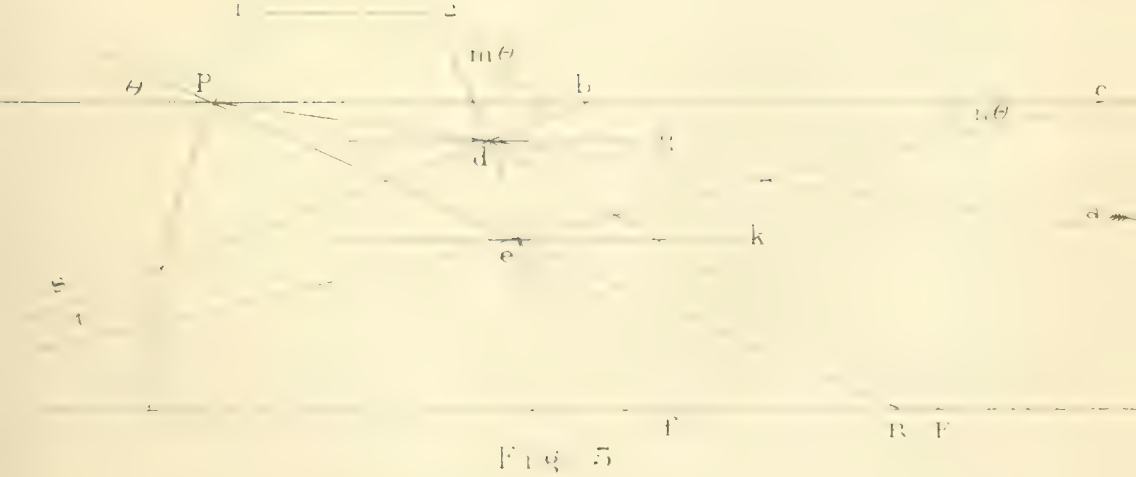


Fig. 5.

pass the focus, an eye immediately behind the focus will see the mirror filled with light; but if it is gradually moved across while the eye still watches the mirror, the illumination of the latter will appear to die away in the same direction as, or in the opposite direction to, the movement of the edge, or uniformly, according as the edge is between the mirror and the focus, or between the eye and the focus, or at the focus. By this means, and by this means only, can the different radii of curvature of the successive zones of a parabolic mirror whose radius of curvature is twenty times its diameter be accurately measured. The close contact between the parabola and the circle is due to the fact that it is one of the third order. I think it worth mentioning that the formula given by Dr. Draper in the 'Smithsonian Contributions to Knowledge,' vol. xiv. (1865), for testing the true parabolic form, gives only half the deviation from the sphere, as was pointed out by a correspondent of the 'English Mechanic' who signs himself "Orderic Vital," and was confirmed by Mr. E. H. Liveing and myself*. I have gone thus fully into the Foucault test, as my method involves the same general principle, viz. making the rays return along the path whence they came.

Before considering the general case applicable to any kind of lens, I think it best first to show the simplicity of the method in a particular and common case—that of a thin equiconvex lens. Fix an ordinary spectacle-lens in a clip, with its principal plane vertical; in front of it place a card with a small hole in it; and illuminate the hole with a candle-flame. It will be found that, when the lens is at a certain distance from the card, there is an inverted image of the hole formed on the card. When this is the case, the light leaving the hole and meeting the front surface of the lens is refracted and meets the back surface normally: most of the light passes through; but a small portion is reflected back along the path whence it came, and is sufficient to produce an image easily visible in the day. This distance of the card from the lens, which is the apparent radius of curvature of the back surface seen through the front surface, is throughout this paper called f. The true focal length F of the same lens may be observed in the usual way; but it is more conveniently found by fixing a plane surface of glass behind the lens, when it will be found that another image may be produced when the lens is about twice as far from the card as it was before. Since an image is produced, the light must have returned along the path whence it came, and must therefore have struck the plane surface nor-

* English Mechanic, vol. xxxi. pp. 89, 184, 207.

mally—that is, have left the lens and returned to it as a parallel beam; therefore the card is at the principal focus. For a plane glass surface a piece of plate glass blacked at the back, or the surface of a prism may be used. The observations of the distances F and f can be easily and accurately made; then the radius of curvature may be found from the formula

$$R = \frac{Ff}{F-f},$$

as I shall presently show.

Before doing so, however, I think it well to describe the most accurate method of observing the distances F and f. The card with the pin-hole is convenient; but it is difficult to find the place with great accuracy where the focus is most sharply defined, and to measure the distance when found. All difficulty is completely avoided by the following plan:—Take a piece of thin sheet metal, of the size and shape shown in Pl. I. fig. 1, and fix in front of it, in the position shown by the dotted line, a small reflecting-prism, so that, when a small bright flame is placed on one side of the prism, a beam of light leaves the slit in the plate. Replace the card by this plate and prism, and move the lens till the aerial image of the slit is formed in the corner, close by the edge of the prism. To examine the position of the image with greater precision, an ordinary positive eyepiece will be found convenient. When the image and the slit are equally distant from the lens, there will be no relative movement on moving the eye; if there is relative movement, the distance between the lens and the plate must be increased or diminished according as the plate or the image appears to move with the eye. When the distance has been properly adjusted, it is easily measured by resting a scale on the continuation of the lower edge of the slit, and moving it till it touches the surface of the lens. The position of the edge of the prism or of the slit may then be read with great accuracy; and it will be found that, on repeating the observations several times, a discrepancy more than a tenth of a millimetre between any of the measures need not occur.

Instead of observing the position with an eyepiece, the Foucault plan may be adopted. Place the eye immediately behind the edge of the prism, so that all the light forming the image enters the eye. Move the prism laterally towards the image, which of course moves to meet it, and observe whether the light which fills the lens dies away uniformly, or whether it seems to retreat from one edge of the lens. If the retreat is in the same direction as the movement of the prism-plate, the distance is too small; if in the opposite direction, too

great. Either of these systems will give accurate results; I prefer the first, as tiring the eye less and being, especially with small lenses, the more accurate.

A convenient support for the lens is made by boring a hole, with a less diameter than the lens, in a piece of thin parallel-sided wood. The lens may be slipped under two clips, so as to rest against the edge of the hole on one side of the wood. On the other side a piece of plate glass, blacked at the back, is cemented or held in a similar way by clips. If this piece of wood is fixed vertically on a horizontal slide, it may be moved away from the prism-plate, and the distances f and F determined in a few minutes. Fig. 2 is a horizontal section of the arrangement when the principal focus F is being determined. The dotted line shows the position for f.

If instead of a lens a single surface only is to be measured, there is of course no difficulty in the case of a concave surface; but a convex surface may have its curvature determined in the following way:—Arrange the prism-plate and flame as before. At a distance in front of the prism-plate more than its focal length fix a converging lens, preferably achromatic. Observe the position of the aerial image on the other side of the lens, and make it coincident with the edge of a plate of metal, m. The positions must be so adjusted that the distance of m from the lens is greater than the radius of curvature of the given surface. Now place this surface between the metal plate and the lens, and move it till an image is formed accurately by the side of the prism. Then the light impinging on the convex surface has been reflected back along the path whence it came, and has therefore struck that surface normally; therefore the place m, where those rays would have met had they not been intercepted, is the centre of curvature of the convex surface. Its radius of curvature can therefore be measured by suitably-formed callipers. Fig. 3 is a horizontal section of the arrangement.

I have stated above that $R=\frac{Ff}{F-f}$, in the case of a thin equi-convex lens. This must now be proved, and the more general case of any kind of lens treated next. First, consider that the lens is so thin that any normal to either surface cuts each at points appreciably equally distant from the axis. Since the image which is produced is partly formed of rays which are near the axis, these rays meet the axis at angles so small that the tangent, the sine, and the arc are convertible terms. If the lens is large and not of very long focus, this will not be true of rays from near the edge of the lens; but as these rays are not necessary for the image, the central ones alone may be

employed, and by them the curvature of the whole surface, if spherical, determined.

On the front surface take any point p, and through it draw a radius $m\mathrm{R}$ of the back surface. Join p with f, the apparent centre of curvature of the back as seen through the front surface. Draw also through p a radius ab of the front surface and a line dc parallel to the axis. Then the angles mpd, dpb, apc are equal to one another. Call these angles θ. The angle $apf = \mu \times$ angle $mpb = \mu 2\theta$; therefore the angle $cpf = \mu 2\theta - \theta$. But the angle $ep\mathrm{R} = \theta$;

$$\therefore \qquad \frac{\mathrm{R}}{f} = 2\mu - 1, \text{ or } \mu = \frac{\mathrm{R}+f}{2f}.$$

By the property of equiconvex lenses, $\mu = \dfrac{\mathrm{R}}{2\mathrm{F}} + 1$;

$$\therefore \qquad \frac{\mathrm{R}}{2\mathrm{F}} = \frac{\mathrm{R}-f}{2f} \text{ and } \mathrm{R} = \frac{\mathrm{F}f}{\mathrm{F}-f};$$

or, in a thin equiconvex lens, the radius is equal to the product divided by the difference of the principal focal length and the apparent radius of the back as seen through the front surface.

It might be expected that, as this formula has been deduced from a specially simple case, a more complicated one would be necessary if the two sides of the lens were not equally curved, or if one surface were plane or concave. But such is not the case; the same formula applies in every possible case, though, as will be shown, experimental difficulty occurs in the case of a diverging meniscus.

The proof of the formula in the case of a thin lens which is not equiconvex is similar to that already given. Make the same construction as before, and let R_1, R_2 be the centres of the surfaces 1 and 2, and f_1 the apparent centre of 1 seen through 2. Also let $\mathrm{R}_1 = n\mathrm{R}_2$. Call each of the angles mpd, $cp\mathrm{R}_1$, θ; then the angles $dp\mathrm{R}_2$ and apc will each equal $n\theta$. As the angle $apf_1 = \mu \times$ angle $mp\mathrm{R}_2 = \mu(1+n)\theta$, $\therefore$ angle $cpf_1 = \mu(1+n)\theta - n\theta$. But angle $cp\mathrm{R}_1 = \theta$;

$$\therefore \qquad \frac{\mathrm{R}_1}{f_1} = \mu(n+1) - n.$$

Substitute $\dfrac{\mathrm{R}_1}{\mathrm{R}_2}$ for n, and it will be found that

$$\mu = \frac{\mathrm{R}_1(f_1 + \mathrm{R}_2)}{f_1(\mathrm{R}_1 + \mathrm{R}_2)}. \qquad \ldots\ldots\ldots \quad (1)$$

A similar proof will show that

$$\mu=\frac{R_2(f_2+R_1)}{f_2(R_1+R_2)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

By the property of lenses,

$$\mu=\frac{R_1R_2}{F(R_1+R_2)}+1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where F is the principal focal length.

By combining (1) and (3),

$$\frac{1}{R_1}=\frac{1}{f_1}-\frac{1}{F}, \text{ or } R_1=\frac{Ff_1}{F-f_1}. \quad . \quad . \quad . \quad . \quad (4)$$

By combining (2) and (3),

$$\frac{1}{R_2}=\frac{1}{f_2}-\frac{1}{F}, \text{ or } R_2=\frac{Ff_2}{F-f_2}. \quad . \quad . \quad . \quad . \quad (5)$$

By combining (1) and (2),

$$\frac{1}{R_1}-\frac{1}{R_2}=\frac{1}{f_1}-\frac{1}{f_2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

It is not a little surprising that, whatever the refractive index of the material of the glass, or the curvature of the front surface, the curvature of the back surface can always be obtained from an expression in which both apparently are omitted. They are both of course involved in each observation, F and f, which accounts for the possibility of their being eliminated.

It is interesting to follow the changes which occur between the two extreme limits of form—a double convex and a double concave lens. Take a double convex lens, and suppose one of the surfaces to be gradually pushed in; when it has become plane we have the first particular case—a plano-convex lens. Call the flat surface 1 and the convex surface 2; then, by (4),

$$\frac{1}{\infty}=\frac{1}{f_1}-\frac{1}{F};$$

$\therefore f_1=F$, or the apparent centre of curvature of the flat surface seen through the round surface is at the principal focus. By (5),

$$\frac{1}{R_2}=\frac{1}{f_2}-\frac{\mu-1}{R_2},$$

since $F=\frac{R_2}{\mu-1}$; $\therefore R_2=\mu f_2$, or the apparent radius of 2 is less than the true radius in the ratio of μ to 1.

If the pushing-in process is continued, the surface 1 will become concave. Four observations can then be made—F, f_1, f_2, and R_1; therefore R_2 may be found by either of the

equations (5) or (6). As the surface 1 becomes more concave, its apparent centre on the other side of the lens will retreat to an infinite distance ; and then the concave side will appear flat when viewed through the convex surface. This is the case when $R_1 = -F$, as may be shown by making $f_1 = \infty$ in (4), or as is obvious from a diagram. When R_1 becomes less than this, f_1 becomes an imaginary point on the other side of the lens, such that, if rays were sent so as to converge upon it, they would return as though they had come from it. Its position could be determined experimentally by the method given for a convex surface on p. 33 ; but as the true radius can be determined directly, there is no necessity to find this imaginary apparent radius.

Let the concavity of surface 1 increase ; the next particular case is that of a watch-glass, where $R_1 = -R_2$. Then F becomes infinite, and the two points f_2 and R_1 become coincident. When the surface 1 becomes still more concave, F becomes negative and virtual, and R_1 and f_2 pass one another. The experimental determination now becomes more difficult; for neither can F or f_1 be observed directly; but still the equations (5) and (6) hold. They may each be found by the method for a convex surface, which is less convenient than the direct method.

If the concavity of the surface 1 continues to increase, another limit will be reached, at which f_2 becomes infinite. This is obviously the case when $R_2 = -F$; that is, when the focal length has been so shortened by the increasing concavity as to be equal to the radius of the convex surface. When this is the case, the surface 1 seen through 2 appears plane. When the concavity passes this limit, f_2 becomes negative and imaginary, and the experimental difficulty is still further increased, for R_1 only can be directly observed ; but still the equations (4), (5), and (6) are true. No further increase in the concavity of 1 will produce any new conditions. Now, the curvature of 1 remaining constant, let 2 become flatter ; when it has become plane, there is no occasion to observe F, f_1, or f_2 to determine the form of the surfaces. When 2 becomes concave also, the curvature of each surface can be directly measured; and all difficulty is removed. Every possible case has now been considered ; and though the equations are always true, experimental difficulty only occurs in the two classes of diverging meniscus.

If a parallel beam of light falls on the lens, it will be refracted at the front surface, partly reflected from the back, and again refracted at the back surface, and be brought to a focus at a distance from the lens equal to half the apparent radius.

As the light, in its passage to and from the apparent centre f_1, is twice refracted by 2 and once reflected by 1, it would seem at first sight that the value of f_1 might be obtained by combining in the usual way the expression for twice the focal length of an equiconvex lens with surfaces having the same curvature as 2 and the radius of 1 ; thus

$$\frac{1}{f_1}=\frac{1}{2F}+\frac{1}{R_1}.$$

But this operation, depending on a false assumption, leads to an erroneous result. It makes $f_1=\frac{R_1R_2}{\mu-1R_1+R_2}$, instead of $\frac{R_1R_2}{\mu-1R_1+\mu R_2}$. The error arises in this way:—When a double convex lens is employed, either to bring or to hurry light to a focus, the bending-powers of the two surfaces depend on the angles they make with the ray in the lens. Now, if one of these angles is great, the other must be small; so that, as a combination, they have the same focus-shortening power, however the light falls on them. But when a ray passing from and returning to the apparent centre f strikes the front surface, that surface makes an angle with the ray in the lens which is greater than the mean in the ordinary way; therefore the surface produces a greater diverting effect; and hence the distance f is less than it would be if the supposition made were correct.

All that has been shown at present is only true when the thickness of the lens is inappreciable. When this is not the case, rays, whether from the principal focus F or from an apparent centre f, will not cut the two surfaces at points equidistant from the axis. First, consider the case of an equiconvex lens. Let fig. 5 represent a portion of a thick equiconvex lens. As before, since the central rays are sufficient to give an image, arcs, sines, and tangents may be considered identical. On one surface take any point p. Through it draw a radius pR, and the line pbc parallel to the axis. Now a ray of light parallel to the axis, meeting the surface in the point p with an angle of incidence equal to θ, will be refracted so that the angle dpe is equal to $\frac{\theta}{\mu}$; therefore the angle $bpd=\theta\frac{\mu-1}{\mu}$. Therefore the line pd continued will meet the axis in a point a such that

$$pa=\frac{\mu R}{\mu-1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

But this ray is diverted at the point d, and bent down so as to

meet the axis at the principal focus F. It is required to find the length dF, or, shortly, F—that is, the distance of the principle focus from the surface. Since the line pa has cut the front surface at a point d nearer the axis than p, the inclination of the normal, bs, at d will be less than θ, and will equal $m\theta$ if

$$m=\frac{pa-t}{pa}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

t being the thickness of the lens. Now the angle pds = the angle dbp + the angle $bpd=\theta\left(m+\frac{\mu-1}{\mu}\right)$; therefore the angle $bd\mathrm{F}=\mu\theta\left(m+\frac{\mu-1}{\mu}\right)$, and the angle

$$qd\mathrm{F}=\mu\theta\left(m+\frac{\mu-1}{\mu}\right)-m\theta=\theta(1+\overline{m}\,\overline{\mu-1}). \quad . \quad . \quad (3)$$

Since the lines dF and da leave the same point d with different inclinations, they will meet the axis at distances which are inversely as these angles. Therefore

$$\frac{da}{\mathrm{F}}=\frac{\overline{1+m}\,\overline{\mu-1}}{\overline{\mu-1}}=\mu\overline{1+m};$$

therefore

$$\mathrm{F}=\frac{pa-t}{\mu(1+m)}=\frac{\mathrm{R}\left(\frac{\mu\mathrm{R}}{\mu-1}-t\right)}{2\mu\mathrm{R}-t(\mu-1)}, \quad . \quad . \quad . \quad . \quad (4)$$

by (1) and (2). This is the distance of the principal focus from the surface of the lens.

The distance ef of the apparent centre from the surface may be found in a similar way. The normal at e makes an angle $n\theta$ less than θ, such that

$$n=\frac{\mathrm{R}-t}{\mathrm{R}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The angle $pet=(n+1)\theta$; the angle $cef=\mu(n+1)\theta$; and the angle $kef=\theta[\mu(n+1)-n]$. Therefore

$$\frac{ef}{\mathrm{R}-t}=\frac{1}{\mu(n+1)-n}, \text{ and } ef=\frac{\mathrm{R}\overline{\mathrm{R}-t}}{(\mu-1)(\mathrm{R}-t)+\mu\mathrm{R}}. \quad . \quad (6)$$

This is the distance of the apparent centre from the surface. By (6),

$$\mu=\frac{(\mathrm{R}+f)(\mathrm{R}-t)}{f(2\mathrm{R}-t)}.$$

Substitute this value of μ in (4); on simplifying, it will be

found that

$$R^2 - R\left(\frac{f^2}{F-f} + 2t\right) = \frac{Ff^2}{F-f} - t^2, \quad \ldots \quad (7)$$

from which R may be found if F, f, and t are given. If t is made equal to 0, equation (7) gives

$$R = \frac{Ff}{F-f}, \text{ or } -f.$$

The first result is the same as that already found for a thin lens; while the value $-f$ seems to have no physical meaning.

If the thick lens is not equiconvex, there are five observations possible—the distances of the two apparent centres from the surfaces, the distances of the two principle foci from the surfaces, and the thickness; but there are only three things to be determined—the two real radii and the refractive index: therefore the equations for R_1, R_2, and μ must be capable of solution. The following are the expressions which may be found by a similar treatment of fig. 5 to that already employed in the case of the equiconvex lens, if it be remembered that all the angles made by surface 1 are $\frac{R_2}{R_1}$ times those made by surface 2 at the same distance from the axis. They are

$$F_1 = R_2 \frac{\frac{\mu}{\mu-1}R_1 - t}{\mu(R_1+R_2-t)+t}, \qquad F_2 = R_1 \frac{\frac{\mu}{\mu-1}R_2 - t}{\mu(R_1+R_2-t)+t},$$

$$f_1 = R_2 \frac{R_1 - t}{\mu(R_1+R_2-t)-(R_1-t)}, \quad f_2 = R_1 \frac{R_2-t}{\mu(R_1+R_2-t)-(R_2-t)}.$$

The first two of these equations give

$$\frac{F_1}{F_2} = \frac{\mu - R_1 t(\mu-1)}{\mu - R_2 t(\mu-1)};$$

and the second two give

$$\mu = \frac{\overline{R_2+f_1}\,\overline{R_1-t}}{f_1(R_1+R_2-t)} \quad \text{and} \quad \mu = \frac{\overline{R_1+f_2}\,\overline{R_2-t}}{f_2(R_1+R_2-t)}.$$

By these μ may be eliminated. The solution for R_1 and R_2 I have not obtained; but I do not think there is any difficulty.

The following application to the case of liquids of the principle of making the rays return along the path whence they came, forms a neat though impracticable method of determining their refractive index when greater than $\sqrt{2}$:—Replace the cross-wires of a telescope by a prism-plate, as already described, but in which the slit is longer and adjustable. Fix opposite the object-glass a piece of parallel-sided plate-glass, with its plane at right angles to the optical axis. The cor-

rectness of this adjustment may obviously be determined at the eyepiece. Let the glass plate form the bottom of a trough in which the liquid may be placed, and let there be under this a dish containing mercury. Now, there is a certain inclination of the telescope at which the beam from the illuminated slit, rendered parallel by the object-glass, is refracted at the free surface of the liquid, and again at the surfaces of the glass plate, so as to leave the latter vertically; then, striking the mercury, it returns along the path whence it came, and may be viewed by the eyepiece. Under these conditions the beam of light on either side of the prism is at right angles to the opposite side; therefore they make equal angles with the adjacent sides, and the prism is at minimum deviation. Calling the inclination of the telescope θ, we have $\sin\theta = \mu \sin\frac{\theta}{2}$; therefore $\mu = \sqrt{2(1+\cos\theta)}$. The telescope may be first inclined on one side and then on the other, and half the angle moved over taken as θ. I have found it utterly impossible to get the sodium-line to keep still for a moment, or to be even fairly defined, as every movement in the neighbourhood produces a tremor on the surface of the liquids, which, in the Science Schools at Kensington at any rate, is so continuous as to make it impossible to observe with accuracy. I should have said that the top of the prism must be covered in with a glass plate, to prevent the evaporation and consequent superficial cooling of the liquid forming the prism, which causes striæ in the liquid, spoiling definition even more than the continuous tremor.

Helmholtz and others have shown that, during accommodation of the eye for near objects, the cornea does not change in curvature, the front surface of the lens becomes more curved and advances, and the back surface does not appreciably change. The proof given is that the images of a light produced by reflection from the cornea and from the back surface of the lens do not change, while that produced by the front surface of the lens advances and becomes smaller. Now it would appear at first sight that these observations prove a flattening of the back surface of the lens during accommodation; for if it did not change in curvature, the rays of light passing twice through the more curved front surface would sooner come to a focus; but since they do not apparently sooner come to a focus, it would seem that a flattening of the back surface must have occurred to counteract the shortening influence of the more curved front surface. In the case of ordinary lenses this would be so; but it so happens that in the crystalline lens the focus by reflection is formed

within it, and so the more curved front surface magnifies the smaller image, which therefore appears unchanged.

While on the subject of reflection in lenses, I think it worth while to mention that convex lenses silvered at the back make excellent and easily constructed concave mirrors. Since both surfaces conduce to bring light to a focus, flatter curves may be used than are necessary for a plain concave reflector of the same focal length; also, since the two surfaces are not parallel, false images are not produced; so that the advantage of glass silvered at the back remains, without the usual disadvantage. A spectacle-lens of about five inches focal length, silvered at the back and mounted, forms an eye-glass (I mean a glass for examining the eye) which every one who works in metal should possess. I have found by its means specks of metal, thrown from the lathe, which were utterly invisible by other means, but which were nevertheless exceedingly painful.

III. *Experiments on the Faure Accumulator.*

By Professors W. E. Ayrton *and* John Perry*.

HAVING made, at the request of the Faure Accumulator Company, a series of experiments on some of their cells, we have thought that a short account of some of the results obtained may not be uninteresting to the members of the Physical Society.

The object of the experiments was to ascertain, *first*, the efficiency of a cell—that is, the ratio of the energy given out by it to the energy put into it; *secondly*, the storing-power of a cell; and, *lastly*, whether or not there was a deterioration in its working-powers. To measure the energy put into any electric circuit, we have merely, of course, to take time-readings of the current flowing through the circuit, as well as the difference of potentials between its two extremities. The current in ampères multiplied by the electromotive force in volts and by 44·25, gives the number of foot-pounds per minute that is being put into that part of the circuit as electric energy. For measuring the current we have used throughout our ammeters (short for ampère-meters), and for measuring electromotive force our voltmeters, the latter being employed of course in a shunt circuit.

Of the total electric energy put into the circuit, and which is measured, in foot-pounds per minute, by 44·25 AV, a portion will be employed simply in heating the circuit, and the

* Communicated by the Physical Society, having been read at the Meeting on February 25, 1882.

remainder may be utilized in producing useful work. For example, if a time-curve be drawn for 44·25 AV when charging a Faure accumulator, the area of the curve will measure the total energy put into the accumulator in foot-pounds; but of this some portion has been wasted in heating the cell, due to the charging having been more rapid than was absolutely necessary. It was, of course, of considerable importance in our experiments to ascertain what portion of the energy put into the cell was really thus wasted; and to measure this the following experiments were made.

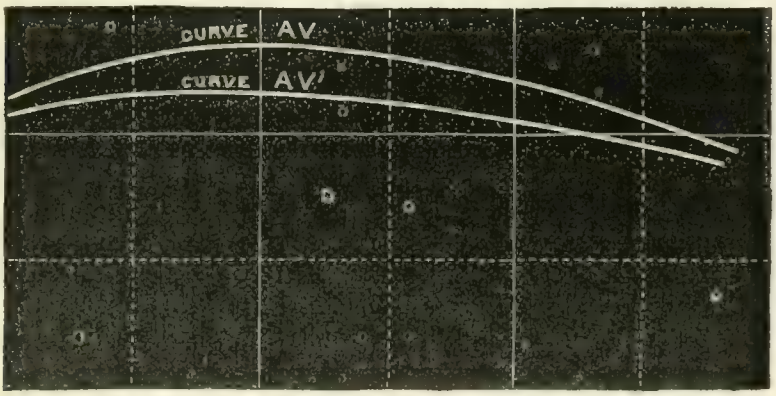
Occasionally the main current was stopped, the shunt current through the voltmeter being left completed. The reading now on the voltmeter gives the difference of potentials produced by the cell itself, whereas the previous reading was the combined difference of potentials produced by the cell and the dynamo-machine charging it. If now a new time-curve be drawn in which the ordinates represent the product of 44·25 AV′, where V′ is the electromotive force of the cell measured on the circuit being broken, and A is the mean value of the current flowing just before breaking and just after closing the circuit, the area of the new curve will represent that portion of the energy put into the cell which is usefully employed in chemical decomposition. The difference between the areas of these two curves represents, then, the amount of energy wasted in heating the cell in foot-pounds.

Again, on discharging the cell, experiments of a similar nature have to be made. The product 44·25 AV represents the number of foot-pounds of work per minute the cell is producing in the external circuit, V being the difference of potentials between the two poles of the cell while it is discharging; but, in addition, there is a certain amount of energy which is being expended in heating the cell itself during discharge.

This, as before, may be ascertained by breaking the main circuit, leaving the shunt-voltmeter circuit completed. The reading on the voltmeter V′ now indicates the real electromotive force of the accumulator during discharge; whereas the previous reading, obtained just before breaking the circuit, represents merely the fraction of the total electromotive force employed in sending the current through the external resistance. If a time-curve be drawn with its ordinates proportional to 44·25 AV′, where A is the mean value of the current just before breaking and just after closing the circuit, its area will represent the total number of foot-pounds of energy per minute being given out by the cell; and the difference between the areas of the last two curves will represent the number of foot-pounds of energy employed in heating the cell itself. It

is to be noticed that during charging V′ is less than V, whereas on discharging V′ is greater than V.

An examination of thirty-five sheets of time-curves, which we have drawn from the experiments we made, shows that, in charging, the curve for AV rises at first; and as it rises more rapidly than that for AV′, this means an increase in the resistance of the accumulator.



As the charging continues, the two curves for AV and AV′ approach one another, showing that the internal resistance of the accumulator diminishes again. On the other hand, at the end of a long discharge the curve for AV falls more rapidly than that for AV′, due to an increase in the internal resistance. Now our experiments show a great constancy in the electromotive force of a Faure cell, and that the falling-off in discharging which occurs during a very rapid discharge, or at the end of a long discharge, is due more to an increase in the internal resistance of the accumulator than to a diminution in the electromotive force, which our methods of experimenting above described enable us to separate and measure independently. But, whether discharging rapidly or whether discharging slowly, there is a most curious resuscitating-power in the cell, which, if disregarded, will cause totally erroneous underestimates to be made of the efficiency of the cell.

This resuscitating-power is more marked for rapid discharges than for slower ones. In the case, for example, of an extremely rapid discharge, we found that when the flow had become apparently so feeble that the cell appeared totally discharged, leaving the poles of the cells insulated caused three times as much electric energy to be given out all together in the second discharge as had been given out in the first. And even when several days are taken to discharge the cell—and we may mention that we have had continuous observations made day and night for several days in certain cases—this

resuscitating-power is wonderfully marked. An insulation of a few hours will cause the energy given off per minute on redischarging to be eight to ten times as great as it was before insulation. Indeed on one occasion, after a cell had apparently nearly discharged itself, it was left shortcircuited with a thick wire for half an hour, then insulated all night, when the number of foot-pounds of work per minute given off at the commencement of the discharge the following morning was found to be ten times as great as it was on the previous evening, and a greater amount of energy was actually taken from it in the second discharge than in the first. This phenomenon gives the Faure accumulators a great value for tramcar propulsion, since, as is well known, it is just on starting after stopping that the strain on the horses is so great.

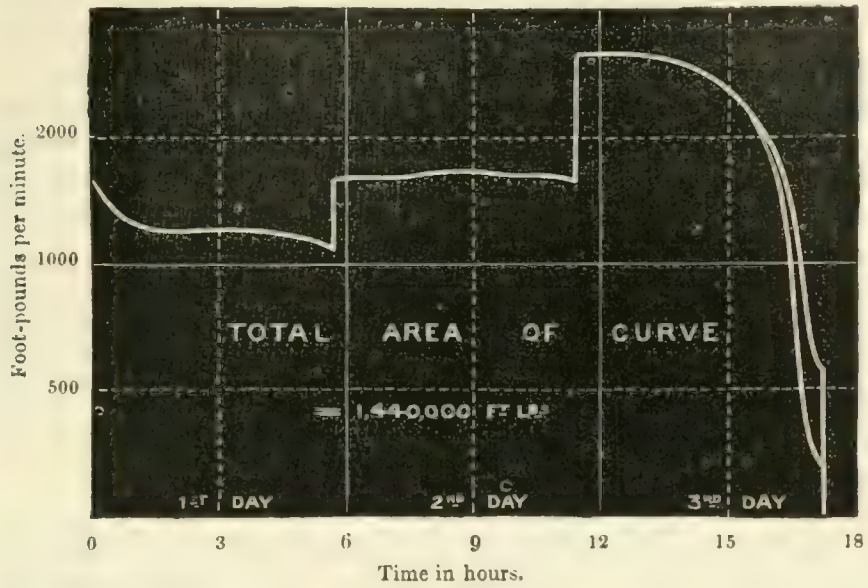
Efficiency.—To determine the efficiency of cells we commence with them empty, or at least as empty as many hours' shortcircuiting with a thick wire could make them. We then measured the total amount of energy put in and the total amount subsequently given out, and we found that, for charges up to a million foot-pounds put into the cell and discharged with an average current of 17 ampères, the loss in charging and discharging combined may not exceed 18 per cent. Indeed, for very slow discharges the loss in charging and discharging combined in some of our experiments has been as low as 10 per cent.

Storing-power.—It is a little difficult to measure the maximum storing-capacity of the cell at the same time that measurements are made of its efficiency, because in the latter case we must take care that we do not put in more electric energy than the cell can hold; on the other hand, if precautions are taken to avoid overcharging, it is a little difficult to ensure that the full charge has been put in. We have therefore separated our experiments for measuring the efficiency from those employed to ascertain the storing-power.

Let us take a single example of the storing-capacity. A certain cell containing 81 lb. of lead and red lead was charged and then discharged, the discharge lasting eighteen hours—six hours on three successive days; and it was found that the total discharge represented an amount of electric energy exceeding 1,440,000 foot-pounds of work. This is equivalent to one horse-power for three quarters of an hour, or 18,000 foot-pounds of work stored per pound weight of lead and red lead. The curve shows graphically the results of the discharge.

Horizontal distances represent time in minutes, and vertical distances foot-pounds per minute of energy given out by the cell, and the area of the curve therefore the total work given

out. On the second day we made it give out energy more rapidly than the first, and on the third more rapidly than on



the second, this being done of course by diminishing the total resistance in circuit. During the last day we were discharging with a current of about 25 ampères. And this cell, like the others, showed, on being insulated after having been apparently totally discharged, that there was still a large charge stored up; hence the numbers given above for the capacity are probably under the total value.

Deterioration.—As to deterioration, two months constant charging and discharging of the two accumulators under test showed no signs of deterioration.

IV. *A Simplified Dispersion-Photometer.*

By Professors W. E. Ayrton *and* John Perry*.

IT will be in the recollection of the Members that in 1879 we described to the Society a dispersion-photometer which enabled measurements to be made of the intensity of the strongest electric light in a small room and for the rays coming from the electric light at any angle—two essentials which appeared to us necessary in an electric-light photometer. The principle of this photometer consisted in our use of a concave lens to weaken the strength of the light, so as to make the illumination of a screen comparable with the illumination of a standard candle, instead of keeping the lamp a distance of

* Communicated by the Physical Society, having been read at the Meeting on February 25, 1882.

50 or 100 feet away, which was the plan in use until that time. We exhibit now five successive forms of the instrument, which illustrate the history of its development to the present time.

1. The first of these is very nearly the same as that described in our former paper, with the exception that we discarded the use of a long screw (shown in our original figure) for adjusting the position of the lens—as we found that a very easy adjustment might be effected with the fingers, the tension of the bellows part making an automatic clutch which fixed the lens-slide in any position.

2. The second specimen is on the same principle, only that telescope-tubes are used instead of a wooden frame and a bellows. Instead of the lens part alone tilting when the elevated or depressed light has to be examined, the candle-box is here made to tilt also, the candle being supported in gimbals so that it may remain vertical for every angle of elevation.

3. The third specimen is on pretty much the same principle; but as we found a difficulty in comparing two illuminated disks whose centres were some distance apart, we arranged in front of these disks two mirrors, which enable us to make the comparison between two illuminated semicircles having the same diameter. The difficulty of adjusting the lens and making a comparison of the illuminations, and reading the scale, without moving one's head, in all these early instruments led us to the

4th form, which is probably familiar to the Members, as it was exhibited at Paris and largely used there for measurements. In this the candle-box and the lens-box are placed end to end, the lens is fixed in a wooden piston which moves in its hollow square box, which is lined with velvet; and the lens shows its position by a pointer moving over the scale outside. The pointer projects from the inside of the wooden cylinder at any point of a long slot, whose sides are made of india-rubber tubing, so that no extraneous light can reach the illuminated screen. A little handle working a rack and pinion enables the lens to be placed in any position. Through a hole at the side the two screens can be viewed reflected in two mirrors, inclined to one another in the space between the candle-box and the lens-cylinder; and the illuminated papers are viewed as two semicircles having a common diameter. In front of this hole we have slides of red and green glass; so that, as our custom has always been, we make two measurements—one a comparison of the ruby-red light of the lamp examined with the red light of the candle, and another of the green lights. This instrument differed from the earlier forms in not requiring any calculation to be made of the strength of the

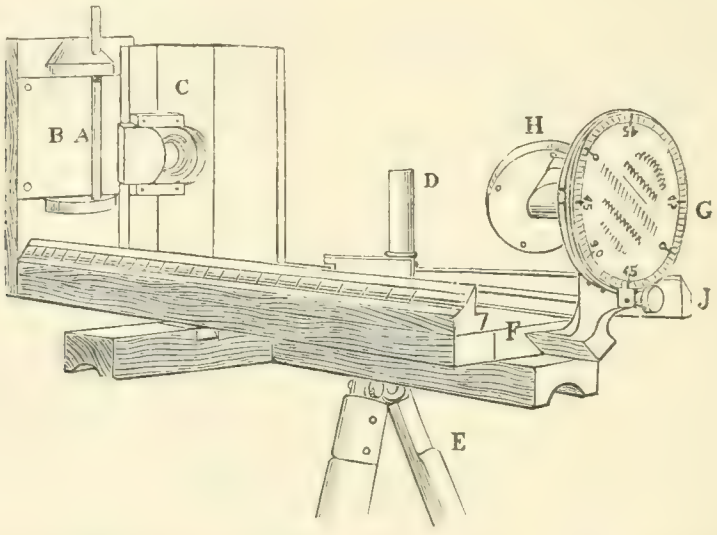
light; that is, the reading of the pointer was not merely a reading of its distance from the screen, but it was a reading in standard candles of the power of the light. Three such scales were placed on the instrument; and there were three certain distances at which the lamp had to be placed for examination. The tilting-arrangement was of course different from that of the earlier forms.

As the instrument had by this time (the end of last year) come into a rather extensive practical use, we had opportunities of seeing that, as an instrument to be used by unscientific persons, it was not yet in a perfect condition, in spite of the many changes that had been made in its construction. The most important difficulty was due to the fact that a slight lateral change in the position of the observer's eye caused the apparent illumination of the screens to vary. Being aware of this fact ourselves, we maintained a certain fixed position of the eye when making observations; but the instrument could not at once be used by persons not accustomed to make delicate experiments.

5. The fifth form, which we now present to the Society, is the outcome of our labours on this subject. We have all along seen the disadvantage of using the Bouguer's two-screen method, since, when lights are examined that have passed through tissue or tracing-paper, a very slight change in the position of the observer's eye makes a very great difference in the apparent illumination, whereas, using Rumford's method, when a sheet of white blotting-paper is employed as a screen very considerable changes in the position of the eye produce no change in the apparent illumination—a result, however, which is not attainable when ordinary drawing-paper is used as the screen. If, however, Rumford's method is to be used to measure the rays coming at different angles from an electric light, a mirror must be employed to reflect them successively onto the same screen; and if used in the ordinary way, the angle of incidence of the rays on the mirror will be different in different cases. Now the difficulty that always met us arose from the inequality of the reflecting-power of an ordinary mirror for rays falling on it at different angles of incidence. We have, however, completely overcome this difficulty in an extremely simple way, by causing the mirror to turn about a horizontal axis inclined at 45° to its plane, and the whole photometer to turn about a vertical axis. With this arrangement the angle of incidence, and consequently the proportional absorption, is the same whatever be the inclination of the rays coming from the lamp to the mirror; and, further, the angle being 45°, the amount of rotation of the

mirror about its horizontal axis necessary to enable measurements to be made of rays coming at any angle, after measurements have been made of the horizontal beam, is exactly equal to the inclination of the beam in question.

Using Rumford's method in this latest form of our photometer, we are to a great extent independent of the presence of other sources of illumination of the screen, so that the apparatus need not be enclosed in a box. At the same time, however, the sensibility of the test is much increased by placing a shade to prevent the electric light shining directly onto the screen. On this screen of blotting-paper, B, is thrown the shadow of a black rod, A, placed in front of it, by a candle in the candle-holder, D.



Now it is well known that if an electric light is also allowed to illuminate this screen, and to throw a second shadow of the rod A on the paper, and if the candle is adjusted at such a distance that the two shadows are of equal intensity, the strength of the light is to that of the candle in the ratio of the squares of their distances from their respective shadows. But instead of allowing the strong light to pass directly to the screen, we cause it to pass through the concave lens in the sliding wooden frame C. A pointer on this slide tells the distance of the lens from the screen. As you are all aware, the weakening of light-intensity produced by the lens enables us to leave our electric lamp within a few feet of the instrument. We have experimentally found that there is no appreciable loss of light in passing through the lens. The candle slides on the bar J; and its distance from its shadow is shown by a pointer on a scale. If f is the focal length of the lens, D the distance of the electric light from the paper-screen, d

the distance of the centre of the lens from the screen, and c that of the candle when the shadows show equal illumination, then, if L is the strength of the examined source of light in standard candles,

$$\sqrt{L} = \frac{D-d}{c}\left\{1 + d\left(\frac{1}{f} + \frac{1}{D-d}\right)\right\},$$

or

$$L = \frac{1}{c^2}\left\{D + \frac{d(D-d)}{f}\right\}^2.$$

For our own use we prefer to employ the formula; but as all the common instruments which have hitherto been manufactured have lenses whose focal length is 4 inches, we have prepared a table, a copy of which is sent out along with each instrument, in which the value of L is given for various values of D, d, and c. Using this table, it is necessary to have the lamp at either 60, 120, or 300 inches from the screen; the candle is either at 10, 14·14, or 20 inches from the screen; and the table is made out for every half inch of the lens-scale. But inasmuch as we find that the improved arrangement of the mirror already referred to constitutes perhaps the most useful part of the instrument, and as the use of this improvement involves many alterations of D, the manufacturer proposes in future not to furnish any table of values of L unless specially asked for.

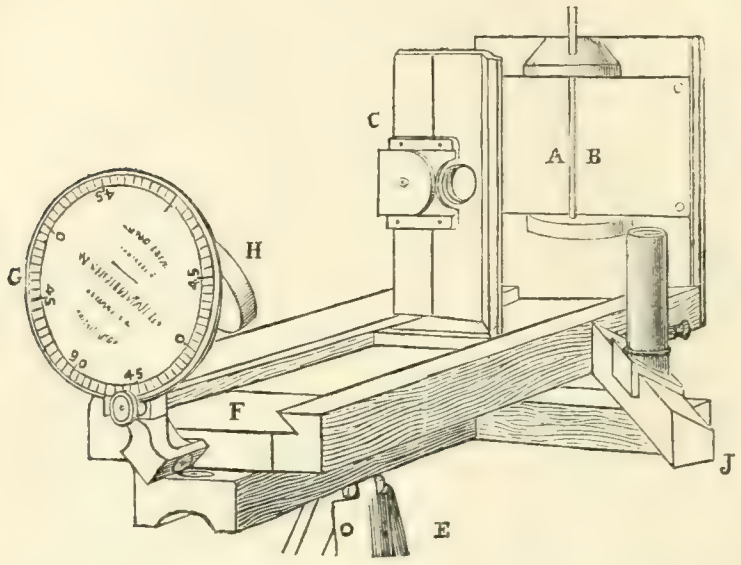
H is the plane silvered-glass mirror which makes the angle of 45° with the axis of the lens, and with the axis about which the mirror itself is free to revolve. As already explained, a ray of light reflected from the mirror and passing through the centre of the lens must, for any position of the mirror, have an angle of incidence of 45°, and so must experience the same amount of absorption, from whatever direction it may have come to the mirror. Further, this angle being 45°, a fixed pointer marks on the graduated circle G, which moves with the mirror, the angle which any ray we may be examining makes with the horizontal.

In this instrument we find that from 30 to 34 per cent. of the incident light at 45° is absorbed, whether this light is of ruby-red or signal-green colour; so that we have the easy practical rule for all cases—add one half to the measured intensity of light reflected.

We need not here refer to the fact that, when investigating the efficiency of an electric lamp, we always measure the horse-power given electrically to the lamp simultaneously with the photometric measurement.

The lamp is suspended in such a way that it can readily be

placed at any elevation. The frame of the tripod-stand is first levelled. A pin at F, directly underneath the centre of the



mirror, passes through the base of the photometer and fits into a hole in the top of the tripod-stand. The photometer, by turning round this pin, can, without producing any change in the distance of the centre of the mirror from the lamp, and therefore without changing the distance from the screen to the lamp, receive the small horizontal motion necessary for the adjustment of a new inclination of the rays coming from the electric light, without any alteration of the distance of the centre of the mirror from the lamp. The divided circle is clamped with the index at 0°; the lamp is lowered or raised till the illuminated disk formed by the reflected light, passing afterwards through the lens, is in the middle of the paper screen. A little sliding shutter with a fine hole in its centre, seen in the figure, enables a very exact adjustment to be made; but in practice we find that we get sufficient accuracy without the use of the shutter. We now measure the distance from lamp to centre of mirror in inches. Equalizing the intensities of the two shadows by adjusting the lens-slide when looking at them through red or green glass, we now note the lens- and candle-readings; and we repeat these operations, changing from red to green and green to red about five times in a minute. The lamp is now raised or lowered and fixed in any position; a few seconds suffice to turn the mirror so that it sends its centre ray exactly through the centre of the lens. The distance from screen to mirror in this instrument being 22 inches, if δ is the distance from centre of mirror to vertical from lamp.

and if θ is the angle of elevation, then

$$D = 22 + \delta \sec \theta.$$

Using this value of D in the formula above, and adding one half to the strength of the light to make up for absorption, the true intensity of the light in standard candles can be ascertained. We find in practice that, if an electric light is moderately steady, ten measurements may be made, with some confidence in their accuracy, in two minutes; and the light may be measured in ten different positions, from an angle of depression of 60° to an angle of elevation of 60°, 100 observations being taken, in less than half an hour.

We may mention one very important result we have been led to by the systematic employment of a photometer which can be used close to the electric light; and that is the large amount of absorption that occurs on certain days when the rays from strong electric lights, and especially the green rays, pass through the air which appears to the eye perfectly clear. At first we were inclined to think the higher results for the candle-power of a lamp obtained with our dispersion-photometer than those obtained with an ordinary distance-photometer were due to some error in our photometer itself; but we have since ascertained that this is due to the absorption of the air—because we find that, if simultaneous measurements are made with ordinary Rumford's photometers, each without lens or mirror, placed at different distances from the lamp in the same azimuth and in the same horizontal plane, the nearer one gives, as a rule, the highest readings; and the difference is the greater the stronger the light, and is greater if the light be examined at each photometer with green glass.

V. *On the Connexion between Viscosity and Density in Fluids, especially Gaseous Fluids.* *By* E. Warburg *and* L. v. Babo *.

THE laws according to which the elasticity and viscosity of a body are connected with its density are of great simplicity in the case of gaseous bodies. The elasticity of these, *i. e.* the reciprocal of their compressibility, is given, according to Boyle and Mariotte's law, by the pressure, and is proportional to the density; the viscosity, measured by the coefficient of friction, is, according to Maxwell's law, independent of the density.

It is known that the first of these laws, that which refers to

* Translated from the *Sitzungsberichte der K. Preuss. Akademie der Wissenschaften zu Berlin*, May 4, 1882, pp. 509–514.

elasticity, holds only approximately, and even that only at moderate degrees of density; at higher densities, according to the investigations of Natterer, Andrews, Cailletet, and others, the connexion between elasticity and density is not even approximately given by Boyle's law, but is apparently more complicated. It can, however, according to van der Waals*, be explained from the kinetic theory of gases, if the volume of the molecules and the attraction between them be taken into account.

Corresponding investigations in relation to the viscosity of gases have hitherto been carried out only so far as Kundt and one of us† have studied the deviations of Maxwell's law at very slight densities. For higher degrees of density the connexion between viscosity and density has not yet been investigated. For the solution of this problem (treated in the present paper for one substance, viz. carbonic acid) the corresponding values at constant temperatures of the coefficient of friction, the density, and, for many reasons, the pressure must be determined.

We employ as the measure of the pressure the inverse value of the volume of a mass of nitrogen at constant temperature of the apartment, the volume of that mass at the pressure of one atmosphere being put =1. To measure the pressure according to this definition a nitrogen-manometer was employed, which was always attached to the principal apparatus, and permitted pressures between 30 and 120 atmospheres to be evaluated.

The density of the substance heated above the critical temperature we determined by a volumetric measurement of the carbonic acid, which at each transition from a greater to a less density was liberated from our apparatus, the volume of which was known to us; the density of the mass in the apparatus after the conclusion of a series of experiments we calculated from the pressure, which then amounted to about 30 atmospheres, by Clausius's formula‡, which at so small a pressure is sufficiently accordant with the observations. At the temperature 32°·6 our experiments comprise the interval of densities between 0·1 and 0·8.

For the determination of the friction-coefficient we employed the method of flow through capillary tubes. The capillary, placed vertical, ended below in a measuring-tube which dipped in mercury, above in a space A, which could from time to time be shut off from the rest of the space by a cock, and in which a diminution of pressure could then be

* *Dissertation*: Leyden, 1873. † Berlin *Monatsberichte*, 1875, p. 160.
‡ Wiedemann's *Annalen*, ix. p. 348.

produced by discharging carbonic acid. After the mercury had been thereby raised in the measuring-tube, the spaces A and B were again put into communication. From the time occupied by the mercury in the measuring-tube in descending from one mark to another, the coefficient of friction was calculated by means of the constants of the apparatus.

Three capillaries were employed, from 6 to 7 centim. in length, and of which the radii amounted to 0·005162, 0·003601, and 0·002847 centim. The validity of Poiseuille's law was controlled; but an equation cannot be deduced from the experiments.

The results obtained are contained in the following Table, in which t denotes the temperature measured by the air-thermometer, s and μ the density and the friction-coefficient in the gramme-centimetre-second system, and p the pressure in the measure above-mentioned; λ is the air-content of the substance, in parts of a volume, as given by analysis. The density of the liquid carbonic acid is taken from Andreef's experiments*.

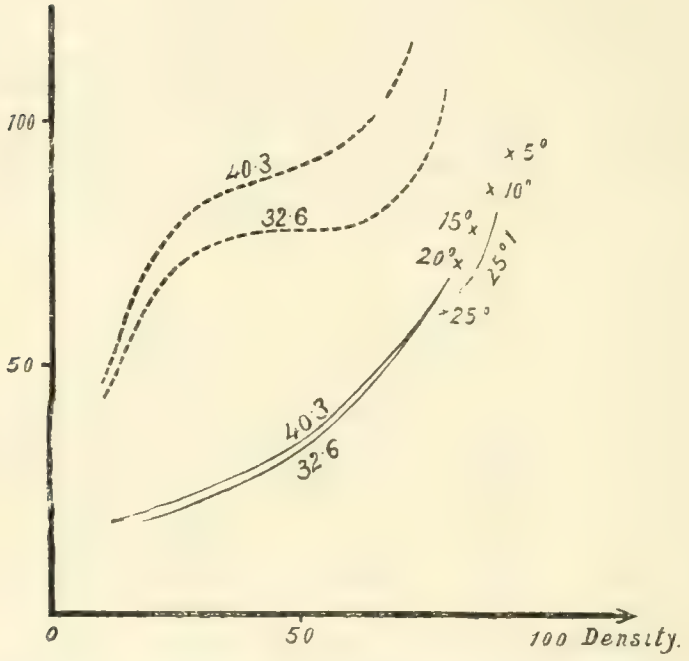
	$t=32°{\cdot}6$. $\lambda=0{\cdot}00074$.		$t=40°{\cdot}3$. $\lambda=0{\cdot}00085$.	
s.	p.	$\mu\,.\,10^6$.	p.	$\mu\,.\,10^6$.
0·800	107·3	677		
0·730	88·5	574	114·6	580
0·660	80·7	493	101·6	499
0·590	78·2	414	94·9	426
0·520	77·6	351	91·7	366
0·450	77·2	304	89·2	316
0·380	76·6	270	86·8	275
0·310	74·6	239	82·7	243
0·240	69·9	213	75·9	218
0·170	60·3	188	64·3	196
0·100	43·1	...	45·3	180

$t=25°{\cdot}1$ $\lambda=0{\cdot}00044$.		
p.	s.	$\mu\,.\,10^6$
105	0·896	800
95	0·875	741
85	0·858	703
75	0·827	665
70	0·809	628

* *Annalen der Chemie und Pharmacie*, 1859, cx. p. 1.

Viscosity of liquid carbonic acid under the pressure of its saturated vapour.
$\lambda = 0{\cdot}0018$.

t.	s.	$\mu . 10^6$.
°		
5	0·922	925
10	0·895	852
15	0·864	784
20	0·827	712
25	0·783	625
29	.....	539



The figure gives a graphic representation of the results—namely, the isotherms* of viscosity and tension, the latter in dotted lines, noted according to the Table.

Andreef's values of the pressure † exceed ours but little—at 32°·6, on an average, about one atmosphere. This may arise from the air-content of the carbonic acid being in Andreef's experiments somewhat less than in ours. The values of p calculated by Clausius's formula, however, are not inconsiderably higher than those observed by us; the differences increase with the density, and reach the value of 10–12 atmospheres. On account of the agreement of our results

* We thus name lines the abscissæ of which are proportional to the densities, and the ordinates to the friction-coefficients and pressures respectively.

† Pogg. *Ann.* Erg. Bd. v. p. 79.

with those of Andreef, found by a quite different method, it is not likely that the differences are due to errors of observation.

Respecting the viscosity, especially its connexion with the density, the results are as follows:—

I. *Above the critical temperature, Gaseous Carbonic Acid.*

1. To the maximum of compressibility $\left(\frac{1}{s\frac{dp}{ds}}\right)$, *i. e.* to the minimum of elasticity $\left(s\frac{dp}{ds}\right)$, given by observation corresponds no minimum of viscosity, which much rather increases in a constantly increasing ratio with increasing density.

$$\left(\frac{d\mu}{ds} \text{ and } \frac{d^2\mu}{ds^2} \text{ are always positive.}\right)$$

2. At the density 0·1, about 500 times the normal, the coefficient of friction exceeds the normal (0·000165 for 40°·3) by only about 9 per cent. of the latter.

3. At the temperatures 32°·6 and 40°·3 the substance shows, at equal density, slightly different values of μ, very different values of p. According to this the viscosity appears to be much more simply connected with the density than with the pressure.

4. The influence of the temperature upon the viscosity, at constant density, is so small that it cannot be inferred with perfect certainty from observations embracing a temperature-interval of only 8°. Since, however, the isotherm for 40°·3 runs entirely above that corresponding to 32°·6, the viscosity appears to increase slowly with the temperature when the pressure is constant.

II. *Liquid Carbonic Acid.*

5. Liquid carbonic acid showed far less viscosity than any other liquid hitherto examined. The friction-coefficient of water, for example, at 15° is 14·6 times that of liquid carbonic acid under the pressure of its saturated vapour. Even the appearance of liquid carbonic acid enclosed in a glass tube which is shaken excites the supposition that this substance possesses very little viscosity.

6. The viscosity of liquid carbonic acid at the temperature of 25°·1 increases with the density. By further extending this investigation, and especially to other liquids also, we purpose to ascertain the influence of temperature upon the viscosity of liquids at constant density—that is, the specific influence of temperature.

7. At densities in the vicinity of 0·8 the isotherm correspond-

ing to 25°·1 runs below both that corresponding to 32°·6 and those corresponding to 15° and 20°. From this follows that carbonic acid of such density, heated from 15° upward, must show a minimum of viscosity between 20° and 32°·6.

Poisson* has given a theory of liquid-friction which starts from the representation that, with respect to a system of simultaneous impacts, a liquid behaves, at the first moment after the expiration of them, like an isotropic solid body. Hence we can speak of the constants of instantaneous elasticity of a liquid. For the coefficient of friction in Poisson's theory we get the expression

$$\mu = \mathrm{K} \,.\, \mathrm{T},$$

where K is the coefficient of instantaneous rigidity†, and T a quantity of time which Maxwell has named the modulus of the relaxation-period. For an ideal gas, Maxwell finds‡ $\mathrm{K}=p$, and hence T, at constant temperature, proportional to the mean length of path.

In a first approximation let us assume that T still has this property when the volume of the molecules and the attraction between them is taken into account; then we get for μ a theoretical expression in which K alone remains unknown—namely

$$\mu = \mu_0 \cdot \frac{\mathrm{K}}{\mathrm{P}} \cdot \frac{s_0}{s}\left(1 - \frac{b \,.\, s}{\Delta}\right),$$

where for the temperature to which μ refers, μ_0 and s_0 denote the values of μ and s for the pressure P of one atmosphere. Δ is the normal density of carbonic acid, and b van der Waal's constant, namely four times the molecular volume, the volume of the substance at 0° and the pressure of one atmosphere being taken as the unit of volume. The equation holds so long as $s < 2b$—that is, for carbonic acid, approximately as long as $s < 0{\cdot}4$.

According to this equation, the occupation of space by the molecules produces a diminution of friction with increasing density, and consequently the opposite deviation from Maxwell's law to that produced by the attraction between the molecules. From the same equation, according to our experiments, for carbonic acid of density 0·38 at 32°·6, K comes to 7·2 kilograms upon the square millimetre—that is, about $\frac{1}{390}$ of its amount for glass, and somewhat more than for tallow§.

* *Journal de l'Ecole Polytechnique*, 1831, t. xiii. p. 139.

† In Kirchhoff's notation (*Vorlesungen*, p. 400).

‡ Phil. Mag. [4] xxxv. p. 211 (1868).

§ Pogg. *Ann.* cxxxvi. p. 295 (1869).

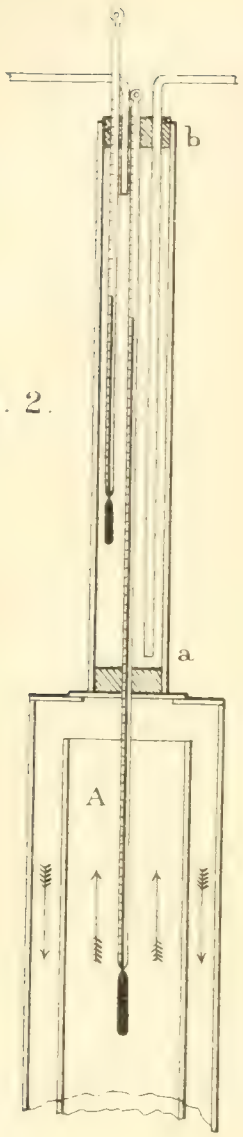


Fig. 2.

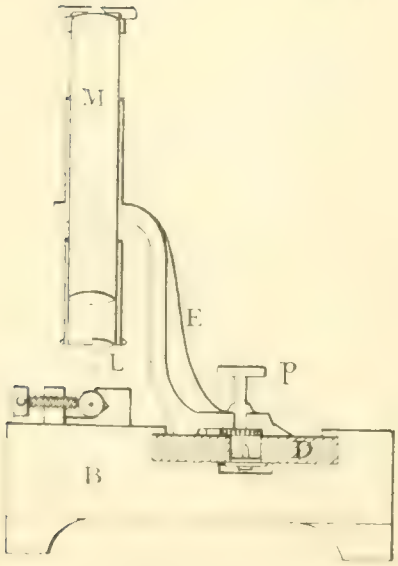


Fig. 1a.

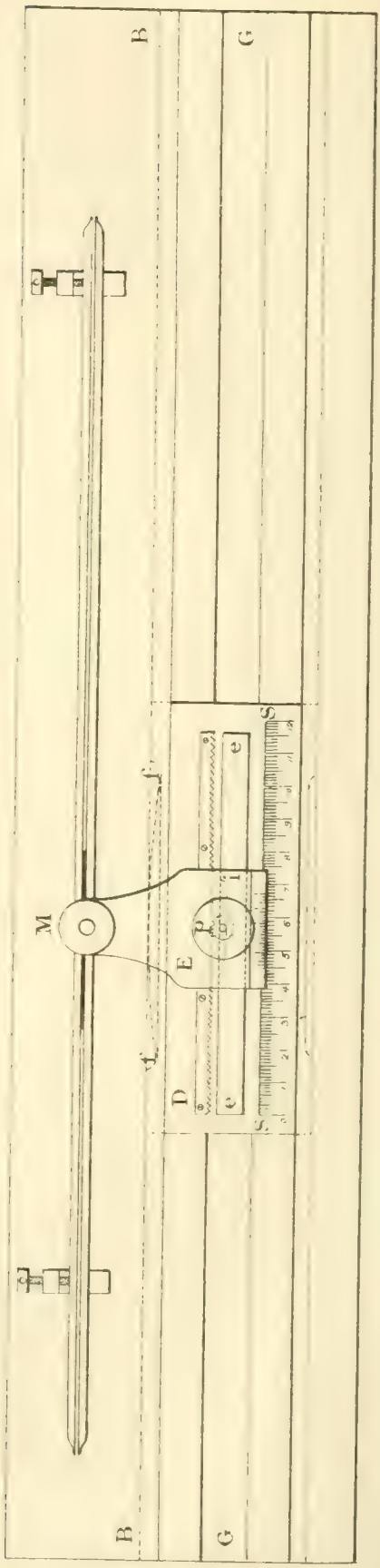


Fig. 1.

VI. *Notes on Thermometry.* *By* F. D. Brown, *B.Sc.*, *Demonstrator of Chemistry at the University Museum, Oxford**.

[Plate II.]

SOME years ago, when I determined to try and find out something about the attractive forces which the atoms and molecules seem to possess, by studying the effects of heat upon chemical substances and upon mixtures of such substances, I was led to the conviction that, if the work which I proposed to do was to be of any permanent use, I should be obliged to take many and minute precautions regarding the measurement of temperatures—a measurement which, owing to the peculiarities of mercurial and other thermometers, is so liable to error. In order to learn how best to use my thermometers, and how to refer their readings to a satisfactory standard, I made a considerable number of experiments. At the time when these experiments were made I imagined that the subject of thermometry, although presenting many difficulties to my mind, had been thoroughly worked out by others, and therefore that a printed record of my observations would be generally deemed to be of little utility. The recent publication of a paper by Dr. E. J. Mills (Edin. Roy. Soc. Trans. 1880), of one by Professors T. E. Thorpe and Rücker (Phil. Mag. [5] xii. p. 1), and more especially of a report by M. Pernet (*Mém. et Travaux du Bur. inter. des poids et mes.* i. 1881, pp. 1–52), has led me to change my opinion, and to think that there still remain many points connected with thermometers about which not only I, but others also, would be glad to have more certain information. Acting upon this belief, I have put together in the following pages some of the results of my experiments.

The Mercurial Thermometer as a Standard.

I was soon convinced that any attempt to express temperatures in degrees of an ideal absolute thermometer, or even to refer them correctly to the readings of an air-thermometer, would involve a most extensive and wearisome investigation, which would postpone indefinitely the work I wished to do. To avoid this substitution of the means for the end, I decided to construct a mercurial thermometer and to use it as a standard, keeping it until such time as the progress of our knowledge should render its comparison with the air-thermometer a matter of less difficulty.

As a mercurial thermometer is very liable to be broken, I first wanted to know whether this instrument fulfilled the primary condition of a true standard, of being capable of

* Communicated by the Physical Society.

reproduction when lost or destroyed. With this end in view, I made two thermometers at different times, and wholly independently one of the other, and compared their readings. To those who may wish at any time to construct a mercurial thermometer without the elaborate appliances ordinarily employed, but in which absolute confidence may be placed, the following details may be of interest:—

A capillary tube of medium bore, about 800 millimetres long, free from all flaws, and having as uniform a section as possible, is provided with a millimetre-scale of 600 divisions. The etching of this scale is a matter of great consequence : it very frequently happens that the divisions on glass tubes are not of exactly equal length, but that, owing to some defect in the dividing-engine or some movement of the tube while undergoing the process of division, some of the divisions are so much longer or shorter than the rest as seriously to interfere with the subsequent process of calibration. Even when all the lines are equidistant, they are often so thick, and present so irregular an outline when viewed through a telescope, that it is impossible to fix upon any particular point as that represented by the dividing-line. The tubes I employed were selected and divided with special care by Mr. Casella, the lines being perfectly straight, less than 0·4 millim. in thickness, and in all cases equidistant.

As a glass tube, however carefully selected, is never of uniform bore, it is necessary to ascertain the relative capacities of the several divisions of the tube, or, in other words, to "calibrate" it. As is well known, this is easily done by placing a thread of mercury in successive positions along the tube and observing its length, the mean capacity of the divisions occupied by the thread being, of course, inversely proportional to that length. In this way, and by adopting the plan of correcting the position of the thread suggested by Dr. Mills in the paper above referred to, which plan he had been kind enough previously to communicate to me privately, a table is readily constructed showing the volume of the tube from the line marked 0 to any line marked n, and also the value of the succeeding division. The only difficulty connected with this process is the accurate measurement of the length of the thread of mercury in its several positions. It is true that this may easily be done with a dividing-engine or some similar instrument, such as a cathetometer provided with a micrometer eyepiece and placed horizontally. As, however, reliable instruments of this class are exceedingly costly, I designed a small piece of apparatus for the purpose, which has proved so convenient and useful that I venture to describe it here.

A mahogany board, B B (Pl. II. fig. 1), about 18 inches long and 4 inches wide, is provided with a groove, G G, of the shape shown in the section (fig. 1 *a*); a piece of gun-metal, about 5 inches long and $\frac{1}{4}$ inch thick, slides in this groove with some little difficulty—the friction, which is produced by the spring *f f*, being necessary to retain the plate rigidly in any given position. The plate, D, is provided with a slot, *e e*, and a millimetre-scale, S S, the dividing lines of which must, like those of the tube to be calibrated, be very fine and truly equidistant. The piece of gun-metal, E, which is provided with a vernier, carries the reading-microscope, M, and can be moved along S S by means of the rack and pinion *p*; the movement is rendered smooth and free from lateral displacement by the spring *c*, which causes the ends of E to remain always in contact with the straight edge of the slot. The thermometer-tube is fixed with suitable screws under the path of the microscope, so that the length of a thread of mercury can be easily measured by placing the microscope so that its cross wire coincides first with one end of the thread and then with the other, and noting on the scale the distance between the two positions.

The millimetres of the brass scale and those of the tube, if marked off by different makers, will often differ a little in length; hence it is generally more satisfactory to obtain from the glass scale the number of whole divisions occupied by the thread, and to measure the terminal fractions only by the microscope.

Since the line on the outside of the tube is nearer the eye than the thread of mercury inside the tube, it is clear that when the microscope is adjusted to view the end of the thread, and is then moved along until the cross wire coincides with the nearest line, this last will be out of focus, and either the whole microscope must be raised up or the distance between the object-glass and eyepiece altered. Now, unless the instrument be constructed with great solidity, and much care be taken to fit accurately all the moving parts, this adjustment will probably alter the position of the optical axis, and so render the measurements inaccurate. To avoid this difficulty, I added a half-lens, L, fitted in the ordinary way on a brass tube sliding on the end of the microscope. This lens of course brings the focus of half the field nearer the object-glass; so that, by properly adjusting it, the divisions are seen through the half-lens at the same time that the mercury is observed through the unprotected part of the object-glass. In this way all disturbance of the microscope is avoided throughout the calibration, which is thus carried out with much greater comfort and accuracy.

Two tubes were calibrated with this apparatus, and tables of their volumes from the first division compiled; they were then furnished with bulbs, filled with mercury, and sealed up in such a manner that they formed thermometers capable of indicating temperatures between 0° and 150° C. The fixed points of the two thermometers having been determined with the precautions indicated below, tables showing the temperatures corresponding to the readings of the scale were made in the usual manner; the two instruments were then compared together, either in a large tank of water which was kept well stirred, or in the steam-apparatus which I described to the Physical Society at the time when these experiments were made. Before a series of readings were taken, both thermometers were heated for at least half an hour in steam, while their zero-points were observed after the series was completed. The numbers given in the following table show that the two thermometers gave practically identical readings. It would seem, therefore, that the mercurial thermometer, when carefully made and systematically heated, does really possess that valuable property of a standard, of being capable of exact reproduction.

Reading of AS, corrected for index-error.	Reading of BS, corrected for index-error.	Corresponding value of AS, in degrees.	Corresponding value of BS, in degrees.	Difference.
58·55	70·64	14·30	14·29	−·01
134·33	150·11	33·69	33·71	+·02
179·69	197·20	45·29	45·30	+·01
321·97	345·96	81·88	81·88	·00
23·42	33·74	5·28	5·28	·00
26·33	36·83	6·03	6·04	+·01
30·07	40·63	6·99	6·97	−·02
33·76	44·57	7·94	7·93	−·01
43·32	54·70	10·40	10·40	·00
47·98	59·60	11·59	11·59	·00
69·42	82·13	17·09	17·09	·00
91·61	105·52	22·78	22·79	+·01

Determination of the Zero-point.

In most books on physics it is stated that, in order to obtain the zero-point of a thermometer, the instrument should be placed in a vessel filled with broken ice and provided with holes at the bottom, through which the water formed by the melting of the ice may escape. In order to learn whether this method is the best possible, the following experiments were made:—A number of tin pots, about 7 inches high and 4 inches in diameter, were obtained, and holes made in the bottoms of two or three of them. A large block of ice was

broken up into small fragments, which were well mixed up, so as to render the whole perfectly uniform in character. One of the tin pots, which we will call A, was filled with some of this ice, which had been washed in a funnel with ordinary water; A was then filled up with water, so as to form a mixture in which the ice largely predominated. A second tin, B, was filled with some more of the ice, which had been washed with ordinary water in the same way; B, however, had holes at the bottom, and the water formed by the fusion of the ice thus drained away. A third tin, C, contained some of the same ice, which had been washed in a funnel with distilled water, and then mixed with distilled water in the same way as in A the ice was mixed with ordinary water. In a fourth tin, D, which was provided with holes, some ice was placed which had been washed with distilled water. Finally a quantity of distilled water was artificially frozen, the ice broken up into small pieces, washed, and mixed with distilled water in a fifth tin, E. A thermometer with a long narrow bulb, and with a stem divided into millimetres, was carefully inserted into each tin in succession, and readings taken with a cathetometer. About 17 millim. of the scale were equivalent to one degree Centigrade. In A the readings soon became constant at 1°·00; in B the readings varied considerably for about half an hour, but finally became constant at 1°·12; in C the thermometer became rapidly constant at 1°·16; in D the readings became constant after a short time at 1°·06; in E the readings did not vary after the first four or five minutes, remaining at 0°·64.

At the end of these observations, which occupied nearly two hours, the thermometer was replaced in A, where the mercury rapidly assumed the same position as before, viz. 1°·00. Seeing that, with the exception of E, the greatest difference in the readings does not amount to 0°·01 C., we may fairly draw the following conclusions:—First, that a constant temperature is more rapidly and certainly obtained with a mixture of ice and water than with ice alone; secondly, that the temperature thus obtained is really that of melting ice; thirdly, that it is preferable to wash and mix the ice with distilled water, ordinary water tending to lower the temperature, though to an insignificant extent.

With the view of seeing whether different varieties of ice gave the same results, two specimens of block ice and one of the rough thin ice collected in winter near London were obtained, while two cylinders of distilled-water ice were artificially produced. These were all broken up separately into small pieces, washed with distilled water, and then mixed with

the same in five tins, A, B, C, D, E. The thermometer placed in these tins marked 1°·30, 1°·34, 1°·26, 1°·30, and 1°·27 respectively (these numbers are not comparable with the former, as the experiments were made a month or so later, when the zero of the thermometer had altered its position). These experiments showed that distilled-water ice gave the same results as ordinary ice, and that the melting-point of different specimens of ice, when mixed with distilled water, was the same within 0°·005 C. The exceptionally low reading obtained with the tin E in the first series of experiments was probably due to the fact that the ice, having been made by means of a freezing-mixture, was not at its maximum temperature.

In subsequent determinations of the zero of thermometers I have always used ordinary block ice, washed and mixed with sufficient distilled water just to fill up the spaces between the pieces, and have not allowed the water to drain away. These results are in accord with those obtained by M. Pernet.

Zero-movements, and Substitution of the Determination of the Steam-point for that of the Zero-point.

In considering the well-worn question of the zero-movements of thermometers, it is important to distinguish between its practical and theoretical aspects. To make a study of zero-movements from an abstract point of view, to find out equations expressing these movements under different circumstances and with different thermometers, to learn that when a certain thermometer has been subjected to a certain series of temperatures at certain intervals of time its indications on next changing its temperature will be affected with a certain index-error, may possibly be of some utility, but it does not aid us much in the endeavour to free the readings of thermometers from the errors with which they are surrounded. When once we have acquired the information that a thermometer subjected only to those changes of temperature which are due to the weather exhibits a gradual rise of zero, that the rise thus taking place in a given time diminishes as the age of the thermometer increases, but differs for different thermometers, when we also know that a thermometer subjected to a high temperature after a considerable period of rest exhibits a decrease in its zero-reading, dependent on the thermometer itself and also on its previous history,—we know all, or nearly all, that we can put to practical use.

Thus, for example, the thermometer attached to my standard barometer was verified at Kew Observatory when it was first supplied to me, some four or five years ago. Since then I have from time to time observed its reading in melting ice,

and have modified accordingly the correction to be applied to it. Now, no observations of other thermometers—no curves or equations representing their zero-movements—could be of any assistance to me in this matter. I knew that the zero would probably rise, and that the amount of the rise would not be the same in my case as in that of others, and that, therefore, I must obtain the index-error experimentally. I also knew that if I boiled the thermometer I should cause irregular changes in the position of the zero; and as there was no necessity for the operation, I avoided boiling it. But if by mischance it had fallen in boiling water, no equations representing the zero-movements of other thermometers would have told me exactly what had happened to mine; I should simply have been obliged to observe its index-error more frequently than before the accident happened.

The question which seems to me to be of the greatest importance with regard to zero-movements is, how we can best reduce the trouble which they cause us. In the case of all meteorological and clinical thermometers, where the changes of temperature are small, as in the above case, it is evident that all we can or need do is to protect the instrument from unnecessary changes of temperature. When, on the contrary, our observations extend over wide ranges of temperature, the difficulties increase considerably. Suppose, for example, that I want to use a thermometer to indicate accurately a series of temperatures between 70° and 90°. It is obvious that if I observe the index-error beforehand, and apply the correction thus obtained to my readings, I shall not be doing right; for the very heating of the thermometer to 70°–90° will have altered the index-error. But if, on the other hand, I first heat the thermometer to 100°, then ascertain its index-error, then make my experiments with it, and finally observe its reading in ice a second time, I shall be tolerably certain, if the index-error is the same at the end as at the beginning of the experiment, that no variation has occurred during the observations.

In most laboratories, however, the frequent determination of the zero-point of a thermometer involves a considerable expenditure of labour: ice has to be purchased, broken up into small pieces, washed, and placed in a suitable vessel. All this requires no little time, and has, moreover, to be repeated at every determination, since the broken ice melts away in the interval. On the other hand, the apparatus for the observation of the steam-point is always in readiness; if, therefore, no greater error arises when the index-error is determined before and after the experiments by means of the steam-point, a great saving of time will be effected, without any corresponding loss of accuracy.

When the temperatures to which the thermometer is to be exposed are greater than 100°, the instrument should be heated for some time to the highest probable temperature before the steam-point is observed for the first time. In this way the lowering of the zero which takes place when a thermometer is heated from 100° to some higher temperature, to which it has not been exposed for some time previously, is effected first of all, and does not take place during the experiments, as it otherwise would.

The only objection which can be raised to this method is that, when some at least of the temperatures to be measured are below 100°, it is possible that the steam-point, which is lowered by the first heating in steam, rises again during the experiments (that is, when the thermometer is at a lower temperature), and then, by the second heating in steam, is again brought to the same position as at first. In this way the observations in steam, although concordant, would not give the true index-correction to be applied to the readings. That the error which thus arises is of no importance is, I think, rendered probable by the following considerations:— The gradual rise of the zero of a thermometer receives its most natural explanation when it is supposed that the glass bulb, after having been heated and somewhat quickly cooled, is in a state of strain which causes it to have a larger capacity than it would have if no such strain existed. As time goes on, and more especially as the thermometer is subjected to small fluctuations of temperature, the particles of the glass gradually yield to the forces which are acting upon them, and take up new and more suitable positions. These molecular movements result in a gradual diminution of the capacity of the bulb, and consequently in a rise of the zero. Now it is evident that, if a certain state of strain is set up when a thermometer is cooled from 100° to 0°, when it is cooled from 100° to some intermediate temperature t the strain set up will be less considerable; there will therefore be a greater tendency for the zero to rise when the thermometer is placed in melting ice than when it is subjected to the temperature t. Consequently, if it be found that, when a thermometer after being heated in steam is placed in ice, no change of the zero takes place for three or four hours afterwards, we may legitimately conclude that, if the thermometer were maintained for the same time at the temperature t, no movement of the zero would occur. I have frequently kept recently-heated thermometers in melting ice for several hours, renewing the ice when necessary; and I have always observed, with all of my instruments, that no change took place for the first three hours, and that

during the next two or three hours the rise was extremely small. It follows, therefore, that if in any series of observations lasting more than three hours the thermometer be heated in steam at the end of every third hour, there will be no uncertainty as to the position of the zero; that if the experiments be carried on continuously for six hours, a slight rise of the zero may occur during the last part of the time, but that this rise will not amount to more than one or two hundredths of a degree.

Correction for the Exposed Portion of the Thread.

When a thermometer is only partially immersed in the medium of which the temperature is to be observed, the readings become subject to an error which arises from the fact that a part of the thread of mercury, together with the corresponding portion of the stem, are at a temperature different from that of the bulb and immersed portion of the stem. The correction, C, usually applied in this case is given by the formula

$$C = m(T-t)N, \quad \ldots\ldots \quad (1)$$

where T = the reading of the thermometer,
t = the temperature of the exposed portion,
N = the number of exposed divisions of the stem which are filled with mercury,
m = the apparent expansion of mercury in glass.

This formula is founded on the assumption that the error in the reading has no other cause than the comparatively unexpanded condition of a portion of the thread and stem.

The apparent expansion of mercury in glass, as obtained from Regnault's experiments, is about ·0001545; but it differs, of course, for different specimens of glass. When this number is employed in the above formula, the values of C obtained are generally believed to be too large; indeed a little reflection will convince us that this must be the case whenever the temperature of the exposed portion is merely measured by placing another thermometer with its bulb halfway up it. This second thermometer evidently measures the temperature of the ascending stream of warm air around the stem; if the stem of the chief thermometer were subjected to the heating influence of this stream, and to no other, its temperature would be rightly given by the subsidiary thermometer; but the thermal conduction along the thread of mercury and along the glass stem must necessarily raise the lower part of the

exposed stem to a temperature higher than that indicated by the subsidiary thermometer. The value of $(T-t)$ therefore is too great, and consequently also that of C.

In order to meet this difficulty, Dr. Mills, instead of endeavouring to give to $(T-t)$ its proper value, has made a large number of experiments with different thermometers with a view to assign a more satisfactory value to m, and has thus been led to draw the following conclusions:—The value ·0001545 of the coefficient m is invariably too great. This coefficient varies with the thermometer employed, and also with the number of divisions of the thread exposed; so that, instead of assigning one definite value to m for each thermometer, we must give it a value

$$m=\alpha+\beta N,$$

where α and β must be determined for each thermometer.

Professors Thorpe and Rücker, on the other hand, while admitting that the value $m=$·0001545 may be generally too large, maintain that it is sufficient to replace it by some other single number, and that the employment of the varying coefficients $\alpha+\beta N$ is unnecessary; they support this opinion by showing that in Dr. Mills's own experiments the alterations in the value of C, caused by the introduction of the term βN, do not amount to more than one or two hundredths of a degree, and are therefore insignificant. Dr. Mills, replying to this, states that the change in the correction C brought about by the term βN often amounts to so many hundredths of a degree that it cannot be neglected.

Now it is clear that by merely placing a second thermometer halfway up the exposed thread, only the roughest idea is obtained of the real temperature of the thread. Suppose, for example, that $T=100°$, and that t is taken at 15°, being subject to an error of 5°: the value of $(T-t)$, which is 85, will be subject to an error of 5°, or about 6 per cent. What, therefore, can be the use of attempting to determine the coefficient β, of which the value would appear ordinarily to be about 0·0000002, when so great a source of error is left unprovided for?

In all experiments in which I have had occasion to use mercurial thermometers, I have endeavoured to avoid any correction for the exposed thread, by making the apparatus and thermometers employed of such relative dimensions that the whole thread and bulb, except the topmost division, are at the same temperature. When this is impossible, and when the experiments require such extreme accuracy, it seems to me that the first thing to be done is to surround the exposed portion of

the thread with a current of running water, and so, while preserving it from the uncertain effects of conduction, radiation, &c., to render possible the observation of its exact temperature. The value of $(T-t)$ being thus correctly measured, that of m is found to be constant for all values of N, and to differ but little from 0·001545. It varies, however, with different thermometers.

The following experiments show most distinctly the truth of this statement:—

One of the standard thermometers mentioned in the first section of this communication was partially surrounded by a glass tube, *a b* (fig. 2), about an inch in diameter; this tube was closed at the bottom with a piece of good cork, about 8 millim. thick, through which the stem of the thermometer passed. The upper end of the tube *a b* was fitted with a cork, in which were four holes—one for the stem of the chief thermometer, a second for a thermometer to indicate the temperature of the water contained in the tube, while through the two others passed the tubes by means of which the current of water was maintained. The thermometer thus furnished was fixed vertically in the ordinary apparatus, A, for determining the 100°-point of thermometers. The open end of A was closed with a thin disk of brass, with a small central hole, through which the thermometer passed. One degree was equal to about four divisions of the millimetre-scale of the thermometer, the readings of which were observed with a cathetometer, and the fractions of a division measured with that instrument. It was found that the readings of the thermometer under these conditions were correct to ·02 of a millimetre, or ·005 of a degree. The numbers given below are the means of three readings, which, however, were nearly always identical. The thermometer in the water was graduated to fifths of a degree, and had been compared with the standard.

The chief thermometer was first heated in the steam for an hour, with two or three inches of the thread above the cork; it was then pushed down until the quicksilver was only just visible above the cork, and the reading noted; it was then pulled up again, and readings taken in various positions, as given in the following table; finally the thermometer was again pushed down as far as possible, and the reading taken, when it was found to be the same as before, showing that no change in the 100°-point had supervened during the experiment. Of several series of observations made in this manner, the one contained in the following table will suffice, since they all led to precisely the same result.

Reading of Standard when wholly immersed = 393·42.
Barometric pressure, corrected and reduced, = 760·1.
Corresponding temperature of steam = 100°·00.

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard, T.	Value of C = 393·42 − T.	Value of m, $= \frac{C}{(T-t)N}$.
317	13°·0	389·01	4·41	·0001599
277·5	12·3	389·54	3·88	·0001594
221	12·0	390·30	3·12	·0001604
173	12·1	390·94	2·48	·0001631
130	12·1	391·58	1·84	·0001610
79	12·1	392·30	1·12	·0001612

An inspection of the above table is sufficient to convince us that the value of m is constant, and equal to the apparent expansion of mercury in the glass of which the thermometer was made; the numbers would probably have agreed even more closely, were it not that it is impossible to arrange the apparatus so that the cold portion of the thermometer-stem follows directly upon the hot portion. There must always be an interval occupied by the cork, the temperature of which is uncertain. It should be remarked that there is no indication whatever of the value of m increasing when that of N increases.

Precisely the same results were obtained with the second standard thermometer, as is shown by the following table:—

Reading of Standard BS, when wholly immersed, = 419·21.
Barometric pressure, corrected and reduced, = 760·5.
Corresponding temperature of steam = 100°·02.

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard BS, = T.	Value of C, = 419·21 − T.	Value of m, $= \frac{C}{(T-t)N}$.
302	12°·0	415·13	4·08	·0001535
237	11·9	415·96	3·25	·0001556
174	12·0	416·84	2·37	·0001548
127	12·0	417·47	1·74	·0001557

Here, again, the value of m varies only within the limits of

the error of observation, and shows no tendency to increase when N increases. It may be noted that with both the above thermometers the mean value of m is *greater* than ·0001545, the value usually assigned to it, but that it differs from that number by so little that the error committed by substituting the one for the other in the calculation of the correction C will rarely amount to more than 0°·02 C.

The above experiments were made at 100°, because this is the only temperature which can be maintained absolutely constant for an hour without the use of a quantity of complicated apparatus; and it is evident that the slightest variation in the temperature would entirely spoil the series of observations. At higher temperatures the sources of error which beset the readings of thermometers increase so rapidly that the exact value of the coefficient m becomes of less and less importance as the temperature rises, notwithstanding the fact that the correction C increases in amount. Since there is no reason whatever to suppose that any different results would be obtained at such higher temperatures, I thought it unnecessary to make any further experiments, more especially as those given above yielded precisely those numbers which the ordinary laws of expansion predicted.

There is another point connected with thermometry, to which I devoted attention some years ago. It has been suggested that when a thermometer is placed in a vapour at maximum tension, as in the ordinary chemical process of distillation, it does not truly indicate the temperature of the vapour. This suggestion owes its origin to the fact that drops are seen to accumulate and drop off the end of the thermometer. It has been supposed that this condensation of the vapour on a surface which should be as hot itself, is due to the molecular attraction of the glass for the vapour. If this be the case, the heat evolved by the vapour during liquefaction on the thermometer-bulb would raise the temperature of the latter. The thermometer would thus indicate a higher temperature than that of the mass of the vapour. The experiments which I made upon this subject, like those instituted by others, were inconclusive. I possess, however, an apparatus which seems to me eminently suited to answer the question satisfactorily. It is at present being employed for other purposes; but I trust that, when it is at liberty, I shall be able to put it to this not unimportant use.

VII. *Notices respecting New Books.*

Geological and Natural-History Survey of Canada. By Alfred R. C. Selwyn, *LL.D., F.R.S., Director. Report of Progress for* 1879–80. Montreal: Dawson Brothers, 1881.

THE Geological Survey of Canada, under the direction of Dr. Selwyn, appears to make steady progress. This volume refers mainly to the work of the Geological Corps during the season 1879–80, which embraced further explorations in parts of the North-west Territory (the Souris-River Coalfield), Hudson's-Bay basin, Quebec, New Brunswick, Nova Scotia, and the Magdalene Islands, as well as the results of the survey in 1879 by Dr. G. M. Dawson of a portion of the northern part of British Columbia, and of the Peace-River country, which will be found to embody the best and most reliable information on this vast and interesting region.

The other Reports comprise many valuable details bearing on the physical features, geological structure, climatal conditions, soils, and economical minerals of the different provinces explored, of which that on Hudson's Bay by Dr. Bell (in continuation of the survey of the two previous years) is very interesting; for perhaps comparatively few people have any adequate conception of the extent of this great Canadian sea, which is the central basin of the drainage of North America. Geologically this basin, excluding the Winnipeg division (the rocks of which range from the Laurentian to the Tertiary), lies within the great Laurentian area of the Dominion; resting upon these, the Cambro-Silurian rocks form an irregular border along the south-western side of the bay; while to the south and west of James' Bay these latter are overlain by Devonian rocks, which occupy a considerable area. The chains of islands which fringe the east coast to the northward of Cape Jones, and also the mainland near Richmond Gulf, are composed of bedded volcanic and unaltered sedimentary rocks, which may be of Lower-Cambrian age; on the western side the quartzites and other rocks, rather largely developed, probably belong to the Cambrian system. The Reporter considers that few of the varied and numerous resources of Hudson's Bay are at all developed, that the fur-trade and oil are the principal; but that the most important of the undeveloped resources are the soil, timber, and minerals, and that the latter may become in future the greatest of the resources of the shores of Hudson's Bay. The concluding part contains a very suggestive paper (with a map) on the northern limits of the principal Forest-trees of Canada. In the Report of British Columbia, Dr. Dawson also gives an account of the distribution of the more important forest-trees of that province.

The geological structure of Northern New Brunswick and Eastern Nova Scotia is fully described in the respective Reports. The Lignite Tertiary formation in the Souris valley (North-west Territory) is reported on by Dr. Selwyn, with appendices on the nature of the strata and their plant-remains by Dr. G. M. and

Principal Dawson. In the western prairie-region the true Carboniferous system is not the coal-bearing formation; and although lignites and coals are known at several different stages of the Cretaceous rocks, it is in the representative of the Fort-Union rocks of the U.-S. geologists that the most extensive and numerous beds of lignite of the Souris-River region occur, and constitute the nearest available supply for the province of Manitoba. "The flora of the Great Lignite Tertiary series of the North-west, though undoubtedly similar to the Miocene of Europe, really characterizes the beds which in the West constitute the transition from the Cretaceous to the Tertiary, and which form one great continuous series, probably on the horizon of the Eocene of Europe, though with local differences which are liable to be mistaken for differences of age" (p. 55 A.)

With regard to the origin of certain granites, Dr. Selwyn (p. 5), in describing the granites along Maine and New-Hampshire boundaries, says that there is absolutely no proof that these later granites are "intrusive," as so designated by Sir W. Logan, but that, like those in Australia and Nova Scotia, all the phenomena connected with them may be more readily explained and understood if we regard them as completely metamorphosed portions of the strata which now surround them; whereas in regions where the granite is older than the adjacent strata similar contact-lines may be seen, but without any change in the mineralogical character of the latter such as occurs when the crystalline rock is the youngest (p. 6).

Although the subjects of these Reports are locally important, they will doubtless be of interest to those who may wish to become acquainted with the physical conditions, geological structure, and economical resources of the Dominion of Canada.

Geological Sketches at Home and Abroad. By A. Geikie, *LL.D., F.R.S., Director-General of the Geological Surveys of the United Kingdom.* With Illustrations. London: Macmillan and Co. 1882.

This work consists of a series of essays previously published in various journals during the last twenty years. Most of them relate to certain districts in this country, Europe, and America, where the striking geological phenomena here recorded were observed by the author. Among the more important subjects noticed in these papers are those having more or less reference to Denudation, Glacial action, Volcanic phenomena, and Rock-weathering. An Erosionist of the advanced school, but by no means inclined to do battle under the extreme "quietest" banners of some of its champions, Dr. Geikie has been led in his wanderings to look at scenery with peculiar interest; and thus, in the essay on the Old Glaciers of Norway and Scotland, it is shown that the Norwegian and Scottish Highlands seem to be but parts of one long tableland of erosion composed of older and chiefly metamorphic rocks, while the fjords and valleys of the one country and the lochs and glens of the other owe their excavation to the great process of denudation

which has brought the land to its present form. The various forms of rock-weathering are discussed as derived from the study of tombstones (Essay viii.); and the effects of modern atmospheric action, conjoined with the bedding and jointing of rocks, are shown in the isolated pillar of Old Red Sandstone, 600 feet high, standing out from the mainland of which it once formed a part, and known as the "Old Man of Hoy" (No. I.).

The power of rain- and river-action is fully shown in the deep gorges excavated through the basalt and other rocks of the Auvergne and Haute-Loire of Central France (No. V.); but great as has been the efficiency of superficial erosion on the development of the terrestrial surface of Europe, the author considers that the fundamental laws of denudation can nowhere be better learnt than in the western region of the Rocky Mountains, the evolution of the mountain-forms of the Uintah range, the high plateaux of Utah, and the great basin of the Colorado, where "the proofs of enormous superficial waste rise to such a gigantic scale as wholly to baffle every observer who has yet attempted to describe them" (p. 229).

In the essays on the Volcanoes of Central France (No. V.), the Yellowstone Geysers (No. X.), and the Lava-fields of North-west Europe (No. XI.), are many interesting and suggestive remarks on volcanic phenomena. The study of the former seemed to throw light on the character and aspect of the Carboniferous volcanos of Central Scotland (p. 102). Again, there were features of former volcanic action on which the phenomena of modern volcanos appeared to afford but little light; "in particular, the vast number of fissures which in Britain had been filled with basalt and now formed the well-known and abundant 'dykes,' appeared hardly to connect themselves with any known phase of volcanism" (p. 276). This has been accounted for by the emission of vast floods of lava, "massive eruptions," without the formation of cones or craters—a view advocated by Richthofen more than twelve years ago,—and that our modern volcanos, Vesuvius and Etna, present us by no means with the grandest type of volcanic action. Dr. Geikie, after his visit to the lava-fields of the Pacific slope, was enabled to realize the conditions of volcanism described by Richthofen, and thus assist in solving a difficulty he had long felt in accounting for the extent of the dykes and other protrusions of basalt, "which can be traced over an area of probably not less than 100,000 square miles in Britain; for they occur from Yorkshire to Orkney, and from Donegal to the mouth of the Tay" (p. 276), which was only part of the far more extensive region that included the Faröe Islands and Iceland.

These stupendous outpourings of lava in the west of Scotland, like those on the plains of Idaho, are considered to be due to the fissure or "massive" type of eruption; and that the basaltic plateaux of Abyssinia and the "Deccan traps" of India probably mark the sites of some of the great fissure-eruptions which have produced the lava-fields of the Old World (p. 285).

Although Dr. Geikie advocates the fissure-eruption theory in explanation of certain volcanic phenomena, it must be remembered that Mr. Scrope, in his energetic review of the Natural History of Volcanic Rocks, strongly opposed Richthofen's twofold division of volcanic rocks into "massive eruptions" and the products of "volcanos proper," and remarked:—"It is utterly impossible to find in the writings of its advocates or in nature any intelligible distinction between volcanic rocks that have issued from fissures, so as to form 'massive' or 'elongated' or 'dome-shaped' mountains, and rocks produced by eruptions from 'volcanos proper'" (Geol. Mag. vol. vi. p. 512).

The last three chapters include lectures on the Scottish school of geology, Geographical evolution, and on the Geological influences which have affected the course of British history.

These well-written essays, now collected and revised, fully convey in a clear and pleasant manner the vivid impressions made on the author during his geologic wanderings, and are replete with scientific facts, occasionally interspersed with notes and illustrations of the striking features of the scenery or of historic and legendary interest.

VIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from Vol. xiii. p. 375.]

April 26, 1882.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On Fossil Chilostomatous Bryozoa from Mount Gambier, South Australia." By Arthur W. Waters, Esq., F.L.S., F.G.S.

2. "*Thamniscus*: Permian, Carboniferous, and Silurian." By George W. Shrubsole, Esq, F.G.S.

3. "On the Occurrence of a New Species of *Phyllopora* in the Permian Limestones." By George W. Shrubsole, Esq., F.G.S.

4. "On the Relations of the Eocene and Oligocene Strata in the Hampshire Basin." By Prof. John W. Judd, F.R.S., Sec.G.S.

The section at Whitecliff Bay, in the Isle of Wight, affords us the means of determining the true order of succession of nearly 2000 feet of Tertiary strata, and is therefore employed as a standard to which to refer the strata seen in sections where the order of succession is not so clear. The author supported the views of Prof. Prestwich as to the limits of the Bracklesham series, as opposed to the opinions expressed on the subject by the Rev. O. Fisher. He pointed out the confusion which has arisen from the correlation of certain strata in the Hampshire basin with the barren Lower and Upper Bagshots of the London area, in which fossils are so rare as to render their geological age somewhat doubtful. To the Lower Bagshot some authors have referred 660 feet of the strata seen at

Alum Bay; while other authors have restricted that name to about 73 feet of the same section. The age of the Upper Bagshot of the London basin is admitted by all authors to be very doubtful. The only way to avoid the confusion unavoidable from using the same names for strata the correlation of which was so hypothetical, was to employ local names for both sets of beds. He proposed to refer to the freshwater sands below the Bracklesham and Bournemouth strata, containing a distinctive flora, as "the Studland beds," and the sands above the Barton clay by the old name of "the Headon-Hill Sands."

Above these sands are a series of clays only about 40 feet thick at Whitecliff Bay, but much thicker at Headon Hill and Hordwell Cliff. These sands and clays form the Headon group: they consist of freshwater strata with bands of limestone and lignite, but including numerous inconstant intercalations of layers containing marine shells, for the most part much dwarfed. The age of the Headon group, as shown by the fossils which it contains, is that of "the zone of *Cerithium concavum*" of continental authors.

The brackish-water Headon group is succeeded at Whitecliff Bay by nearly 100 feet of purely marine strata. These marine beds, which had been shown to rest on an eroded surface of the Headon beds, contain the remarkable fauna which had been recognized by many British and foreign geologists as that of the Lower Oligocene. Similar strata with the same fossils are found in the New Forest, at Lyndhurst, Brockenhurst, Roydon, and other points, and there also attain a considerable thickness. It was pointed out that this marine series is quite distinct from the Headon, or zone of *Cerithium concavum*, with which it had been confounded.

The author had been very severely criticised for the views which he had put forward in a former paper as to the manner in which the Brockenhurst series is represented in the section at the west end of the Isle of Wight. There was much difficulty in these variable estuarine beds in correlating the beds seen in Colwell Bay with those exposed in the cliffs of Headon Hill. With several previous authors on the subject, he maintained that the great series of sandstones and limestones forming Warden Point and How Ledge are continuous with those exposed in the face of Headon Hill, and, consequently, that the marine beds of Colwell Bay overlying these limestone series are younger than the brackish-water bands interstratified with the Heddon beds of Headon Hill. His critics, however, insisted that these two beds agreed with one another in such a manner that they must be regarded as parts of the same bed, separated by denudation. In opposition to this view it was pointed out that the Colwell-Bay bed is of the most inconstant character, and long before reaching Headon Hill is seen to be on the point of thinning out and disappearing altogether.

In conclusion, the author pointed out that his own interpretation of the succession and correlation of the strata in the Hampshire basin brings them into complete harmony with that which is maintained by the great majority of continental geologists, while that of his critics appeared hopelessly irreconcilable with their views.

IX. *Intelligence and Miscellaneous Articles.*

SCIENCE AND METAPHYSIC.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WILL you allow me space to say a few words upon some points which are raised and discussed in your review of Prof. Max Müller's translation of Kant's 'Critique of Pure Reason.' The first question of importance, namely that of the meaning of metaphysic and its relation to science, is an extremely interesting one, and, as it seems to me, well worthy of discussion from a scientific standpoint. One side of the question is put forward in your review; and it is the other side which I wish briefly to state here, with the object of eliciting discussion.

Metaphysic, as now understood, and Science are not words which are opposed as regards meaning. The subject-matter of science and of metaphysic are distinct: they run in different, yet closely connected channels. And perhaps here, at the outset, I may be permitted to remark that, in any sense in which the word metaphysic is or has been held, it is inadmissible to instance Auguste Comte as a metaphysician, even if his own reiterated statements upon the question did not forbid. Any one who has read the *Philosophie Positive* will know how Comte never lets an opportunity pass without ridiculing both metaphysic and metaphysician, in the old sense of the words. He regarded the metaphysical method of looking at things as a stage through which the human mind has to pass before it reaches the final stage, which he called the positive or scientific. In fact, one of the main objects of his great work was to free science from metaphysic as he understood it. When Comte censured as useless the study of the fixed stars with the object of discovering their chemical composition, he was arguing upon grounds that were of the most commonplace and superficial kind, and could not by any means be shown to be even remotely connected with metaphysic. I am unable to agree, except in a very limited sense, with Prof. Max Müller in thinking that there is any connexion between the philosophical systems of Kant and Comte. Metaphysic, until quite recent times, has been mainly an attempt to discover the supposed hidden causes of phenomena: a mode of inquiry which had its systematic origin in Aristotle, and was extended and formulated by the Schoolmen. In science the same tendency was manifest. Abstract entities were assumed as originating causes; and it is only recently that science has given up the search for these causes as futile, and sought to show how phenomena take place instead of why they take place. Here was a change of method in science; and what I wish to emphasize is that a similar change has occurred in metaphysic. Metaphysic has adopted the new method. While formerly the metaphysician endeavoured to discover what the facts of nature were in their hidden essence, he now seeks to find out what these facts are known as or appear

to be; in fact his whole purpose is to arrive at a complete analysis of the contents of the mind. The constant question in metaphysic is, What do we *mean* by such words as Time, Space, Cause, Necessity, Power, and other facts which are taken as ultimates by Science? It does not aim at discovering the cause of these facts, but simply at studying their relations. The introduction of this method is mainly due to Kant; and it is on this account that his claim to so high a place in the history of philosophy rests, although his method still retained a large admixture of the old conception. This new method of philosophizing without the assumption of entities, which we may call the New Metaphysic, will be understood by reference to such works as Mr. Shadworth Hodgson's 'Philosophy of Reflection,' or M. Renouvier's *Essais de Critique Générale*. What may be the merits of the systems propounded in these books, taken as a whole, I do not here discuss; but of the former of them, at least, it may be said that there is no conclusion touching scientific questions which Science, as such, would hesitate to indorse.

Yours truly,

WYNDHAM R. DUNSTAN.

ON THE DEPRESSION OF THE ZERO-POINT IN MERCURIAL THERMOMETERS. BY J.-M. CRAFTS.

It is well known that a thermometer which has remained for a long time at the ordinary temperature presents when it is heated a depression of its zero-point. The amount of the depression depends on the temperature to which the thermometer is raised; and M. Pernet has demonstrated that the depressions are proportional to the squares of the temperatures when it is heated from 0° to about 100°. For higher temperatures Mr. Mills has found a very different course. He does not indicate all the details which would be necessary to enable one to usefully criticise his data; and I will confine myself to saying that the numbers he gives resemble those obtained with thermometers which have not undergone a suitable preparation, and that the method of experimenting which I desire to present to the appreciation of the Academy leads to very different results.

Before measuring the depressions produced by a reheating, it is necessary to make sure of two things:—(1) that the starting-point is the zero raised to the maximum; (2) that the phenomenon one wishes to observe is not masked by the permanent elevation of the zero, which may take place at the same time as the depression. I have ascertained that this last effect is invariably produced when one heats even for a few minutes a thermometer which has not been previously heated for a long time to a temperature near that of the experiment. This movement can be effectually suppressed by preliminary processes of heating which will be described further on; and each series of depressions and reelevations must be repeated, in order to make sure that no permanent displacement of the zero has taken place. Let us suppose this preparation completed: the ther-

mometer has been heated to the highest degree of temperature employed in the experiments, 355° for instance; the position of the zero has been observed, the thermometer submitted to any series of operations; and, on heating again to 355°, the original position of the zero has been recovered; it remains to raise this depressed zero to its highest position. A thermometer can be kept indefinitely at the ordinary temperature, or even be heated for several weeks to 100°, without producing the total reelevation of the zero; and the study in detail of the means which facilitate that movement has revealed new facts of some importance in regard to the theoretic views considered in some preceding communications.

I will briefly repeat the explanation which seems to account best for all the movements of the fixed points of a thermometer. The glass, softened during the process of blowing, retains indefinitely, at the ordinary temperature, a residual separation of its particles, similar to that observed in an exaggerated degree in Rupert's drops. On such glass being heated, greater mobility is imparted to its particles, and the normal shrinking is induced (the disappearance of the abnormal expansion); and this action of heat is the more pronounced the more nearly the temperature is approached which produced the original separation. When a thermometer is in operation, the shrinkage causes a permanent raising of the zero-point, which may reach the considerable figure of 26°. It is evident that if a separation of the same kind, although inferior in amplitude, can be produced at will, can be made to persist for an indefinite time at the ordinary temperature, and to disappear by heating, this manner of understanding the phenomenon will be notably confirmed. Experiment realizes this prevision. In fact it is sufficient to know the true depression of the zero produced by boiling the mercury in a thermometer to ascertain that it persists indefinitely at the ordinary temperature. The depression is about 2° for French glass containing oxide of lead, the reelevation at the end of the first six months is from 0°·4 to 0°·5; the subsequent elevation during from five to ten years rarely exceeds 0°·5; and after ten years the zero remains stationary, leaving half of the depression subsisting, of which the total removal, equalling an additional degree, can be effected by suitable means. This conclusion has been confirmed by numerous experiments on the depressions produced at different temperatures. It is especially by heating to very high temperatures that depressions are produced which do not disappear. This phenomenon resembles that of the permanent expansion of a body when the limit of its elasticity has been passed.

The total reelevation after a depression of the zero, is accomplished more rapidly at high temperatures. Let us compare the periods of time when the intervals of temperature remain nearly equal. Five thermometers, after being heated for 24 hours to 306°, were kept at 218° until the position of the zero had become constant; for which four days were required. Afterwards the zero depressed at 218° was reelevated by heating for 18 days to 100°. It requires from six months to two years for the total

reelevation of the zero depressed at 100° when the thermometer is left at the ordinary temperature. The termination of the reelevation being the limit of the abatement of a movement, it is evident that it cannot be stated with exact precision; and it is especially by comparing the course at its different phases for each interval that one gets an exact idea of the variation of rapidity of the reelevation according to the temperature at which one works. When the intervals are smaller the reelevation is still more rapid: thus, from 0·8 to 0·9 of the total reelevation of the zero depressed at 100° can be produced by heating the thermometer during three days to 80°, four days to 60°, and five days to 40°.

It will be seen that, the greater the interval between the temperature which has produced a depression and that at which the thermometer is maintained to accomplish the reelevation, the slower is the movement: and it may not be complete if the interval notably exceeds 100°. These data have been utilized in the following manner to prepare some thermometers for the study of the real depressions of the zero. In order to cause the abnormal tension produced during the blowing of the glass to disappear, the instruments nos. 1 and 5 were heated 11 days to 355°; nos. 13 and 15 were heated three years to 206° and 266°; nos. 31 and 32, before being filled with mercury, were heated 100 hours to 440°, and cooled as gradually as possible during 100 hours; and finally all were heated for one day to 306° and the zero-points observed. In order to reelevate completely the zero from its position depressed by the heating to 306°, all the thermometers were heated and kept for four days at 218°, eighteen days at 100°, five days at 80°, seven days at 60°, six days at 40°, nine days at 20°, three days at 10°, and two days at zero.

Here only one series of experiments can be cited: they are summed up in the following Table: the first column contains the numbers of the thermometers; those which follow give the depressions corresponding to the temperatures indicated at the top of the columns.

	40°	60°	80°	100°	160°	218°	260°	306°	355°
1.	0·00	0·06	0·19	0·31	0·74	1·12	1·33	1·63	2·19
5.	0·04	0·08	0·18	0·29	0·56	0·76	0·91	1·14	1·51
13.	0·02	0·03	0·17	0·31	0·69	0·87	1·09	1·30	2·15
15.	0·01	0·05	0·18	0·31	0·75	0·97	1·12	1·40	2·05
31.	0·02	0·06	0·22	0·37	0·84	1·15	1·46	1·77	..
32.	..	..	..	0·28	0·69	0·98	1·21	1·56	2·06

Neglecting thermometer no. 5, which is of German soda glass, the numbers for all the others, which are of glass containing oxide of lead, are sufficiently concordant to permit the table to be employed for estimating, with but a very slight error, the depressions which will be produced by heating thermometers manufactured at Paris. The relation between the temperatures and the depressions might be expressed by a general formula: but simple interpolation suffices for practical needs.—*Comptes Rendus de l'Académie des Sciences*, May 8, 1882, t. xciv. pp. 1298–1301.

ON THE OSCILLATIONS OF THE PLANE OF POLARIZATION PRODUCED BY THE DISCHARGE OF A BATTERY: SIMULTANEITY OF THE ELECTRICAL AND OPTICAL PHENOMENA. BY D. BICHAT AND R. BLONDLOT.

We proposed to ourselves to study the rotation of the plane of polarization in a transparent body under the action of the current from the discharge of a Leyden jar. The experiment was arranged as follows.

Between a polarizer and an analyzer at extinction the transparent body is placed in a bobbin with a long and fine wire which is connected with the armatures of a battery. An exciter intercalated in the circuit permits the discharge to be produced when the difference of potential is sufficient. At the instant of each discharge the eye, placed in front of the analyzer, perceives a vivid reappearance of light, which shows that the plane of polarization has been deflected.

This fact having been proved, we sought to analyze it. For that purpose we placed before the optical apparatus a mirror rotating about a vertical axis. The polarizer was furnished with a slit, likewise vertical, the image of which was observed in the mirror by means of a telescope. By a suitable arrangement we compelled the spark to burst forth at the moment when the mirror, in its continuous rotation, occupied such a position that the image of the slit was visible in the telescope. We thus saw in the rotating mirror the reappearance of light due to each discharge.

What we observed was this:—In general, in the field of the telescope a series of broad luminous bands are seen, separated by narrower dark bands. The appearance reminds one absolutely of that which is observed when the light of the spark is examined. It is known that in that case the successive luminous bands correspond to currents alternately in opposite directions: the discharge is *oscillatory*. We have ascertained that it is the same with the plane of polarization. In fact, if the analyzer be rotated a small angle in a certain direction, the images of the even order are seen to be weakened, and at the same time the images of the odd order to increase in brightness. If the rotation take place in the opposite direction, the images of the odd order are weakened, and those of the even order become brighter.

The plane of polarization, then, undergoes successive rotations alternately in opposite directions: it *oscillates* about its normal position. To each oscillatory discharge corresponds an oscillatory movement of the plane of polarization.

This being admitted, is there simultaneity between the electric and optic phenomena? or does the movement of the plane of polarization manifest itself in an appreciable time after the electric action? We have solved this question in the following manner:—To the apparatus employed for the preceding experiments we added an arrangement permitting to be seen at the same time, in the rotating mirror, the bands furnished by the light of the spark and those due to the oscillation of the plane of polarization. For this

purpose the exciter was placed so that the spark, by means of a suitable optical system, illuminated a vertical slit. A fixed mirror sent back the light proceeding from this slit upon the rotating mirror, and thence into the telescope. The movable mirror being at rest, the vertical images of the two slits were seen in the field distinctly. By regulating the position of the fixed mirror, these two slits, which had the same breadth, were brought to be each precisely in the prolongation of the other.

During the rotation of the mirror, at the moment when the battery is discharged, each of the images dilates in the horizontal direction. Two systems of bands alternately luminous and dark are thus seen one above the other: the one is due to the light of the spark; the other proceeds from the polarizing-apparatus.

The experiment shows that the brilliant bands of one of the systems forms exactly the prolongation of the luminous bands of the other, and that it is the same with the dark bands. If the mirror be rotated more and more rapidly, the breadth of the bands increases, *but the correspondence of the two systems still remains perfect.* Therefore, with the close approximation which our apparatus permits us to obtain, we may conclude that *the two phenomena, electrical and optical, are simultaneous.*

In order to measure that approximation, we slightly displaced the fixed mirror, so as to destroy the correspondence of the two images. It is clear that the displacement produced the same effect as any delay that might have existed between the two orders of phenomena. We thus secured that a delay of $\frac{1}{30000}$ of a second should be quite appreciable; we can therefore affirm that the delay, if it exists, is less than $\frac{1}{30000}$ of a second.

The experiments were made with heavy flint glass and bisulphide of carbon successively as the transparent body. M. Villari, by causing a cylinder of flint glass to rotate between the poles of an electromagnet*, ascertained that at a sufficient velocity the phenomenon of rotatory polarization ceases to exist; hence he concluded that, to produce magnetization of the flint glass, a time comprised between 0·001244 and 0·00241 of a second is required. Now the sensitiveness of our method permitted us to appreciate a displacement corresponding to a forty-fourth part of that time.

An unpublished experiment of MM. P. Curie and Ledeboer accords with our conclusions. Substituting for the copper disk of Foucault's apparatus a glass disk, and causing it to rotate at the rate of a hundred turns per second, they observed no diminution in the rotation of the plane of polarization. It seems, then, that another explanation of the very interesting experiment of M. Villari must be sought.

We are moreover in accord with him upon this point, that the rotation of the plane of polarization ceases at the same instant as the electric action.—*Comptes Rendus de l'Académie des Sciences*, June 12, 1882, t. xciv. pp. 1590–1592.

* Pogg. *Ann.* cxlix. p. 324 (1873).

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X. *Remarks on Absolute Systems of Physical Units.* By A. F. SUNDELL*.

THE great importance of the absolute system of physical units, introduced by Gauss, and developed by William Weber, Kohlrausch, Maxwell, and Jenkin, is no doubt fully recognized by every physicist. It is therefore the more surprising that the so-called *dimensions* of the physical units are so little employed in practice, notwithstanding the demonstration of their great utility given by Prof. Kohlrausch in his well-known *Leitfaden der praktischen Physik*. These dimensions may be regarded as the actual *names* of the units; and where they do not accompany the statement of the numerical values of magnitudes, the data are as incomplete as if, in giving a length or a surface, the unit of length or of surface employed were not mentioned.

The works of the authors named contain all that is necessary for the practical employment of the absolute system. But inasmuch as there are several different systems of absolute units employed in physics whose mutual relationships have not been sufficiently clearly pointed out, although definite indications of the connexion between them are to be found, especially in Weber's *Elektrodynamische Maassbestimmungen*, and in Maxwell's great work, 'A Treatise on Electricity and Magnetism,' I propose to put forward here an elementary theory of absolute systems of physical units.

* Translated from a separate impression from the *Acta Soc. Scient. Fenn.* Tom. xii. (Helsingfors, 1881), communicated by the Author.

1. Various physical conceptions have resulted from the endeavour of physicists to represent the results of their researches mathematically as simply as possible, and to extend them. Most physical conceptions, even if not required for this special purpose, are at any rate absolutely necessary for a satisfactory formal demonstration of physical laws. If, for example, we seek to represent the law of a given uniform motion mathematically, the equation

$$s = ct \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is sufficient; in which s represents the distance described in the time t, and c is a constant dependent on the units employed for s and t. If by means of this equation we determine the value of the constant c by means of the distance described in a given time, we are then able to calculate the distance for any time t. The equation (1) may then be regarded as the mathematical representation of the law of the given motion. But with the value found for the constant, the equation can be used only for this particular motion, and not for any other uniform motion. But for any other motion of the same kind, an equation of the same form may be used, but with a different value of the constant c. It is in fact impossible to give a general formula for uniform motion with only the two conceptions of *distance* and *time*. But if we employ also a third conception, h, proportional to the ratio of *distance* to *time*, we obtain the formula

$$h = c\frac{s}{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which is sufficient for any uniform motion. This conception (h) is of course that of velocity. If from any particular uniform motion we determine the value of the constant c, we are able to calculate from the formula (2) the value of any one of the three quantities when the values of the two other quantities are given.

Equation (2) may be regarded as the general formula of uniform motion; but it may also be regarded, on the other hand, as an equation defining the conception of velocity, if we take the conceptions of space and time as already in any way defined. Each new conception in physics may be defined by an equation containing this new quantity, together with other quantities previously defined. Thus, for example, the conception of acceleration may be defined by the equation

$$a = c\frac{s}{t^2},$$

which relates to uniformly accelerated motion with zero initial

velocity. But since $\frac{s}{t}$ is proportional to a certain velocity (viz. the final velocity), this equation may be better written

$$a = c\frac{h}{t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In order to extend these conceptions to every case, we must often take account of infinitesimals. Thus, for example, to extend the conception of velocity to the case of motion not uniform, it must be defined by the differential equation

$$h = c\frac{ds}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Acceleration, when not uniform, is defined by the equation

$$a = c\frac{dh}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

But even in these new forms the equations express the same things as in their simpler form. Velocity is proportional to the increase of distance, and acceleration to the increase of velocity in the unit time. The conceptions of force (k) and mass (m) are connected by the equation

$$k = cma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This equation may be regarded as a definition of force, if mass be taken as a conception previously defined—or, on the other hand, as a definition of mass, if the conception of force has been first defined in any way. Each additional conception is defined by a separate equation. Moreover, these quantities may be connected by equations distinct from the defining equations which express natural laws. Thus, for example, the Newtonian law of gravitation gives the equation connecting length, mass, and force,

$$k = c\frac{m^2}{\mathrm{L}^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where k denotes the force with which a mass m attracts an equal mass at a distance L.

2. We have thus a certain number (p) of distinct equations in which q quantities occur. Each equation may contain a constant. The units of the different quantities may be chosen at pleasure ; the numerical value of the constant in an equation depends on the units of the quantities occurring in the equation. If, for example, we determine the constant of equation (2) from the velocity of light, and take the geographical mile as unit of length, the second as unit of time, and the

velocity of light as the unit of velocity, we must put $s=40000$, $t=1$, $h=1$; hence $c=\frac{1}{40000}$. Equation (2) becomes therefore $h=\frac{s}{40000t}$, or $s=40000ht$; which holds good for any uniform motion so long as distance, time, and velocity are expressed in the units chosen. But if we take as unit of length a quadrant of the earth's meridian (1350 geographical miles) without changing the other units, then we must put $s=\frac{40000}{1350}=\frac{800}{27}$, and the constant c has now the value $\frac{27}{800}$.

As the constants of the equations thus change with the units, so, conversely, by choosing appropriate units, particular values may be given to the constants. If we choose the units so as to make certain constants equal to unity, the corresponding equations assume the simplest form, and the constants disappear from them. Units so chosen are termed *absolute units*, and form an *absolute system*. If we assume that p (the number of equations) is smaller than q (the number of quantities), then we may eliminate all the constants by choosing the units properly; $q-p$ units remaining undetermined, and p units having values determined by the condition that each constant is equal to unity. But if we wish to have more than $q-p$ units, say $q-p+x$ undetermined, then x constants must be retained, which are then to be considered as independent conceptions. Let us take a few examples. The four conceptions distance, time, velocity, and acceleration are connected by the equations (2) and (3). If we wish to eliminate the two constants, we must take as unit of velocity the velocity of the uniform motion in which unit distance is described in unit time; the unit of acceleration must be the acceleration of a uniformly accelerated motion in which the velocity increases or diminishes in unit time by unit velocity.

Then equations (2) and (3) may be written as follows:—

$$h=\frac{s}{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$a=\frac{h}{t}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and similarly equations (4) and (5) become

$$h=\frac{ds}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

$$a=\frac{dh}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Of the four units, any two are chosen at pleasure. If we choose for units of length and time the metre and the second respectively, then the unit of velocity becomes the velocity of a uniform motion in which 1 metre is described in 1 second; and if the velocity of uniformly accelerated motion change by one unit of velocity in one second, then its acceleration is unity. But if we arbitrarily choose the second and the velocity of light as units, then, according to equation (8), the unit of length is the distance (40,000 geographical miles) which light traverses in one second. The corresponding acceleration will be, as we easily see, $\frac{40000 \times 7420}{9{\cdot}81}$ times as great as the acceleration due to gravity. We need not follow out the two other possible combinations.

By choosing suitable units of mass and force we may eliminate the constants of equation (6), which assumes the form

$$k = ma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The six quantities—space, time, velocity, acceleration, force, and mass—are connected by the four equations (2), (3), (6), and (7); if we wish to eliminate the four constants, we must retain the four units already determined. Equation (7) assumes then the form

$$k = \frac{m^2}{\mathrm{L}^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

The units of mass and force must be chosen so that, on the one hand (equation 12), unit force communicates unit acceleration to unit mass, and so that, on the other hand, unit mass attracts an equal mass at unit distance with unit force. Of the six units any two may be chosen at pleasure. But if we wish to choose three units at pleasure—say those of length, time, and mass—then we must retain one constant, for example that of equation (7) (the "constant of attraction"); for, since the units have been some of them chosen at pleasure, and some of them determined by means of the three remaining equations, the constant of the equation in question does not usually become unity. We may then regard this constant as a quantity defined by the equation

$$c = \frac{k\mathrm{L}^2}{m^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

If we wish to choose four units at pleasure, say those of length, time, force, and mass, we must retain one more constant, for example that of equation (6).

By the introduction of absolute units the equations of defi-

nition are much simplified ; since the constants of the equations are partly eliminated, partly regarded as new conceptions, it may be said that the equations from which an absolute system is derived contain no constants—that is, no numerical factors; each side of the equation has only one term. We will call these equations the fundamental equations. Together with these equations there occur in physics a large number of equations or formulæ; but these are not distinct from the fundamental equations, but are formed from them by various combinations and methods of calculation, integration, and so forth : they are therefore not so simple as the fundamental equations; the two sides may contain several terms, into which various numerical factors enter. It is necessary in our choice of units not to lose sight of these equations derived from the fundamental equations ; only, in using them, we must express the quantities in the units obtained from the corresponding fundamental equations. If, for example, we employ the formula for centrifugal force,

$$k = \frac{mh^2}{r},$$

we must express k, m, h, and the radius of curvature r in the units which correspond to the fundamental equations (8), (9), and (12).

In the same way, the general equation for uniformly accelerated motion,

$$s = h_0 t + \tfrac{1}{2} a t^2,$$

requires the units for s, t, h, h_0, and a, which are obtained from the fundamental equations (8) and (9).

3. According to what has been explained, in an absolute system certain units may be chosen at pleasure ; these are termed *fundamental units*. The magnitudes of all the other units are determined by the condition that the constants in certain equations of definition are to be equal to unity : such units are therefore termed *derived units**. We see that, in consequence of the mode in which the absolute units are determined, the fundamental equations will be satisfied if in them we replace the quantities by their units. If, for example, we take equation (8) for velocity, it will also hold good if we replace the quantities by their respective units. But if we do this in the ordinary way, by putting $h=1$, $s=1$, and $t=1$, we arrive at the identity $1=1$, which is of no further use to us. But if we put $s=$ unit length, $t=$ unit time, $h=$ unit velocity,

* Kohlrausch, *Leitfaden*, 3rd ed. p. 206 ; Maxwell, 'Treatise on Electricity and Magnetism,' i. pp. 2 & 5.

we obtain the relation

$$\text{unit velocity} = \frac{\text{unit length}}{\text{unit time}};$$

i. e. unit velocity and the quotient $\frac{\text{unit length}}{\text{unit time}}$ are identical magnitudes; and we may therefore regard this quotient as the *name* of the unit of velocity, if this unit is derived from the units of length and time. But if we take the metre as the unit of length and the second as the unit of time, then the velocity derived from these units must be called $\frac{\text{metre}}{\text{second}}$, which means that a velocity equal to 10 such units of velocity is to be written $10\,\frac{\text{metre}}{\text{second}}$.

The function $\frac{\text{unit length}}{\text{unit time}}$ is called by Maxwell* the *dimensions* of the unit of velocity. If in all the fundamental equations we replace quantities by their units, we obtain a system of equations according to which the absolute units are functions amongst themselves of certain fundamental units. These functions, which are obtained by solution of these last-named equations, are the *dimensions* or *names* of the derived units.

For the sake of brevity we will, as a rule, denote the units by the same symbols as the quantities; but, to avoid confusion, we will include the symbols of units in brackets. Thus $[l]$ will denote the unit of length.

As an example of the different systems of units and dimensions, we will consider the group of the six conceptions of length, time, velocity, acceleration, mass, and force. We will choose $[l]$, $[t]$, and $[m]$ as fundamental units; then from the four equations (2), (3), (6), (7) we may omit three constants, say those of the first three equations; and so we obtain as fundamental equations the equations (8), (9), (12), (7). By substitution of the units we obtain the following relations:—

$$[h]=\left[\frac{l}{t}\right], \quad]a]=\left[\frac{h}{t}\right], \quad [k]=[ma], \quad [k]=\left[c_1\frac{m^2}{l^2}\right]. \quad (15)$$

By solution of these equations we obtain the following dimensions:—

$$[h]=\left[\frac{l}{t}\right], \quad [a]=\left[\frac{l}{t^2}\right], \quad [k]=\left[\frac{ml}{t^2}\right], \quad [c_1]=\left[\frac{l^3}{t^2m}\right]. \quad (16)$$

If we keep the same fundamental equations, but choose $[l]$,

* 'Treatise on Electricity and Magnetism,' i. p. 2. Compare Kohlrausch, *Leitfaden*, p. 207.

$[t]$, and $[k]$ for fundamental units, the dimensions become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [m]=\left[\frac{t^2k}{l}\right],\quad [c_1]=\left[\frac{l^4}{t^4k}\right]. \quad (17)$$

If, on the other hand, we leave the constant of equation (6) standing, we obtain new fundamental equations (8), (9), (6), and (13) for a system with three fundamental units, and the equations of the units become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{h}{t}\right],\quad [k]=[c_2ma],\quad [k]=\left[\frac{m^2}{l^2}\right], \quad (18)$$

where c_2 denotes the constant of equation (6). If, again, we choose $[l]$, $[t]$, and $[m]$ as fundamental units, we obtain the dimensions

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [k]=\left[\frac{m^2}{l^2}\right],\quad [c_2]=\left[\frac{mt^2}{l^3}\right]. \quad (19)$$

We see that the dimensions of units change not only with change of the fundamental units, but also when the fundamental units remain the same and the fundamental equations are taken differently.

If we wish to regard only $[l]$ and $[t]$ as fundamental units, all four constants are eliminated; the equations (8), (9), (12), and (13) become fundamental equations, and the dimensions become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [m]=\left[\frac{l^3}{t^2}\right], \text{ and } [k]=\left[\frac{l^4}{t^4}\right]. \quad (20)$$

On the other hand, if we assume four fundamental units, we must leave two constants standing. If we take equations (8), (9), (6), and (7) as fundamental equations, the equations of units become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{h}{t}\right],\quad [k]=[c_2ma],\quad [k]=\left[c_1\frac{m^2}{l^2}\right]. \quad (21)$$

If $[l]$, $[t]$, $[m]$, and $[k]$ are the fundamental units, the dimensions become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [c_2]=\left[\frac{t^2k}{lm}\right],\quad [c_1]=\left[\frac{l^2k}{m^2}\right]. \quad (22)$$

The choice of fundamental units is limited only by the consideration that they must not all occur in the same formula. Hence we see from equation (15) or (16) that $[l]$ $[t]$ and $[h]$, $[h]$ $[a]$ and $[t]$, $[l]$ $[t]$ and $[a]$, or $[k]$ $[m]$ and $[a]$ cannot be fundamental units at one and the same time; on the

other hand, the last-mentioned group may be employed for this purpose in the system corresponding to equation (8).

4. It is very important to be able to pass from one system of units to another, and to determine the ratio between the units of the two systems. This problem presents the following special cases:—

I. When the new system contains one new fundamental unit besides the fundamental units of the old system. Let L be the value in the old system of the new fundamental unit defined by the equation

$$L = M^h N^k, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

where M and N are quantities defined independently of L. As the unit for L in the new system may be chosen at pleasure, this quantity is expressed by a new number L_1; but the numerical values of M and N are not altered. Equation (23) is therefore not satisfied by L, M, and N, but must be written with a constant α,

$$L_1 = \alpha M^h N^k. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

The system of fundamental equations is therefore changed: the number of equations remains the same; but the number of quantities is increased by one. Since we must regard α as a new conception defined by equation (24), we must therefore increase the number of fundamental units by one. Consequently the relations of the units as well as their dimensions are partially altered. By comparing equations (23) and (24) we find that

$$L = \frac{L_1}{\alpha}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

whence, since the units are inversely proportional to the numerical values of a given quantity, it follows that

$$[L] = n[L_1], \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25a)$$

where n is the numerical value of α.

The ratios of the remaining units are easily obtained from the other fundamental equations without alteration. It is to be remarked that the constant α is in general not an abstract number, but that the dimensions of its unit are given by the equation

$$[L_1] = [\alpha M^h N^k]. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

If, for example, we wish to pass from the system (15) into the system (21), where, besides the three fundamental units $[l]$, $[t]$, and $[m]$, we take $[k]$ also as a fundamental unit, we must start from the equation (12), which defines the conception k.

This equation becomes

$$k_1 = c_2 m a.$$

We have between the old unit of force and the new one the relation

$$[k] = n[k_1],$$

where n denotes the numerical value of c_2. The new dimensions of the units are determined by equation (22).

Example 1.—Let us take as the old unit of force

$$[k] = 1 \frac{\text{metre} \times \text{kilogramme}}{(\text{second})^2},$$

and as new unit of force the weight of a gramme at the Observatory in Paris; then, by equation (2), since $m = 0{\cdot}001$ kilogramme, and $a = g = 9{\cdot}808 \frac{\text{metre}}{(\text{second})^2}$,

$$[k_1] = 0{\cdot}009808 \frac{\text{metre} \times \text{kilogramme}}{(\text{second})^2} = 0{\cdot}009808 [k].$$

Hence $n = \frac{1}{0{\cdot}009808}$; and as the dimensions of c_2 are $\left[\frac{t^2 k_1}{lm}\right]$, the complete value of the new constant becomes

$$c_2 = \frac{1}{0{\cdot}009808} \frac{(\text{second})^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}},$$

if we call the new unit of force a gramme (Paris). We have further,

$$\left.\begin{aligned} 1 \text{ gramme (Paris)} &= 0{\cdot}009808 \frac{\text{metre} \times \text{kilogr.}}{(\text{second})^2}, \\ 1 \frac{\text{metre} \times \text{kilog.}}{(\text{second})^2} &= \frac{1}{0{\cdot}009808} \text{ gramme (Paris).} \end{aligned}\right\} \quad (27)$$

II. One of the fundamental units of the old system is a derived unit in the new system; the remaining units are common to both systems. Since the new system contains one fundamental unit less, it must also contain one constant less. The new system is therefore obtained by eliminating a constant.

Let

$$\alpha \mathrm{L} = \mathrm{M}^h \mathrm{N}^k \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

be the equation containing the constant α to be eliminated. In the new system this equation assumes the form

$$\mathrm{L}_1 = \mathrm{M}^h \mathrm{N}^k, \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

and defines the conception L_1; the dimensions of its unit are

given by the equation

$$[L_1]=[M^h N^k]. \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

Now we find

$$\alpha L = L_1,$$
$$[L]=n[L_1], \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

where, again, n is the numerical value of α. If we introduce into this equation the dimensions of $[L_1]$ as well as the dimensions of $[L]$, if this unit is also a derived unit in the old system, we obtain at any rate a relation into which the fundamental unit enters, which is to be made a derived unit. We can then determine from equation (31) the ratio of the units in question in the old and new systems.

If, for example, we wish to pass from the last system with four fundamental units to system (17), where $[l]$, $[t]$, and $[k]$ are fundamental units, we must make $[m]$ a derived unit and eliminate the constant c_2 from the equation $k=c_2 ma$. We accomplish this by putting $c_2 m=m_1$, whence

$$[m]=n[m_1],$$

where n is the numerical value of c_2. The new unit of mass $[m_1]$, according to equation (17), has the dimensions $\left[\frac{t^2 k}{l}\right]$. If we retain the last units, we have

$$c_2=\frac{1}{0{\cdot}009808}\,\frac{\text{sec.}^2\times\text{gramme (Paris)}}{\text{metre}\times\text{kilogramme}},$$

and

$$\left.\begin{aligned} 1\text{ kilogramme} &= \frac{1}{0{\cdot}009808}\,\frac{\text{sec.}^2\times\text{gramme (Paris)}}{\text{metre}},\\ 1\,\frac{\text{sec.}^2\times\text{gramme (Paris)}}{\text{metre}} &= 0{\cdot}009808\text{ kilogramme}. \end{aligned}\right\} \quad (32)$$

If we wish to make $[t]$ and not $[m]$ a derived unit, we must put $c_2 a=a_1$, so that the equation $k=c_2 ma$ becomes $k=ma_1$, defining the conception a_1.

We obtain then

$$[a]=n[a_1];$$

and by introducing the dimensions of $[a]$ and $[a_1]$,

$$\left[\frac{l}{t^2}\right]=n\left[\frac{k}{m}\right],$$

whence

$$[t]=\frac{1}{\sqrt{n}}\left[\frac{l^{\frac{1}{2}} m^{\frac{1}{2}}}{k^{\frac{1}{2}}}\right]=\frac{1}{\sqrt{n}}[t_1].$$

Retaining the former units, we have

$$\left.\begin{aligned} 1 \text{ second} &= \sqrt{0{\cdot}009808}\,\frac{\text{metre}^{\frac{1}{2}}\,\text{kilogr.}^{\frac{1}{2}}}{\text{gramme (Paris)}^{\frac{1}{2}}}, \\ 1\,\frac{\text{metre}^{\frac{1}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{gramme (Paris)}^{\frac{1}{2}}} &= \frac{1}{\sqrt{0{\cdot}009808}}\ \text{second}. \end{aligned}\right\} \quad . \quad (33)$$

In the same way we find, if $[l]$ is to be made a derived unit,

$$\left.\begin{aligned} 1 \text{ metre} &= \frac{1}{0{\cdot}009808}\,\frac{\text{gramme (Paris)} \times \text{sec.}^2}{\text{kilogr.}}, \\ 1\,\frac{\text{gramme (Paris)} \times \text{sec.}^2}{\text{kilogr.}} &= 0{\cdot}009808 \text{ metre}. \end{aligned}\right\} . (34)$$

It is to be remarked that the relations (27), (32), (33), and (34) may be regarded as determinations of the four units from any one of them.

III. By successive employment of the two preceding methods we are able to exchange one fundamental unit for another, while retaining the same fundamental equations. In one of the examples given, we have in fact exchanged the unit of mass (kilogramme) for the unit of force (gramme, Paris) while retaining the two remaining units (metre and second).

IV. By successively employing methods I. and II., we may change the system of fundamental equations by transferring a constant from one equation to another without altering the number of the fundamental units. If, for example, we wish to pass from the system (15) to the system (18), we first of all eliminate the constant of equation (7) by method (II.), and then introduce a constant into equation (12) by method I. We may of course adopt the reverse method. The calculation is most simple if it is possible to retain the same fundamental units. If, for example, in the system of equations (8), (9), (12), and (7) the constant is to be transferred from equation (7) to equation (8), we must make the following substitutions, $[l]$, $[m]$, and $[t]$ being the fundamental units :—in equation (7), $\frac{k}{c} = k_1$; in equation (12), $k = k_1 c$, and hence $\frac{a}{c} = a_1$; in equation (9), $a = a_1 c$, and hence $\frac{h}{c} = h_1$; and in equation (8), $h = h_1 c$. We obtain thus the fundamental equations

$$h_1 = \frac{1}{c}\frac{s}{l}; \quad a_1 = \frac{h_1}{l}; \quad k_1 = m a_1; \quad k_1 = \frac{m^2}{l^2};$$

and the dimensions become

$$[k_1]=\left[\frac{m^2}{l^2}\right],\quad [a_1]=\left[\frac{m}{l^2}\right],\quad [h_1]=\left[\frac{ml}{l^2}\right],\quad [c]=\left[\frac{l^3}{t^2m}\right],$$

as before.

5. The rules we have given make it possible to pass from one system of units to another. The process, however, is somewhat troublesome. When the magnitude of a physical conception is given in one system of units and we wish to find its numerical value in another system, the following is in all cases the safest and quickest method of arriving at the result* :—

If we assume three fundamental units and denote them by P, Q, R, then the dimensions of a derived unit N are of the form

$$N=P^aQ^bR^c,$$

where a, b, c are positive or negative rational exponents. If we assume units of other magnitudes p, q, r, and if $P=ep$, $Q=fq$, $R=gr$, where e, f, and g are constants, then

$$N=e^af^bg^c\,.\,p^aq^br^c.$$

If p, q, and r are fundamental units, the new derived unit n has the dimensions p^a, q^b, r^c, and

$$N=e^af^bg^c\,.\,n,$$

or the given unit N is $e^af^bg^c$ times as large as the new unit n.

If one of the new units, for example p, be itself a unit derived from the units q, r, and a new fundamental unit s, by means of the formula

$$p^a=q^{b_1}r^{c_1}s^d,$$

then

$$N=e^af^bg^c\,.\,p^aq^br^c=e^af^bg^c\,.\,q^{b+b_1}r^{c+c_1}s^d.$$

The new unit n is determined by the formula

$$n=q^{b+b_1}r^{c+c_1}s^d;$$

consequently in this case also

$$N=e^af^bg^c\,.\,n.$$

We obtain thus the following practical rule for obtaining the value of a unit referred to new fundamental units:—In the dimensions of the unit replace the old fundamental units by their values in the new fundamental units, and carry out the algebraic operations indicated as if the names of the units were algebraic magnitudes. This rule gives not only the ratio of the old unit N to the new unit n, but also the dimen-

* Compare Kohlrausch, *Leitfaden*, p. 207.

sions of the new unit. Of course the rule holds good if the number of fundamental units is greater or smaller than three.

Example 2.—The unit of force derived from the metre, second, and kilogramme $= 1\,\frac{\text{metre} \times \text{kilogr.}}{\text{sec.}^2}$. If now we take the centimetre, the gramme, and the minute as new fundamental units, we put

metre = 100 centimetres, kilogramme = 1000 grammes,
second = $\frac{1}{60}$ minute,

and so obtain

$$1\,\frac{\text{metre} \times \text{kilogr.}}{\text{sec.}^2} = \frac{100\ \text{centimetres} \times 1000\ \text{grammes}}{(\frac{1}{60}\ \text{minute})^2}$$

$$= 100 \times 1000 \times 60^2\ (= 360 \times 10^6)\,\frac{\text{centim.} \times \text{gramme}}{(\text{minute})^2};$$

that is, the first unit of force is 360 million times larger than the second.

If here we wish to exchange the former unit of mass, the kilogramme, for the derived unit $\frac{(\text{sec.})^2 \times \text{gramme (Paris)}}{\text{metre}}$, then by equation (32) we obtain

$$1\,\frac{\text{metre} \times \text{kilogr.}}{\text{sec.}^2} = 1\,\frac{\text{metre} \times \frac{1}{0{\cdot}009808} \times \frac{(\text{sec.})^2 \times \text{grm. (Paris)}}{\text{metre}}}{(\text{sec.})^2}$$

$$= \frac{1}{0{\cdot}009808}\,\text{gramme (Paris)}.\ \text{Compare equation (27)}.$$

It is clear that the same rule holds for the reduction of any magnitudes to new fundamental units.

Example 3.

$$1\ \text{Daniell} = 112 \times 10^9\,\frac{\text{millim.}^{\frac{3}{2}} \times \text{mgr.}^{\frac{1}{2}}}{\text{sec.}^2}\ \text{(Gauss-Weber system)}$$

$$= 112 \times 10^9\,\frac{(0{\cdot}001\,\text{metre})^{\frac{1}{2}} \times (0{\cdot}000001\ \text{kilogr.})^{\frac{1}{2}}}{\text{sec.}^2}$$

$$= \frac{112 \times 10^9}{10^{\frac{3}{2}} \times 10^{\frac{6}{2}}}\,\frac{\text{metre}^{\frac{3}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{sec.}^2}$$

$$= 112 \times 10^{\frac{3}{2}}\,\frac{\text{metre}^{\frac{3}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{sec.}^2}$$

$$= 112 \times 10^{\frac{3}{2}}\,\frac{\text{metre}^{\frac{3}{2}} \times \left(\frac{1}{0{\cdot}009808}\,\frac{\text{sec.}^2 \times \text{grm. (Paris)}}{\text{metre}}\right)^{\frac{1}{2}}}{\text{sec.}^2}$$

$$= \frac{112 \times 10^{\frac{3}{2}}}{\sqrt{0{\cdot}009808}}\,\frac{\text{metre} \times \text{gramme (Paris)}^{\frac{1}{2}}}{\text{sec.}}\ \text{(gravitation system, Equation 32).}$$

By the aid of this rule, the elimination of a constant is more easily effected than as given above by II. If the constant to be eliminated has the value

$$\alpha = n[P^a Q^b R^c],$$

where [P], [Q], and [R] are the fundamental units, and if we wish to make the fundamental unit [P], for example, into a derived unit and at the same time to eliminate the constant, *i. e.* to make it equal to unity, we must put

$$[P] = n^{-\frac{1}{a}}[Q^{-\frac{b}{a}} R^{-\frac{c}{a}}],$$

where α becomes equal to unity.

If, again, we wish to eliminate the constant

$$c_2 = \frac{1}{0{\cdot}009808} \frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}}$$

introduced into example 1 by making the unit of mass a derived unit, we do so by putting

$$1 \text{ kilogramme} = \frac{1}{0{\cdot}009808} \frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre}}$$

(compare equation 32); c_2 becomes equal to unity, and we have a system in which the only fundamental units are the units of length, time, and force. In order to make the unit of time a derived unit, we put

$$1 \text{ sec.}^2 = 0{\cdot}009808 \frac{\text{metre} \times \text{kilogr.}}{\text{grm. (Paris)}}$$

or

$$1 \text{ sec.} = \sqrt{0{\cdot}009808} \frac{\text{metre}^{\frac{1}{2}} \text{ kilogr.}^{\frac{1}{2}}}{\text{grm.}^{\frac{1}{2}} \text{ (Paris)}}$$

(compare equation 33), and so on.

We may follow a similar rule in using a physical formula to find the value of an unknown magnitude. We introduce the numerical values of the known magnitudes into the formula, together with the appropriate dimensions or names of the units, and solve the equation, thus finding at once the value of the unknown magnitude and its dimensions.

Example 4.—Let us determine the value of the constant c_2 from the equation $k_1 = c_2 ma$ (compare example 1). If the force $k_1 = 1$ gramme (Paris) act upon the mass $m = 0{\cdot}001$ kilogramme, it produces the acceleration

$$a = g \text{ (Paris) } 9{\cdot}808 \frac{\text{metre}}{\text{second}^2}.$$

If we put these values in the equation given, we have

$$1\,\text{gramme (Paris)} = c_2 \times 0{\cdot}001\,\text{kilogramme} \times 9{\cdot}808\,\frac{\text{metre}}{\text{sec.}^2},$$

$$1\,\text{gramme (Paris)} = 0{\cdot}009808\,\frac{\text{metre} \times \text{kilogramme}}{\text{sec.}^2} \times c^2,$$

and

$$c_2 = \frac{1}{0{\cdot}009808}\,\frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}}.$$

The rule given above is also of service in the exchange of units, as is shown in the following examples:—

Example 5.—Let the velocity of light be taken as fundamental unit instead of the unit of time. We know that

$$\text{Velocity of light} = 40{,}000\,\frac{\text{geographical miles}}{\text{second}}.$$

Then we have the velocity

$$1\,\frac{\text{metre}}{\text{second}} = \frac{1}{7420}\,\frac{\text{geogr. mile}}{\text{second}} = \frac{1}{7420 \times 40{,}000}\ \text{vel. of light},$$

and the acceleration

$$9{\cdot}808\,\frac{\text{metre}}{(\text{sec.})^2} = 9{\cdot}808\,\frac{(\text{metre})^2}{(\text{sec.})^2} \times \frac{1}{\text{metre}}$$

$$= \frac{9{\cdot}808}{7420^2 \times 40{,}000^2}\,\frac{(\text{velocity of light})^2}{\text{metre}}.$$

From this last number we may obtain the ordinary value as follows:—

$$\frac{9{\cdot}808}{7420^2 \times 40{,}000^2}\,\frac{\text{vel. of light}^2}{\text{metre}} = \frac{9{\cdot}808 \times 40{,}000^2}{7420^2 \times 40{,}000^2}\,\frac{\text{geogr. mile}^2}{\text{sec.}^2 \times \text{metre}}$$

$$= \frac{9{\cdot}808 \times 40{,}000^2 \times 7420^2}{7420^2 \times 40{,}000^2} = 9{\cdot}808\,\frac{\text{metre}}{\text{sec.}^2}.$$

Example 6.—Let the acceleration due to gravity (Paris) $= 9{\cdot}808\,\frac{\text{metre}}{\text{sec.}^2}$ be taken as the fundamental unit instead of the unit of time. We obtain then, for example,

$$1\ \text{horse-power} = 75\,\frac{\text{metre} \times \text{kilogr. (Paris)}}{\text{second}} = 75\,\frac{\text{met.}^{\frac{1}{2}}}{\text{sec.}} \times \text{metre}^{\frac{1}{2}}$$

$$\times\,\text{kilogr.(Paris)} = 75\left(\frac{\text{met.}}{\text{sec.}^2}\right)^{\frac{1}{2}} \times \text{met.}^{\frac{1}{2}} \times \text{kilogr.(Paris)}.$$

$$= \frac{75}{\sqrt{9{\cdot}808}}\ \text{acceleration due to gravity (Paris)}^{\frac{1}{2}} \times \text{metre}^{\frac{1}{2}}$$

$$\times\,\text{kilogr. (Paris)}.$$

6. The systems of units most frequently employed in physics are that due to Gauss and Wilhelm Weber, whose fundamental units are those of length, time, and mass, and that employed in many text-books (*e. g.* in Wüllner's *Experimental-Physik*), as well as by engineers, which has for fundamental units the units of length, time, and force. In this last system the gravitating force of a unit of weight is taken as the unit of force. This system may therefore be termed the gravitation system. The Newtonian equation (7) holds good for this system with the introduction of a constant (constant of attraction). The examples given above (examples 2 and 3) show how a quantity given in the Gauss-Weber system may be transformed into the gravitation system: the unit of mass of the first system must in general be replaced by the derived unit of mass of the second system. Conversely, if we wish to pass from the second system to the first, we must replace the unit of force by the derived unit of force of the first system. For this purpose we employ relationships of the same kind as equations (32) and (27).

The reduction is made most simply by employing the gravitating force of the unit of mass (m) of the first system as the unit of force (k_1) of the second system. We have then

$$\left.\begin{aligned} [k_1] &= g[k] = g\left[\frac{lm}{t^2}\right], \\ \text{and hence} \quad [m] &= \frac{1}{g}\left[\frac{t^2 k_1}{l}\right], \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (35)$$

where g expresses the numerical value of the acceleration of gravity at the place. Since the name of unit of weight is employed both for the unit of mass and for the unit of force, these units may be easily distinguished from each other by attaching to the name of the unit of force the name of the place where the gravitating force of the unit of weight is equal to the unit of force, as we have done in previous examples.

Example 7.

$$\text{One horse-power} = 75\,\frac{\text{metre} \times \text{kilogramme (Paris)}}{\text{second}}$$

(gravitation system)

$$= 75\,\frac{\text{metre} \times 9{\cdot}808\,\dfrac{\text{metre} \times \text{kilogr.}}{\text{second}^2}}{\text{second}}$$

$$= 75 \times 9{\cdot}808\,(=735{\cdot}6)\,\frac{\text{metre}^2 \times \text{kilogr.}}{\text{second}^2}$$

(Gauss-Weber system).

The chief constant of the two systems is the constant of

attraction—which is equal to the square of the so-called Gauss's constant. The value of this constant is given by Gauss* as

$$0{\cdot}0172\frac{\text{semi-major axis of earth's orbit}^{\frac{3}{2}}}{\text{mean day} \times \text{sun's mass}^{\frac{1}{2}}}.$$

Hence we have the attraction-constant

$$=(0{\cdot}0172)^2\frac{\text{semi-major axis of earth's orbit}}{\text{day}^2 \times \text{sun's mass}}$$

$$=\frac{(0{\cdot}0172)^2\times(19{,}900{,}000)^3}{(86{,}400)^2\times 320{,}000}\frac{\text{geogr. mile}^3}{\text{sec.}^2\times\text{earth's mass}}$$

$$=\frac{(0{\cdot}0172)^2\times(19{,}900{,}000)^3\times(7420)^3}{(86{,}400)^2\times 320{,}000\times\frac{4}{3}\times 3{\cdot}14\times(6{,}370{,}000)^3\times 6000}\frac{\text{metre}^3}{\text{sec.}^2\times\text{kilogr.}}$$

$$=\frac{615}{10^{13}}\frac{\text{metre}^3}{\text{sec.}^2\times\text{kilogr.}},$$

if we take the mean mass of 1 cubic metre of the earth as equal to 6000 kilogr. The force with which a mass of 1 kilogr. attracts an equal mass at a distance of 1 metre is then $\frac{615}{10^{13}}\frac{\text{metre}\times\text{kilogr.}}{\text{sec.}^2}$. If we transform the value of the constant of attraction into the gravitation system, we obtain

$$\frac{615}{10^{13}}\frac{\text{metre}^3}{\text{sec.}^2\times\text{kilogr.}}=\frac{615}{10^{13}}\frac{\text{metre}^3}{\text{sec.}^2\times\frac{1}{9{\cdot}808}\frac{\text{kilogr. (Paris)}}{\text{metre}}}$$

$$=\frac{615\times 9{\cdot}808}{10^{13}}\left(=\frac{604}{10^{12}}\right)\frac{\text{metre}^4}{\text{sec.}^4\times\text{kilogr. (Paris)}}\dagger.$$

7. Of systems with four fundamental units, that with fundamental units of length, time, mass, and force is not without interest. In order to obtain such a system we must retain two constants in the fundamental equations (2), (3), (6),

* *Theoria Motus Corporum Cœlestium*, p. 2.

† Wüllner (*Lehrbuch der Experimental-Physik*, vol. i. 1870. p. 145) puts the mass of a cubic metre of the earth $=6$, and hence finds for the attraction-constant the value $\frac{6128}{10^{11}}$, which value, if the unit of mass ($=9{\cdot}808$ kilogr.) employed in the same work (vol. i. p. 59) is to be retained, is $\frac{1000}{9{\cdot}808}$ times too great, and must be altered to

$$\frac{9{\cdot}808}{1000}\times\frac{6128}{10^{11}}=\frac{601}{10^{12}}$$

(very nearly the same as given above). The unit of mass,

$$1\frac{\text{sec.}^2\times\text{kilogr. (Paris)}}{\text{metre}}=9{\cdot}808\text{ kilogr.},$$

therefore attracts an equal mass at a distance of 1 metre with the force $\frac{601}{10^{12}}$ kilogr.

and (7). If we choose for this purpose the constant of equation (6) (the force-constant) and that of equation (7) (the attraction-constant), we obtain a system in which the dimensions of the chief units are given by equation (22). We have already found the value of the force-constant c_2 (example 1), namely

$$c_2 = \frac{1}{0{\cdot}009808} \frac{(\text{second})^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}}$$
$$= \frac{1}{9{\cdot}808} \frac{(\text{second})^2 \times \text{kilogramme (Paris)}}{\text{metre} \times \text{kilogramme}}.$$

According to equation (22), the dimensions of the attraction-constant are $[c_1] = \left[\frac{l^2 k}{m^2}\right]$; we may find its new value as follows:—

$$c_1 = \frac{615}{10^{13}} \frac{(\text{metre})^3}{(\text{sec.}^2) \times \text{kilogr.}} = \frac{615}{10^{13}} \frac{(\text{metre})^2}{(\text{kilogr.})^2} \times \frac{\text{metre} \times \text{kilogr.}}{(\text{second})^2}$$
$$= \frac{615}{10^{13}} \frac{(\text{metre})^2}{(\text{kilogr.})^2} \times \frac{1}{9{\cdot}808} \text{ kilogr. (Paris) (equation 35)}$$
$$= \frac{615}{10^{13} \times 9{\cdot}808} = \frac{(\text{metre})^2 \times \text{kilogramme (Paris)}}{(\text{kilogramme})^2},$$

or

$$c_1 = \frac{627}{10^{14}} \frac{(\text{metre})^2 \times \text{kilogramme (Paris)}}{(\text{kilogramme})^2}.$$

The other mechanical equations of definition would remain unaltered. Thus we should have:—

for the work A, the relation $A = ks$, with the dimensions

$$[A] = [kl];$$

for the *vis viva*, $u = mh^2$,

$$[u] = \left[\frac{ml^2}{t^2}\right];$$

for the momentum, $q = mh$,

$$[q] = \left[\frac{ml}{t}\right];$$

and so on.

Such a system has this advantage for a beginner—that he is able to express either a force or a mass in units of weight, and so would escape the reduction (difficult to understand and often forgotten) of a force from units of weight into units of force of the Gauss-Weber system, or of a mass from units of weight into units of mass of the gravitation system. The following circumstance may, however, perhaps be considered inconvenient. The constant of force passes over into the equations which give the connexion between work and change of

vis viva, and between the so-called time-integral of force* $\int k\,dt$ and the change of momentum, as well as into certain other mechanical equations which are derived from equation (6).

The equations are as follows:—

$$A = \tfrac{1}{2} c_2 m(h^2 - h_0^2) = \tfrac{1}{2} c_2 (u - u_0),$$

$$\int k\,dt = c_2 m(h - h_0) = c_2 (q - q_0),$$

instead of

$$A = \tfrac{1}{2}(u - u_0),$$

$$\int k\,dt = q - q_0,$$

in the usual systems with three fundamental units. That the equations in question, as well as equation (6), are free from any special constant in the usual systems is, no doubt, to be formally mentioned; but the circumstance is not of practical importance. For physicists often, and engineers almost always, express both forces and masses in units of weight, and the reduction to absolute units necessary for the employment of the ordinary formulæ introduces a constant in a disguised form.

8. The different systems of electrical measurement arranged by Wilhelm Weber, which are connected with the magnetic system of Gauss, are of special interest.

We have the following equations between the chief conceptions of this group of phenomena :—

$$\mu^2 = ck\mathrm{L}^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

$$e^2 = ck\mathrm{L}^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

$$i = c\frac{e}{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

$$i = c\frac{k\mathrm{L}^2}{l\mu}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

$$i^2 = c\frac{k\mathrm{L}^2}{l^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

$$\mathrm{E} = c\frac{kh}{i}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

where μ denotes the quantity of free magnetism or strength of a magnetic pole, c the quantity of electricity, and i the current-strength; l is the length of an element of the current-path, L the distance between two elements of the current, two magnetic poles, or between a current-element and a magnetic pole; k is the attraction or repulsion between current-elements, magnetic poles, or electric masses. The magnetic pole is to

* Thomson and Tait, 'Theoretical Physics,' § 29.

be supposed to be in the line drawn from the centre point of the element acting on it, and at right angles to it; the two current-elements which act upon each other are to be regarded as parallel to each other and at right angles to the line joining their centres.

The complete formulæ are

$$ck = \frac{il\mu}{L^2} \sin\theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39\text{A})$$

$$ck = \frac{i^2 l^2}{L^2} (\cos\epsilon - \tfrac{3}{2}\cos\theta\cos\theta_1; \quad . \quad . \quad . \quad (40\text{A})$$

where ϵ, θ, and θ_1 are known angles. Equation (41) relates to the motion of a closed circuit in the neighbourhood of currents or of magnets; i is the strength of the induced current, k the electrodynamic action of the inducing currents or magnets on the induced conductor, estimated according to the direction of the motion; h is the velocity of motion, and E the induced electromotive force.

Since the units of length, time, velocity, and force are already fixed by a system of fundamental equations, we have six equations connecting the four new conceptions, μ, c, i, and E. In forming the corresponding fundamental equations we must therefore retain two constants. The electrostatic (or mechanical) system is obtained by retaining the constants of equations (36) and (40). In the electromagnetic system we retain the constants of equations (37) and (40). A very convenient system is obtained by retaining the constants of equations (38) and (40); this system may be called the Gauss-Weber system: it is completely worked out in Kohlrausch's *Leitfaden*. Lastly, we have the electrodynamic system with the constants of equations (37) and (39). The values of the constants are as follows in the Gauss-Weber system:—$c = \frac{1}{2}$ (equations 40 and 40 A), and $c = \dfrac{1}{e_0}$ (equation 38), where e_0 is the total quantity of electricity (positive and negative) passing per second with the unit current of this system. Weber and Kohlrausch have found

$$e_0 = 31 \times 10^{10} \frac{\text{millimetre}}{\text{second}}{}^{*}. \quad . \quad . \quad . \quad . \quad . \quad (42)$$

* Wiedemann, *Lehre vom Galvanismus und Elektromagnetismus*, vol. ii. div. 2. p. 457. Weber himself takes into account only one form of electricity, and puts therefore $e_0 = \dfrac{31 \times 10^{10}}{2}$ (more exactly $15{\cdot}537 \times 10^{10}$) $\dfrac{\text{milllimetre}}{\text{second}}$ (*Abh. der königl. sächs. Ges. der Wissensch.* vol. iii. Also in Zöllner, *Principien einer elektrodynamischen Theorie*, pp. 93-131).

By the aid of IV. we easily find the following values of the constants in the other systems:—

Electrostatic system,

$$c=\frac{1}{e_0^2}\text{ (equation 36)}.\quad c=\frac{e_0^2}{2}\text{ (equations 40 and 40A)};$$

electromagnetic system,

$$c=\frac{1}{e_0^2}\text{ (equation 37)},\quad c=\frac{1}{2}\text{ (equations 40 and 40A)};$$

electrodynamic system,

$$c=\frac{2}{e_0^2}\text{ (equation 37)},\quad c=\sqrt{2}\text{ (equations 39 and 39A)}.$$

We obtain a formula of induction suited for practical use by replacing the force k in equation (41) by its value calculated from the formula (39A) or (40A).

Since the constant of equation (41) has the value unity in all four systems, it follows that the work performed by the current in unit time is equal to the product of electromotive force into current-strength. Moreover the equation corresponding to Ohm's law which determines resistance is of the same form in all three systems.

The relations of the chief units of the electromagnetic, electrodynamic, and electrostatic systems, together with their dimensions, are given in Prof. Wiedemann's *Lehre vom Galvanismus und Elektromagnetismus*, vol. ii. div. 2. pp. 467–471; they are also given for the electrostatic and electromagnetic systems in Maxwell's 'Treatise on Electricity and Magnetism,' vol. ii. pp. 239–245.

The occurrence of the constant e_0 in so many of the formulæ connected with electricity would seem to show that these formulæ could be derived from some natural law. Wilhelm Weber finds this in his *fundamental electrical law*,

$$k=\frac{e_1e_2}{\mathrm{L}^2}\left[1-\frac{a^2}{16}\left\{\left(\frac{d\mathrm{L}}{dt}\right)^2-2\mathrm{L}\frac{d^2\mathrm{L}}{dt^2}\right\}\right].$$

Here $\frac{1}{a}$ is the quantity of positive or negative electricity (measured electrostatically like e_1 and e_2) which traverses the circuit in unit time when the current-strength is equal to the electrodynamic unit current, *i. e.* is equal to $\frac{1}{\sqrt{2}}$ electromagnetic unit. Consequently we have $\frac{1}{a}=\frac{e_0}{2\sqrt{2}}$ or $\frac{a^2}{16}=\frac{1}{2e_0^2}$,

where e_0 is to be taken in accordance with equation (42). The fundamental law may then also be written

$$k = \frac{e_1 e_2}{L^2}\left[1 - \frac{1}{2e_0^2}\left\{\left(\frac{dL}{dt}\right)^2 - 2L\frac{d^2L}{dt^2}\right\}\right]. \quad . \quad (43)$$

9. Systems with two fundamental units result by eliminating one constant from the fundamental equations for systems with three units. Various systems with two fundamental units may be formed by elimination of the constant of attraction. If in the value of this constant $\left(\frac{615}{10^{13}}\frac{\text{metre}^3}{\text{sec.}^2 \times \text{kilogr.}}\right)$ we put

$$1 \text{ kilogr.} = \frac{615}{10^{13}}\frac{\text{metre}^3}{\text{sec.}^2} = \frac{615}{10^4}\cdot\frac{\text{millim.}^3}{\text{second}^2}, \quad . \quad (44)$$

this constant becomes unity, and we obtain a system with the two fundamental units of length and time. The unit of mass derived from the metre and the second is consequently

$$1\frac{\text{metre}^3}{\text{second}^2} = \frac{10^{13}}{615} \text{ kilogramme.}$$

The unit of force 1 kilogramme (Paris)

$$= 9{\cdot}808\frac{\text{kilogr.} \times \text{metre}}{(\text{second})^2} = \frac{9{\cdot}808 \times 615}{10^{13}}\frac{\text{metre}^4}{\text{sec.}^4};$$

consequently the new unit of force is

$$1\frac{\text{metre}^4}{\text{second}^4} = \frac{10^{13}}{9{\cdot}808 \times 615} \text{ kilogramme (Paris).}$$

This system is employed for attraction-problems by Thomson and Tait ('Theoretical Physics,' §§ 459, 774)*. The density† of water $\left(=1000\ \frac{\text{kilogr.}}{\text{metre}^3}\right)$ becomes in this system $\frac{615}{10^{10}}\frac{1}{\text{sec.}^2}$ (thus independent of the unit of length). In order to compare this number with the number given by Thomson and Tait (§ 774) we must multiply it by $\frac{6}{5{\cdot}5}$, since Thomson and Tait put the mean specific gravity of the earth equal to 5·5. We

* Compare also H. Weber and Kohlrausch (Zöllner, *Principien einer elektrodynamischen Theorie*, p. 129); Maxwell, 'Treatise on Electricity and Magnetism,' vol. i. p. 4.

† Density is here taken to mean mass of unit volume of the substance (Maxwell, p. 5).

thus obtain for the density of water the number

$$\frac{6}{5{\cdot}5} \times \frac{615}{10^{10}} \frac{1}{\text{sec.}^2} = \frac{6{\cdot}7}{10^8} \frac{1}{\text{sec.}^2},$$

which agrees exactly with Thomson and Tait's number. It is to be observed that the dimensions of the electric and magnetic units in this system have exponents which are whole numbers. In the Gauss-Weber system the dimensions of $[\mu]$ and $[e]$ are the same as of $[m]$; for i we have the dimensions $\left[\frac{l^2}{t^2}\right]$, for $[\mathrm{E}']$ $\left[\frac{l^3}{t^3}\right]$, and so on.

Example 8.—The Weber unit of current

$$= 1 \frac{\text{mm.}^{\frac{1}{2}} \times \text{mgr.}^{\frac{1}{2}}}{\text{sec.}} = \frac{1}{10^{\frac{9}{2}}} \frac{\text{metre}^{\frac{1}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{sec.}}$$

$$= \frac{\sqrt{615}}{10^{\frac{9}{2}} \times 10^{\frac{13}{2}}} \left(= \frac{\sqrt{615}}{10^{11}}\right) \frac{\text{metre}^2}{\text{sec.}^2} \text{ (equation 44).}$$

If we wish to return to the ordinary system, we must observe equation (44). We may calculate as follows :—

$$\frac{\sqrt{615}}{10^{11}} \frac{\text{metre}^2}{\text{sec.}^2} \text{ (current-strength)} = \frac{\sqrt{615}}{10^{11}} \frac{\text{metre}^{\frac{3}{2}}}{\text{sec.}} \times \frac{\text{metre}^{\frac{1}{2}}}{\text{sec.}}$$

$$= \frac{\sqrt{615}}{10^{11}} \left(\frac{\text{metre}^3}{\text{sec.}^2}\right)^{\frac{1}{2}} \times \frac{\text{metre}^{\frac{1}{2}}}{\text{sec.}} = \frac{\sqrt{615}}{10^{11}}$$

$$\times \frac{10^{\frac{13}{2}}}{\sqrt{615}} \frac{\text{kilogr.}^{\frac{1}{2}} \times \text{metre}^{\frac{1}{2}}}{\text{sec.}} \text{ (eq. 44)} = \frac{1}{10^{\frac{7}{2}}} \frac{\text{metre}^{\frac{1}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{second}}$$

(Weber unit of current).

The constant of attraction may also be eliminated by putting

$$1 \text{ second} = \sqrt{\frac{615}{10^{13}}} \frac{\text{metre}^{\frac{3}{2}}}{\text{kilogr.}^{\frac{1}{2}}} = \frac{\sqrt{615}}{10^5} \frac{\text{millimetre}^{\frac{3}{2}}}{\text{milligr.}^{\frac{1}{2}}},$$

so that the unit of time becomes a unit derived from the unit of length and the unit of mass,

$$1 \frac{\text{metre}^{\frac{3}{2}}}{\text{kilogr.}^{\frac{1}{2}}} = \sqrt{\frac{10^{13}}{615}} \text{ seconds.}$$

The unit of force $1 \frac{\text{metre} \times \text{kilogr.}}{\text{second}^2}$ becomes $\frac{10^{13}}{615} \frac{\text{kilogr.}^2}{\text{metre}^2}$,

from which the new unit of force

$$1\,\frac{\text{kilogr.}^2}{\text{metre}^2} = \frac{615}{10^{13}} \cdot \frac{\text{metre} \times \text{kilogr.}}{\text{sec.}^2}$$

$$= \frac{615}{9{\cdot}808 \times 10^{13}}\ \text{kilogr. (Paris).}$$

The dimensions of the other principal units become

$$[a] = \left[\frac{m}{l^2}\right], \quad [h] = \left[\frac{m^{\frac{1}{2}}}{l^{\frac{1}{2}}}\right], \quad [e] = [\mu] = [m]$$

(according to the fundamental equations of the Gauss-Weber system),

$$[i] = \left[\frac{m}{l}\right], \quad \mathrm{E} = \left[\frac{m^{\frac{3}{2}}}{l^{\frac{3}{2}}}\right],$$

and so forth. A quantity of electricity is given in ordinary units: we find the electrostatic unit

$$1\,\frac{\text{millim.}^{\frac{3}{2}} \times \text{milligr.}^{\frac{1}{2}}}{\text{second}} = \frac{10^5}{\sqrt{615}}\ \text{milligr.},$$

or

$$1\ \text{milligramme} = \frac{\sqrt{615}}{10^5} \cdot \frac{\text{millim.}^{\frac{3}{2}} \times \text{milligr.}^{\frac{1}{2}}}{\text{sec.}}\ \text{Weber units};$$

i. e. $\frac{10^5}{\sqrt{615}}$ or about 4000 milligr. ordinary matter has equal action at a distance as 1 Weber unit of electricity.

Instead of eliminating the constant of attraction, we may obtain a system with two fundamental units by eliminating the constant

$$e_0 = 31 \times 10^{10}\,\frac{\text{millim.}}{\text{sec.}}.$$

If, in doing this, we wish to make the unit of time a derived unit, we must put

$$1\ \text{second} = 31 \times 10^{10}\ \text{millimetres.}$$

Time must therefore be expressed as a length, the new unit of time 1 millimetre $= \frac{1}{31 \times 10^{10}}$ sec.

The units of the four electric systems have the same dimensions and the same magnitude, with the exception of the units of the electrodynamic system. The dimensions of the most important units are as follows:—

$$[h]=[l^0m^0] \text{ (abstract number)},$$

$$[a]=\left[\frac{1}{l}\right],\quad [k]=\left[\frac{m}{l}\right],\quad [t]=[l],\quad [\mathrm{A}]=[m];$$

constant of attraction $[c_1]=\left[\frac{l}{m}\right]$, $\quad [e]=[\mu]=[m^{\frac{1}{2}}l^{\frac{1}{2}}]$,

$$[i]=\left[\frac{m^{\frac{1}{2}}}{l^{\frac{1}{2}}}\right]=[\mathrm{E}].$$

We see that, by eliminating the constant (e_0), the distinction of the units by their dimensions is lost for the most part—a result which was to be expected, since e_0 has no independent dimensions, but is to be regarded as a velocity.

10. The systems with two fundamental units have yet another constant; if this be also eliminated, we have a system with only one fundamental unit. If we start with the system given above, with the fundamental units of length and time, we may eliminate the remaining constant c_0 by the relationship

$$1 \text{ sec.}=31\times10^{10} \text{ millimetres.} \quad . \quad . \quad . \quad . \quad (45)$$

We thus obtain a system in which the unit length is the only fundamental unit. The three electrical systems now combine into one. The dimensions of the units are as follows:—

$$[h]=[l^0] \text{ (abstract number)},\quad [t]=[l],\quad [a]=\left[\frac{1}{l}\right],$$

$$[m]=[l],\quad [k]=[l^0],\quad [\mathrm{A}]=[l],\quad [\epsilon]=[\mu]=[l],$$

$$[i]=[\mathrm{E}]=[l^0].$$

The distinction of units by their dimensions is now completely lost; velocity, force, current-strength, and electromotive force are abstract numbers; time, mass, work, quantity of electricity, and magnetic pole are to be regarded as lengths.

The unit-mass $1\ \frac{\text{millim.}^3}{\text{sec.}^2}$ $\left(=\frac{10^4}{615}\right.$ kilogr.) becomes equal to $\frac{1}{31^2\times10^{20}}$ millim. Consequently the unit of mass

$$1 \text{ millim.}=31^2\times10^{20}\,\frac{\text{millim.}^3}{\text{sec.}^2}=\frac{31^2\times10^{24}}{615}\ (=157\times10^{22}) \text{kilog.},$$

or nearly one fourth of the earth's mass (650×10^{22} kilogr.).

Weber's formula may now be written

$$k=\frac{e_1e_2}{\mathrm{L}^2}\left[1-\tfrac{1}{2}\left\{\left(\frac{d\mathrm{L}}{dt}\right)^2-2\mathrm{L}\,\frac{d^2\mathrm{L}}{dt^2}\right\}\right].$$

Weber himself simplifies the formula thus:—

$$k=\frac{e_1e_2}{\mathrm{L}^2}\left[1-\left\{\left(\frac{d\mathrm{L}}{dt}\right)^2-2\mathrm{L}\,\frac{d^2\mathrm{L}}{dt^2}\right\}\right].$$

If we compare this formula with formula (43), we find that

it is not e_0, but $e_0\sqrt{2}$ which is eliminated by putting 1 sec. $= 31 \times 10^{10}\sqrt{2}$ millim. $= 44 \times 10^{10}$ millim. The new unit of time is $\frac{1}{44 \times 10^{10}}$ sec. The constant of attraction, too, is eliminated by Weber by introducing a unit of mass

$$\frac{2 \times 31^2 \times 10^{24}}{615} \text{ (} = 314 \times 10^{22}\text{) kilogr.}$$

(nearly half the earth's mass*). These last two formulæ appear to us equally simple; but the first of them, as we saw above, has the advantage of combining the three electrical systems into one. In returning to the ordinary systems, attention must be paid to equations (44) and (45).

Example 9.—The quantity of electricity 1000 millimetres is to be expressed in electrostatic units $\frac{\text{millim.}^{\frac{3}{2}} \times \text{mgr.}^{\frac{1}{2}}}{\text{sec.}}$. We have 1000 millim. electricity

$$= 1000 \frac{\text{millim.}^3}{\text{millim.}^{\frac{3}{2}}} = 31^2 \times 10^{20} \times 1000 \frac{\text{millim.}^3}{\text{sec.}^2} \text{ (equation 45)}$$

$$= 31^2 \times 10^{23} \left(\frac{\text{millim.}^3}{\text{sec.}^2}\right)^{\frac{1}{2}} \times \frac{\text{millim.}^{\frac{3}{2}}}{\text{sec.}} = \frac{10^2}{\sqrt{625}} \times 31^2$$

$$\times 10^{23} \frac{\text{millim.}^{\frac{3}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{sec.}} \text{ (equation 44)}$$

$$= \frac{10^2 \times 31^2 \times 10^{23} \times 10^3}{\sqrt{615}} \left(= \frac{10^{28} \times 31^2}{\sqrt{615}}\right) \frac{\text{millim.}^{\frac{3}{2}} \times \text{mgr.}^{\frac{1}{2}}}{\text{sec.}}.$$

It is to be observed that a system with two or with only one fundamental unit is not completely determinate, unless its connexion to the ordinary systems with three fundamental units is given by relations such as equations (44) and (45). The systems with three fundamental units are therefore to be regarded as the fundamental systems of mechanics.†

11. The ordinary units of mechanics are not convenient nor

* Weber and Kohlrausch (Zöllner, *l. c.* p. 130).

† The above systems with less than three fundamental units may be regarded as the result of taking the unit of length, the unit of velocity, and the unit-of-attraction constant as fundamental units. If, for example, we take the millimetre, the velocity e_0, and the attraction-constant c_1 as fundamental units, we obtain, from $c_1 = 615 \times 10^{-4}\text{mm.}^3 \times \text{sec.}^{-2} \times \text{kilogr.}^{-1}$, 1 kilogr. $= 615 \times 10^{-4}\text{mm.}^3 \times \text{sec.}^{-2} \times c_1^{-1}$; and from $e_0 = 31 \times 10^{10}$ mm. $\times$ sec.$^{-1}$, 1 sec. $= 31 \times 10^{10} \times$ mm. $\times e_0^{-1}$; which expressions become identical with (44) and (45), if we omit c_1 and e_0 from the dimensions. The constants c_1 and e_0 should be left in the fundamental equations, so as not to disturb their homogeneity.

sufficient to give a quantitative representation of all the phenomena of physics; but we are obliged sometimes to introduce arbitrary units. Among these may be reckoned the unit angle, which is very useful in considering circular motion, for which purpose it replaces the unit of length. From the unit angle and the unit of time we derive the units of angular velocity and angular acceleration; since, further, moment of inertia corresponds to mass, and moment of rotation to force, we obtain a complete analogy with motion in a straight line, and equations analogous to the equations (2), (3), and (6). This analogy is lost by regarding the angle as an abstract number.

The conceptions derived from the phenomena of heat.—A quantity of heat w is proportional to the elevation of temperature T which it can cause in a mass m. We obtain thus the equation of definition,

$$w = cm\mathrm{T},$$

where c is a factor depending on the material nature of the body, which we will call the *capacity for heat*. The dimensions of the unit of heat are determined by the equation

$$[w] = [cm\mathrm{T}].$$

We have here three new units, of which we may choose two as fundamental units. Of the three possible combinations, we have determined upon one, and choose units for capacity $[c]$ and temperature $[\mathrm{T}]$ at pleasure, so that the unit of heat $[w]$ becomes a derived unit: it is the quantity of heat required to raise by one unit the temperature of the unit mass of a substance having unit capacity. The units of temperature in use are well known; for unit of capacity we take the capacity of water; the capacity of a body is therefore expressed by the same number as its so-called specific heat. As examples furnished by the science of heat we may take the following :—

The heat necessary to raise 10 kilogr. mercury (capacity $=0{\cdot}033$ capacity of water) from 0° C. to 100° C. $= 0{\cdot}033 \times 10 \times 100 (=33)$ kilogr. × Centigrade degree × capacity of water.

Coefficient of linear expansion of iron $= 0{\cdot}000012 \dfrac{1}{\text{degree C.}}$

$$= 0{\cdot}000012 \frac{1}{\frac{4}{5}\text{ degree R.}} = \frac{5}{4} \times 0{\cdot}000012 \frac{1}{\text{degree R.}}$$

$$= \frac{5}{9} \times 0{\cdot}000012 \frac{1}{\text{degree F.}}.$$

Latent heat of ice $= 79{\cdot}25$ degrees C. × capacity of water.

We obtain the work A which is equivalent to a quantity of heat w by multiplying this quantity by the mechanical equivalent

of heat Q,

$$A = Qw.$$

The dimensions of Q are

$$\frac{[A]}{[w]}.$$

We usually put

$$Q = 424 \frac{\text{metre} \times \text{kilogramme (force)}}{\text{kilogramme (mass)} \times \text{degree C.} \times \text{capacity of water}}$$

$$= 424 \times 3{\cdot}281 \times \tfrac{5}{9} (= 773) \frac{\text{English foot} \times \text{English pound (force)}}{\text{Engl. pound (mass)} \times \text{deg. F.} \times \text{capac. of water}}.$$

This value does not agree with either of the two ordinary mechanical systems, since both the unit of force and the unit of mass are here fundamental units. But if we introduce either

$$1 \text{ kilogramme (force)} = 9{\cdot}81 \times \frac{\text{metre} \times \text{kilogr. (mass)}}{(\text{second})^2},$$

or

$$1 \text{ kilogramme (mass)} = \frac{1}{9{\cdot}81} \frac{(\text{second})^2 \times \text{kilogr. (force)}}{\text{metre}},$$

we obtain in both cases

$$Q = 424 \times 9{\cdot}81 (= 4160) \frac{(\text{metre})^2}{\text{sec.}^2 \times \text{degree C.} \times \text{capacity of water}},$$

which value may be employed either with the Gauss-Weber system or with the gravitation system.

In stating physical magnitudes, we frequently see arbitrary units employed besides the absolute units: it is important in using such units to state their names completely, the names being often formed according to the theory of dimensions.

Example.—Electrolytic (chemical) unit of current

$$= \frac{\text{cubic centim. of electrolytic gas}}{\text{minute}} \text{ or } \frac{\text{milligramme copper}}{\text{minute}}.$$

The rules for the reduction to new units are then always applicable, as the above examples in heat show, and render the calculation rapid and certain.

Helsingfors, January 1881.

Postscript.—After the above was in type the author became acquainted with Dr. Herwig's work, *Physikalische Begriffe und absolute Maasse* (Leipzig, 1880). The units of the science of heat are stated somewhat differently by Herwig: the equivalent Q is first eliminated, so that the units of quantity of heat and of work become identical; secondly, the capacity of water is taken as a conventional unit, and its name is omitted from the dimensions.

XI. Moseley's *Theory of Steady Flow*. *By* Major Allan Cunningham, *R.E.*, *Honorary Fellow of King's College, London**.

A NEW *Theory* of the Steady Flow of a Liquid was proposed by the late Canon H. Moseley, in an important essay published in this Magazine in 1871–72†. The paper was valuable as an attempt to form a rational theory of the steady motion of a real fluid, and to deduce results of great scientific and practical interest from it. Formulæ were arrived at for the *Velocity* at any point in a Pipe flowing full, and for the *Discharge* both of a Pipe flowing full and of an Open Channel, and for some other quantities of less importance.

In the investigation it was assumed that:—

I. *Pipes*.—In long uniform pipes at uniform slope, flowing full,—

1. The motion is a steady motion in lines parallel to the axis of the pipe.
2. The surfaces of equal velocity are similar, and similarly situate, to the enclosing margin.
3. The measure of tangential resistance to the fluid at the enclosing margin is $(\lambda_1+\mu_1 v^2)$.
4. The measure of tangential resistance to fluid particles flowing past each other is $(\lambda+\mu.(\partial v)^2)$.
5. The whole of the kinetic energy lost by any particle of the fluid under subheads 3 and 4 is probably converted into heat energy, or, at any rate, lost as kinetic energy.

II. *Open Channels*.—In long uniform open channels at uniform slope,—

6. The discharge is one half of the discharge of a pipe flowing full whose lower half is the open channel in question, and upper half is similar to the lower (reversed).

The evidence as to the reality of the assumptions Nos. 1, 2, 5 is not stated: they are apparently only working hypotheses. No. 3 is accepted from Poncelet's experiments on the friction of a fluid on a solid. No. 4 is merely an assumption that the expression for fluid-friction is of the same functional form as that between a fluid and a solid. No. 6 is accepted from Darcy's experiments on pipes and open channels.

It must be remarked, in passing, that there is really no evidence as to the existence of steady motion in parallel lines even in pipes flowing full, and that in the case of open chan-

* Communicated by the Author.

† Vol. xlii. 1871, pp. 184 and 349; vol. xliv. 1872, p. 30.

nels, at any rate, all modern experiment points* to the conclusion that the motion is *essentially unsteady* and that the *stream-lines interlace freely in all directions.* Next, hypothesis No. 2 is contrary to the evidence of Bazin's experiments. In Bazin's work two diagrams are given† of cross sections of two rectangular iron pipes (flowing full) laid with two sides horizontal, with velocity-measurements made at many points on several horizontal and several vertical lines thereof figured thereon: now the contours of loci of points of equal velocity traced thereon are all rounded (like ellipses); so that hypothesis No. 2 disagrees with nature.

From these hypotheses Nos. 1 to 5, the primary result is an expression for the *velocity* at any point in a pipe flowing full; and thence follows, by a simple integration, an expression for the *discharge* through the same. By help of No. 6 (which is a result of pure experiment) this last result is extended so as to give a formula for *discharge* in open channels.

It will be seen that the results for pipes (flowing full) are strictly dependent on the hypotheses Nos. 1 and 2 of steady motion in parallel lines, and of the surfaces of equal velocity being similar and similarly situate to the enclosing margin; whilst the formula for discharge in open channels involves no assumptions as to the actual motion therein, but depends only on the correctness of the theory of the motion in pipes flowing full together with the connecting link No. 6 (derived from experiment).

From the uncertainty of the assumptions, it is evident that the only test of the correctness of the theory is the comparison of numerical results therefrom with observation.

The extensive *small-scale* experimental results‡ of MM. Darcy and Bazin on pipes and open channels not exceeding 2 metres in width nor $\frac{1}{2}$ metre in depth were applied (in the original essay) in numerical verification of the formulæ quoted —viz. for *Velocity* at any part of a circular pipe flowing full, and for *Discharge* both in pipes flowing full and in open channels. The result was *apparently satisfactory*; that is to say, there was certainly a remarkable approximation on the whole (with occasional very large discrepancies) between the experimental and theoretical results. A sound theory of flow of

* See 'Roorkee Hydraulic Experiments,' vol. i. chap. vi., for a summary of the evidence.

† *Recherches expérimentales sur l'écoulement de l'eau dans les canaux découverts*, pl. xviii.

‡ *Recherches expérimentales relatives au mouvement de l'eau dans les tuyaux*, by H. Darcy. Paris, 1857.

Recherches expérimentales sur l'écoulement de l'eau dans les canaux découverts, by H. Darcy and H. Bazin. Paris, 1865.

water would be of such great scientific interest, and its ultimate result—a good formula for Discharge—would be of such great practical use in engineering, that it is well worth while to thoroughly test this new theory : it will be shown that the approximate agreement above noticed is quite illusory.

The primary result of the investigation is, as stated, a formula for the velocity v at any point at a distance r from the centre of a pipe of radius R flowing full, the central velocity (in the same cross section) being v_0, viz.

$$v = v_0 \cdot \epsilon^{-\gamma r \div R}.$$

Upon different hypotheses, and by a quite different investigation, M. Darcy proposed* the formula

$$v = v_0 \cdot \left\{ 1 - m \left(\frac{r}{R} \right)^{\frac{3}{2}} \right\},$$

and applied the same (his own) experimental results in verification.

It is quite clear that these two expressions, differing so greatly *in form*, cannot both be correct rational formulæ; and yet they both give numerical results agreeing sufficiently with Darcy's experimental results to have satisfied their proposers. But the fact is, that the numerical test relied on from Darcy's experiments is (though this seems to have escaped attention) a very poor one. Darcy's velocity-measurements were made at only five points in a vertical line through the centre of each pipe, viz. at the centre and at points symmetrically above and below the centre at $\frac{1}{3}$ and $\frac{2}{3}$ of the radius from the centre ; thus embracing only the middle $\frac{2}{3}$ of the diameter in question, within which the change of velocity is very small, and the *velocity-curve* (or locus of the equation) is therefore very flat. Thus almost any very flat curve would agree tolerably well with the observations in the middle $\frac{2}{3}$ of the diameter, especially when the comparison is made (as in the present instance) between ordinates measured in so large a unit as a metre, as the difference (in metres) would then only be small decimals. The dissimilarity of the two curves is in this case very striking, their convexities being actually *turned opposite ways*. Thus Moseley's curve is concave downstream with a cusp at the middle, whilst Darcy's is convex downstream with an apse at the middle. On plotting Darcy's *observations* they will be found to give curves generally very flat, convex downstream with an apse at the middle; so that Moseley's velocity-formula does not agree with nature.

All the rest of the investigation depends on this primary

* *Recherches expérimentales* &c., by H. Darcy, p. 128.

velocity-formula, and therefore fails as a *rational theory* along with it. It might, however, happen that the other results might prove to be good empirical formulæ. The only others admitting of test are the formulæ for Discharge, which are given in several forms, as suited for pipes of various sections (circular, rectangular, &c.) flowing full and for open channels. A large number of the discharge-measurements of MM. Darcy and Bazin in circular pipes flowing full and in small rectangular and trapezoidal open channels, are compared in the original essay with the results computed from the formulæ; and here, again, there is a remarkable approximation in the results. This turns out, however, to be no criterion of approximation on the large scale, as will appear below.

Some extensive *large-scale* experiments on flow of water in large canals of various widths up to 200′ and depths up to 11′, and various discharges up to 7000 *cubic feet per second*, made under the author's superintendence near Roorkee in Northern India, in 1874–79, and recently published*, afford a test for the formula for discharge in open channels on the large scale. The formula given is

$$\left.\begin{array}{l}\text{Discharge } \textit{in cubic}\\ \textit{metres per second}\end{array}\right\} \quad D' = \tfrac{1}{2}\frac{B'}{R'}\cdot\{1-(1+2R')\cdot\epsilon^{-2R'}\}\cdot v'_0,$$

where

B' = wetted border, R' = hydraulic mean depth, both *in metres*;
v'_0 = central surface-velocity *in metres per second*;
D' = discharge *in cubic metres per second*.

The formula is a very laborious one for computation; but its failure on the large scale can be readily shown, without complete computation, as follows:—It is obvious that the quantity $(1+2R')\cdot\epsilon^{-2R'}$ is always a + quantity, rapidly decreasing as R' increases: hence the above expression gives a result always *less than* $\frac{1}{2}\frac{B'}{R'}\cdot v'_0$ *cubic metres per second*, and therefore (changing to British measures) the

$$\left.\begin{array}{l}\text{Discharge, } \textit{in cubic}\\ \textit{feet per second}\end{array}\right\} \quad D \text{ is always } < \frac{(3{\cdot}281)^3}{2}\times\frac{B}{R}\times\frac{v_0}{3{\cdot}281},$$

$$\text{or } < 5{\cdot}382\,\frac{B}{R}\cdot v_0,$$

where B, R, v_0 are here measured *in feet*.

* Roorkee Hydraulic Experiments, 1881, 3 vols. (published at Thomason C. E. College, Roorkee, N.W. P., India, 1881).

A few selected instances from the Roorkee experiments*, with widely differing data (as to width, depth, velocity, and discharge), are given in the table below.

Site.	Data.			Value of $5{\cdot}382\,\frac{B}{R}v_0$.	Discharge-measurement
	B.	R.	v_0.		D.
Solání left aqueduct	100·2	6·75	4·05	324	2328
Solání right aqueduct	105·8	7·96	4·52	323	3429
Solání embankment, main site...	190·8	9·34	4·66	512	7169
Fifteenth mile, new site	182·0	9·49	4·91	507	7187
Belra site	196·4	9·02	3·67	430	5611
Jaolí site	200·2	7·82	3·43	473	4631
Kamhera site	69·5	4·84	3·40	263	961

It will be seen that the computed values of $5{\cdot}382\,\frac{B}{R}v_0$, which *should be all greater than* the discharge-measurements (D), are actually only from about $\frac{1}{4}$ to $\frac{1}{14}$ of the latter; so that the formula is evidently useless for application to large bodies of water.

The Roorkee experiments also furnish† important (negative) evidence on the vexed question of the supposed convexity of the water-surface in an open channel. Canon Moseley states (in the same essay‡) that, as a result of theory,—

> "As the pressure is everywhere less where the velocity is greater, it is evident that there will be a tendency in the liquid on the surface to flow from the sides of the channel towards the centre, and that thus the velocity of the surface-water at the centre will be diminished, and the water heaped up, drowning, as it were, the point of greatest velocity in the section."

Now two of the results above indicated—viz. the constant transference of surface-water § from the edges towards the centre and the depression‖ of the maximum velocity below the surface—have been amply verified experimentally. But as to the "heaping-up" of the surface-water at the centre, or, in other words, as to the surface-convexity (the existence of which seems to be accepted by writers on hydraulics), the author has not been able to find any certain experimental evidence. Some special experiments were accordingly made by him to test the point, at a site *extremely favourable* for the trial, in a canal of about 170′ surface-width and 11′ depth, with a central sur-

* Roorkee Hydraulic Experiments, vol. i. p. 297.
† *Op. cit.* vol. i. chap. viii. *passim.*
‡ Philosophical Magazine, [4] vol. xliv. p. 44.
§ Roorkee Hydraulic Experiments, vol. i. chap. xvii. arts. 14–14 *b.*
‖ *Op. cit.* vol. i. chap. xii. *passim.*

face-velocity of about 4·5 *feet per second*. The experiment (fully detailed in the Roorkee work*) was a very delicate one, on account of the incessant slight oscillations of the water-surface; but every possible care was taken. The result of twelve trials on one day and twenty-four on another, was that the mean water-surface (*i. e.* average of the oscillations) at the centre and edges was (on a calm day) *most probably level*.

XII. *On the Influence of Time on the Change in the Resistance of the Carbon Disk of* Edison's *Tasimeter*. *By* T. C. Mendenhall, *Columbus, O.*†

ABOUT five years ago Edison announced the discovery of the remarkable property possessed by carbon when prepared in a special manner, in virtue of which its electrical resistance was greatly lessened by subjecting it to an increase of pressure. Among the numerous interesting applications of this discovery which were quickly made, none was more promising or more interesting than the tasimeter devised by Edison himself. The extreme sensitiveness of the carbon to the slightest changes in pressure gave rise to the hope that the instrument would far exceed in delicacy those previously in use for the detection of minute quantities of heat.

Mr. Edison was a member of the Draper Eclipse Expedition in the summer of 1878, and used his tasimeter during the total eclipse of July 29 in that year, attempting to measure the heat emitted by the sun's corona. His report to the director, Dr. Henry Draper, was published in the Proceedings of the American Association for the Advancement of Science for the same year. This report shows that the attempt was by no means as successful as could have been desired, the principal obstacle being apparently the difficulty in the adjustment of the tasimeter so that the galvanometer-needle would remain at zero, and to secure its return to that point after it had been deflected. In fact, the zero adjustment was only made by the use of a peculiar shunt of variable resistance ingeniously contrived by Mr. Edison for the purpose.

The writer is not aware of any other systematic attempt to secure quantitative results through its use; and, as far as known, the instrument has been generally regarded as peculiarly inconstant and unreliable in its indications.

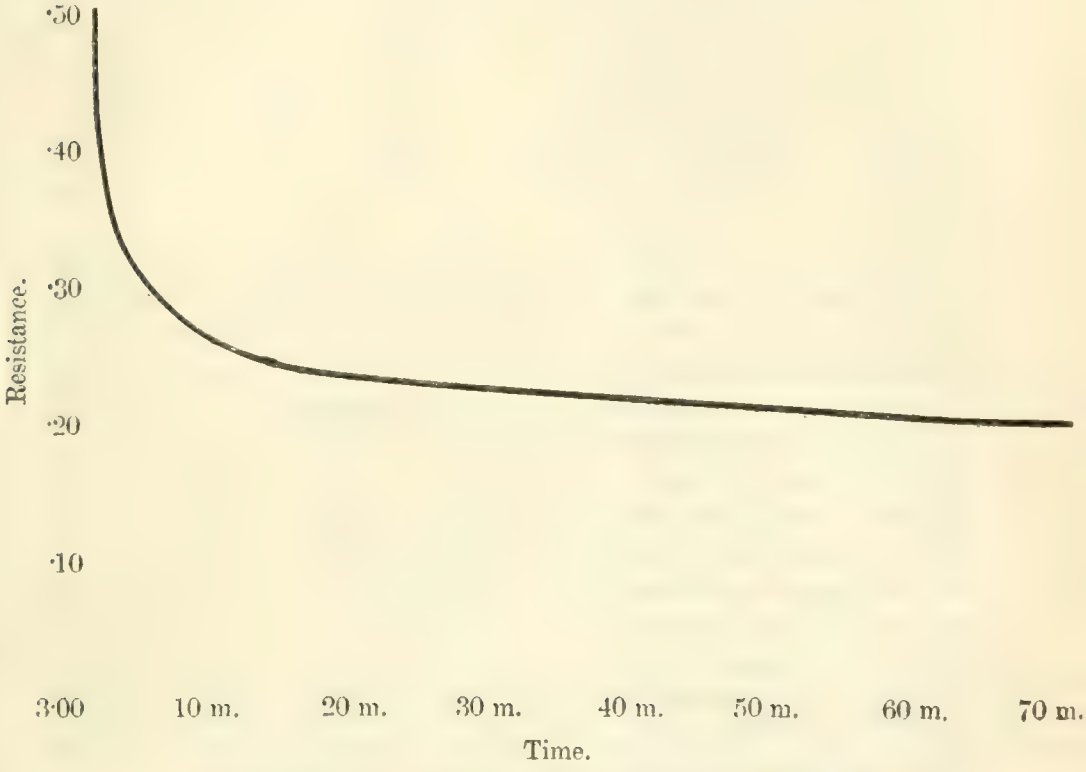
Having in his possession a tasimeter constructed after the

* *Op. cit.* vol. i. chap. viii. arts. 4, 4 *a*.

† From Silliman's American Journal of Science for July 1882, having been read, by invitation, at the April meeting of the National Academy.

model of that described in the report referred to above, the writer undertook a short time ago to investigate the quantitative relation between pressure and resistance for the carbon disk which belonged to it. In a series of preliminary experiments, the use of the toothed wheel and screw, by means of which the pressure is communicated to the disk, was found to be extremely objectionable on account of the impossibility of exactly reproducing a given pressure. This portion of the instrument was therefore entirely removed, and an arrangement made by means of which any definite pressure might be quickly brought to bear upon the disk or removed from it. A slender brass rod was placed in a vertical position upon the centre of the upper contact piece, the upper end of which rested lightly in a small conical cavity made on the underside of the scale-pan of a balance. The weight was suspended above by a fine thread passing over a pulley; so that by raising or lowering it, the pressure was applied or removed as was desired. The carbon disk was made one of the branches of a Wheatstone's bridge, as described by Mr. Edison. In lowering the weight, care was taken to make the movement slow enough to avoid any shock to the disk. When the apparatus stood with the weight lifted, the adjustment of the galvanometer to the zero was made without any difficulty, the resistance of the disk appearing to be quite constant. When the pressure was applied, however, the adjustment became very troublesome; and after a few trials it was discovered that *time* was a very important element in the problem. The addition of a pressure of fifty grams reduced the resistance to nearly one fourth of what it was in its normal condition *instantly*; but it was found that the minimum was not reached at once. The resistance continued to fall during the first two or three minutes with considerable rapidity, and after that more slowly. A series of experiments was accordingly undertaken for the investigation of this phenomenon. After a number of trials, the bridge was adjusted so that when the key was closed simultaneously with the application of the pressure the needle of the galvanometer would remain momentarily at zero; for the instantaneous effect of this pressure seemed to be quite constant. In a few seconds, however, the needle began to move, showing that the resistance was diminishing. With this constantly decreasing resistance it was, of course, difficult to obtain balances which were very accurate; but generally one could be obtained within a minute after the application of the pressure, and another a minute or two later, and so on. The operation was repeated many times, and a number of points for the curve shown

below were obtained, which, though necessarily somewhat scattering, were so situated as to render its general form almost certain. In almost every instance, immediately after the removal of the pressure the normal resistance was again measured; and it was found that while time was necessary for the resistance to reach a minimum after the application of the pressure, the disk seemed to recover its maximum normal resistance instantly upon its removal.



Curve showing the relation between Resistance and Time.

After the construction of the curve showing the relation between time and resistance, and on the supposition that it correctly represents that relation, it was easy to know what the adjustment of the bridge should be at the end of any given time; and thus the difficulty of that adjustment disappeared.

When tested in this way, the curve was found to be correct within the errors of experiment. The following table exhibits the resistances after various times, the *instantaneous* resistance being called 100. The resistance before the addition of the pressure of 50 grams was 11·67 ohms, which immediately fell to 3·52 ohms upon the application of the weight.

Time, in minutes.	Resistance.	Time, in minutes.	Resistance.
0	100	15	92·9
1	96·6	20	92·5
2	95·4	25	92·3
3	94·9	30	92·1
4	94·5	35	92·0
5	94·2	40	91·8
6	93·9	50	91·5
7	93·7	60	91·2
8	93·6	70	90·9
9	93·4	80	90·8
10	93·3	90	90·7
12	93·1		

It will be seen that the resistance falls a little more than 3 per cent. in one minute, about 5 per cent. in three minutes, and about 10 per cent. in one and a half hours; and it seems tolerably certain that even then a minimum is not reached. In two or three instances the time of continuous pressure was prolonged to twenty-four hours, the resistance at the end being slightly lower than at any previous reading. Finally, the apparatus was left with the weight applied for one week. No measurements were made during that time; but at the end the resistance was found to be decidedly lower than it was at the end of two hours after the application of the pressure; and it is especially to be noticed that, on the removal of the pressure, the normal resistance of a week before was instantly recovered. In this case the pressure applied was 100 grams. The resistance before the application of the pressure was 11·08 ohms. Upon applying the pressure, it immediately fell to 2·34 ohms. In two hours this had been reduced to 2·10 ohms; and at the end of a week it was 1·93 ohm. Thus in two hours it was reduced by about 10 per cent.; and after one week it was again about 10 per cent. lower.

It appears, therefore, that the element of time plays an important part in the phenomena exhibited by the carbon disk; and it seems highly probable that this has been one of the principal causes, if not the chief cause, contributing to the inconstancy and unreliability of the indications of the tasimeter. The experiments made thus far indicate a fair degree of constancy in its results when this factor is considered. The writer hopes to be able to make further examination concerning the extent to which all the conditions necessary to its use may be controlled.

The resistance of carbon under pressure has been made the

Fig. 1.

Fig. 2

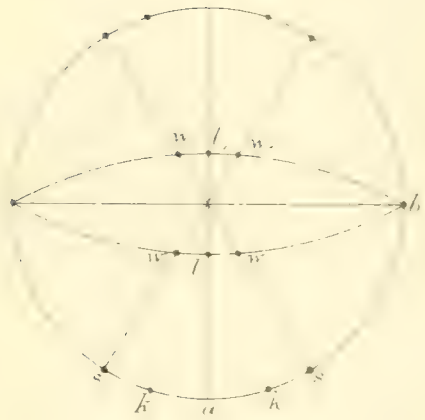


Fig. 3.

Fig. 4

Fig. 5.

Fig. 6.

Quartz.

subject of investigations recently by Mr. Herbert Tomlinson and Professor Silvanus Thompson. The conclusion reached by both is that the diminution of resistance is really due to the contact between the electrodes; and it appears that Professor W. F. Barrett has arrived at a similar conclusion, as a result of experiments made upon a "button of compressed lampblack." Without knowing any thing about the nature of these experiments, the writer desires to record his belief that this theory does not entirely account for the facts stated above. Besides, it seems a little difficult to understand how so small a pressure as fifty grams, added to an already existing pressure of about the same amount, can increase the area of contact between a flat plate and a flat disk nearly four times, to say nothing of the "recovery" which takes place so promptly upon the removal of the pressure.

XIII. *Crystallographic Notes.* *By* W. J. Lewis, *M.A.**

[Plate III.]

PSEUDOBROOKITE.—A pupil of mine, whilst making a list of the apatites in the Brooke collection preparatory to their registration, called my attention to a specimen of asparagus-stone from Jumilla, Murcia, on which were some minute black crystals of apparently rhombic symmetry. They were clearly not hematite, which is frequently found in thin laminæ in the matrix from this locality. The measurements obtained show it to agree well with the mineral discovered by Dr. Koch (Groth's *Zeitschrift f. Krys.* iii. p. 306), which he has called pseudobrookite.

The crystals are very small, and consist of simple prisms with the makrodiagonal pinakoid, strongly striated parallel to their intersections, and terminated by minute bright dome-planes (figs. 1 and 2). They have a considerable tendency to more or less parallel growth; and the opposite planes of the prism are not, as a rule, accurately parallel. They are brittle, and seem to have no good cleavage.

The quantity so far obtained from the specimens in the Cambridge collection is very small (not more than one grain), and it has been impossible to get a chemical analysis made. A preliminary examination by Dr. Hugo Müller confirms, to a certain extent, the belief that it is identical with Koch's pseudobrookite. The following table gives the angles observed and calculated, as also the corresponding angles given by Dr. Koch:—

* Communicated by the Crystallological Society, having been read June 3, 1882.

	Lewis.			Koch.	
	Calculated.	Observed means.		Calculated.	Observed.
a m	25° 49′	*25° 49′ (mean of 5 best)		25° 51′	26° 31′
		25 45½ (mean of 20)			
a l	44 3⅓	.....		44 6	44 6
a e	69 1½	69 2 (mean of 5)		68 50	68 56
e e,	41 57	*41 57 (mean of 6)		42 20	41 19
m e	71 12	71 7½ (mean of 6)			
m e,	108 48	108 42			

The development requires us, in my opinion, to take *m* as (1 1 0). The plane *e* might conveniently have the indices (1 0 3); and the elements would then be:—

System rhombic.

$$D=(0\,1\,0, 0\,1\,1)=29^\circ\ 5'{\cdot}5;\quad E=(0\,0\,1, 1\,0\,1)=48^\circ\ 59'{\cdot}6;$$
$$F=(1\,0\,0, 1\,1\,0)=25^\circ\ 49'.$$

or
$$a : b : c = 1 : 2{\cdot}067 : 1{\cdot}150.$$

The mineral is specially interesting, as, from Dr. Koch's analysis, it seems to be a compound of ferric oxide and titanic acid, and to be therefore a dimorphous form of ilmenite. Its crystal-elements do not approach those of Brookite sufficiently near to justify us in considering it isomorphous with the latter mineral. It offers, therefore, a fresh instance of the peculiar connexion which exists between oxide of iron and oxide of titanium.

Ludlamite.—In the Brooke collection is a specimen with a label in Heuland's handwriting, "Phosphate of Iron on a fossil, Stösgen near Linz on the Rhine, *new*." The phosphate of iron is in minute pale green translucent crystals, having the characteristic three-faced-wedge habit of ludlamite on the free terminations. The best crystals of Cornish ludlamite, as described by Prof. Maskelyne, give angles which vary greatly, owing to more or less parallel growth. The crystals from Stösgen, although half a dozen different crystals have been tried, have such imperfect faces that no measurements have been yet obtained which render a comparison with the ludlamite from Cornwall possible. They seem to have a good cleavage, which, however, manifests the irregular growth of the minute crystals by the indefiniteness of the reflexion obtained from the cleavage-face. They are deposited on the sides of a cavity in the midst of a small mass of greenish-grey matrix, which consists of bundles of coralloid structure containing apparently a quantity of the same substance. I hope during the

course of the long vacation to be able to settle definitely the crystallography of the mineral.

Idocrase.—The Brooke collection contains a small crystal of idocrase, apparently from Zermatt, which has minute planes adjoining the base which do not seem to have been hitherto noticed. They are striated, and the measurements obtained are not good. Using p and $p_{,}$ to denote two of the planes in a zone with c, and $p_{,,}$ one of the others, the measurements obtained were $cp=6°\ 3'$ (best), $cp_{,}=6°\ 19'$, and $pp_{,,}=9°\ 26'$. The form (1 1 7) is that which agrees best with these measurements; the angles required by it being $(0\,0\,1,\ 1\,1\,7)=6°\ 10\frac{1}{2}'$, and $(1\,1\,7,\ 1\,\bar{1}\,7)=8°\ 42\frac{1}{2}'$.

Zoisite.—In the Cambridge collection is a small specimen of zoisite in small bright green crystals imbedded in calcite. One of them had terminal planes; and by careful extraction from the calcite, I obtained a crystal showing four terminal pyramidal faces. The faces were rough, and deeply striated parallel to the edge lying in the brachydiagonal plane, so that the measurements obtained were not good. They agree, however, sufficiently well with those given by Brögger (Groth's *Zeitschrift*, iii. p. 471); and they have led me to the conclusion that Brooke must have been mistaken in giving the angle $wk=56°\ 30'$, an angle which has been assumed in all the mineralogical works to give the elements of the crystal. DesCloizeaux observed a poor dome-plane, of which I seemed to have doubtful indications, and has given a table of angles calculated from Brooke's data. The positions of the faces are shown in the stereographic projection (fig. 3). The angles given by M. DesCloizeaux are compared in the following table with those of Brögger and with those observed and calculated by me.

	Calculated.	Means of observed angles.	Angles adopted by Brögger.	Angles calculated by Dx.
bs	*58° 8′	58° 9½′	*58° 17′	*58° 8′
sk	14 36	14 36		
$ss_{,}$	63 44	63 35	63 26	63 44
bw	*73 9	73 10½ 73 19 (Miller)	73 9	72 28½
sw	56 42	56 52½	56 32½	55 13½
$ww_{,,}$	66 36	66 14	66 55	69 33
aw	62 12½	62 54	.....	61 1
ww_3	55 35	55 5	55 57	57 58
$s_{,}w$	75 56	75 35 (Miller)	75 43	75 23
wk	57 54⅓	.....	.....	*56 30
$ll_{,}$	58 19	60° near (Dx.)	.....	61 3

The plane a is doubtful. The planes k were only measured on one side of the crystal; and a, possibly a result of repetitions of the opposite k planes, was found on the other side. The difference in the angles $w w_{//}$ and $w w_3$ given by Brooke's data and those adopted by me suffice, I think, to justify my belief in an erroneous impression as to the prism-plane which gave the reading 56° 30′. The angles $b s$ and $b w$ are unmistakable, and are those used in determining the elements; the remaining angles found by Brögger and myself agree as nearly as can be expected with those calculated.

Quartz.—In the Cambridge collection are two crystals of quartz, each of which has a well-developed plane whose indices were determined by the late Prof. Miller to be $(50,\bar{1}9,\bar{1}9)$. Professor Miller seems never to have published this result; nor has any record of the measurement which led to it been found amongst his papers. I therefore remeasured the crystals, and obtained an angle which agrees almost exactly with that required by the indices $(50,\bar{1}9,\bar{1}9)$. The larger crystal is a broken prism about 40 millim. long by 8 millim. across. The face y on this crystal is about 1·5 millim. long by about ·75 millim. broad, and is smooth and bright. Near the edge $[by]$ it is slightly rounded. As shown in the diagram (fig. 4), it has to the left a large rough x plane and a long somewhat narrow s plane. The r plane above y is developed so as to all but blot out the other terminal planes, and is traversed by a few horizontal lines, due to repetition of r and some plane in the zone $[ry]$. At the extreme top the plane r is more strongly striated, and is penetrated in a perfectly arbitrary way by small crystals of quartz which are ill developed. In the zone $[z r_{/}]$ are three minute planes, angular measurement of which places them in the position s, l, u. These planes, however, do not succeed one another in this order, but form re-entrant angles, u being that adjacent to $r_{/}$, l next, and s last, and adjacent therefore to $b_{//}$. The planes u and l are strongly striated parallel to the edge $[b_{//} s]$. The planes observed on this crystal are $b\,(2\,1\,\bar{1})$, $r\,(1\,0\,0)$, $z\,(2\,2\,\bar{1})$, $y\,(50\,\bar{1}9\,\bar{1}9)$, $\phi\,(8\,\bar{1}3\,8)$, $s = \alpha\,(4\,\bar{2}\,1)$, $x\,(4\,\bar{2}\,\bar{1})$, $u\,(6,17,\bar{1}2)$ or $(5,14,\bar{1}0)$, $l\,(3\,10\,\bar{6})$.

The smaller crystal is a slender prism about 13 millim. long. The plane y on it is not so well developed as in the former crystal, and it is considerably more rounded near the edge $[by]$. The image, however, given by it is quite distinct. The plane s to the right of y is very largely developed; and x and r appear as very narrow planes below it. The crystal is a combination of $b\,(2\,\bar{1}\,\bar{1})$, $r\,(1\,0\,0)$, $z\,(2\,2\,\bar{1})$, $y\,(50\,\bar{1}9\,\bar{1}9)$, $x\,(4\,\bar{1}\,\bar{2})$, $v\,(16\,\bar{5}\,\bar{8})$, and a plane near ϕ.

The planes observed on these two crystals are given by the stereographic projection (fig. 5).

Mr. Thomas Davies, of the British Museum, lent me a crystal which showed a very prominent plane below the s plane, as also some narrow ones, looking somewhat like striations, between them. The distribution of the faces on this crystal is shown by the stereographic projection (fig. 6). It is a combination of $b\,(2\,\bar{1}\,\bar{1})$, $r\,(1\,0\,0)$, $z\,(2\,2\,1)$, $s=a(4\,1\,\bar{2})$, $g=a(3\,2\,\bar{4})$, $t\,(5\,3\,\bar{6})$, $s'\,(7\,2\,\bar{4})$, $x\,(4\,\bar{1}\,2)$, $\chi\,(\bar{15}\,8\,8)$.

Direct observations of t and s' in the zone $[b_{//}\ s]$ were too little reliable to be of any service. These planes were therefore determined by observations of the angles they make with b and $z_{//}$. The plane s' is somewhat doubtful. The faces w and u are very rounded, and no reliable measurement could be made.

The planes $(50\ \overline{19}\ \overline{19})$, $(3\,2\,\bar{4})$, $(5\,3\,\bar{6})$, $(7\,2\,\bar{4})$ are not given by M. DesCloizeaux; and this is possibly the first time that their existence has been recorded. The following table gives the observed and calculated angles for these planes:—

	Calculated.	Observed.
	° ′	
rr . . .	30 $25\frac{1}{6}$	30 $23\frac{1}{2}$ on the larger crystal. 30 $20\frac{1}{2}$ on the smaller crystal.
$z\chi$ . . .	36 15	36 1
r, s . . .	28 54	28 53
$r, l\ (3\,10\,\bar{6})$	33 47	33 57 not good.
$r, u\ (6\,17\,\overline{12})$	38 9	37 52 „ „
$r, u\ (5\,14\,\overline{10})$	38 29	„
$b_{//}g$ . . .	10 19	10 16
$b_{//}t$ . . .	13 $41\frac{1}{2}$	.....
$b_{//}s_{/}$ . . .	34 33	35 0 near.
bg . . .	52 39	52 43
$bt\ (5\,3\,\bar{6})$.	50 44	50 29
$bs'\ (7\,2\,\bar{4})$.	39 $10\frac{1}{2}$	39 $10\frac{1}{2}$
$z_{//}t$ . . .	30 42	30 $28\frac{1}{2}$
$z_{//}s'$ . . .	27 44	28 0

XIV. *On the Dimensions of a Unit of Magnetism in the Electrostatic System of Measures.* *By* R. Clausius*.

THREE articles in the June number of the Philosophical Magazine, respecting my determination of the dimensions of the electrostatic unit of magnetism, induce me to give some further explanations on the point of view from which I started in that determination.

One of the greatest advances of physical science was Ampère's establishment of the connexion between magnetism and current electricity, and his pointing out that, in respect of its action, a small magnet can be replaced by a small closed electric current, of which the quantity and intensity stand in the following relation to the strength of the magnet:—the product of the intensity and the surface round which the current flows is equal to the moment of the magnet. In consequence of this principle, magnetism need no longer be regarded as a peculiar and separately existing agent, but to the word magnetism a notion can be attached the definition of which is to be drawn from electrodynamics.

But then it is evident that this definition must be such as to correspond with Ampère's proposition universally, and independently of the system of measures employed, and not such that a current and a magnet which are equivalent when one system is employed become of different values on the employment of another. On this condition I have based my determination of the static unit of magnetism.

Now, with respect to Maxwell's treatment of the matter, he has in several passages of his work quoted Ampère's proposition as a correct one, without anywhere adding any limiting remark to the effect that the proposition is to be regarded as valid only in the electrodynamic system, and not in the electrostatic. His units, however, he has determined in such wise that Ampère's proposition is satisfied only in the electrodynamic system, while in the electrostatic the quantities which according to Ampère should be equal are represented by expressions which have different dimensions, and hence their values are changed in quite different ways by an alteration of the fundamental units.

How Maxwell arrived at his formula for the electrostatic unit of magnetism, which deviates from Ampère's proposition, it is impossible to say with certainty, as no explanation about it is given in his work. Nevertheless, as I have already said in my previous paper, it can be inferred from the context that

* Translated from the German MS. communicated by the Author.

the way in which he has brought into the calculation the force acting between an electric current and a magnetic pole determined the derivation of his formula.

If we suppose given an electric current of intensity i passing along a straight line of infinite length, the force exerted by the current upon a magnetic pole m at the distance L of the straight line will be represented by $2im\mathrm{L}^{-1}$, provided that i and m be measured in electrodynamic measure. If, then, each of the three quantities i, m, and L is a unit of the kind of quantity in question, the expression takes the value 2 and represents 2 units of force. Hence, introducing the formula of the unit of mechanical force $[\mathrm{MLT}^{-2}]$, we can write

$$2[im\mathrm{L}^{-1}]=2[\mathrm{MLT}^{-2}],$$

from which follows

$$[im]=[\mathrm{ML}^{2}\mathrm{T}^{-2}],$$

and, if for $[i]$ we substitute $[e\mathrm{T}^{-1}]$,

$$[em]=[\mathrm{MLT}^{-1}].$$

This is the equation of Maxwell's, cited in my preceding paper, in which for $[e]$ he has inserted the expression of the electrostatic unit of electricity in order to get the electrostatic unit of magnetism.

It is, however, to be remarked that in forming this equation the formula of the unit of mechanical force is put for an electrodynamic force, and thereby the equation obtains the character of an electrodynamic equation, into which we must not directly put electrostatic units.

In opposition to this, Mr. Everett says that my equation derived from Ampère's proposition stands, in respect of the property of being electrodynamic, on a par with Maxwell's. But this I must controvert. Ampère's proposition enunciates that the forces exerted by a magnet and by the corresponding electric current are *equal to one another*, but does not represent those forces by any formula. Maxwell's equation, on the contrary, rests, according to the above derivation, upon the introduction of a definite formula for the electrodynamic force, namely the formula of the unit of mechanical force ; and it is this circumstance that makes it an electrodynamic equation, in the sense that it is not suitable for application to electrostatic units, as the determination of the latter rests upon the representation of a quite different force (the electrostatic force exerted by two quantities of electricity upon each other) by the formula of the unit of mechanical force.

In Mr. J. J. Thomson's article another objection appears, in

the words "Mr. W. D. Niven has pointed out to me that the value given by Clausius for the dimensions of a magnetic pole does not make the magnetic force between two such poles of the dimensions of a force, which ought clearly to be the case." I cannot understand this objection. In the *electrodynamic* system of measures the force between two magnetic poles $[m_d]$ is represented by the formula of mechanical force. Now, if in the *electrostatic* system also the force between two magnetic poles $[m_s]$ is to be represented by a formula of the same dimensions, then must $[m_s]$ have the same dimensions as $[m_d]$, which is no more the case with Maxwell than with me, and also cannot be the case; while if instead of $[m_s]$ the quantity is put which I have denoted by v. d. $[m_s]$ (the value of the electrostatic unit of magnetism reduced to electrodynamic measure), the expression of the force between two magnetic poles assumes again the dimensions of the mechanical-force formula. Mr. Niven might have made the same objection against the electrodynamic unit of electricity as he has made against the electrostatic unit of magnetism.

J. J. Thomson raises also an objection of his own. He says that, in determining magnetic force the *magnetic permeability* μ of the medium in which the current is placed must also be taken into account; and then he adds:—"Thus the force between two magnetic poles depends on the medium in which they are placed; but, according to Maxwell, the magnetic force between a current and a magnetic pole does not." In this sentence a distinction is made between the force of a magnetic pole and the magnetic force of a current, which is quite foreign to Ampère's theory. According to that theory a small closed current can completely replace a small magnet with respect to the forces exerted by it upon other magnets or closed currents, so that it is no longer necessary, in order to explain the magnetic actions of a body, to assume the presence in it of a special agent to be designated by the name of magnetism, but instead of this we can suppose that the molecules are encircled by electric currents, and that these exert the actions in question. It is of course contradicting this to say that the magnetic forces proceeding from an electric current act according to other laws than those which govern the forces proceeding from a magnet.

For all these reasons I must continue to maintain that, if physicists accept Ampère's theory (which is certainly done by most physicists, and, as I believe, was done also by Maxwell), they must, to be consistent, accept also the formula determined by me for the electrostatic unit of magnetism, instead of the formula set up by Maxwell.

Fig. 5

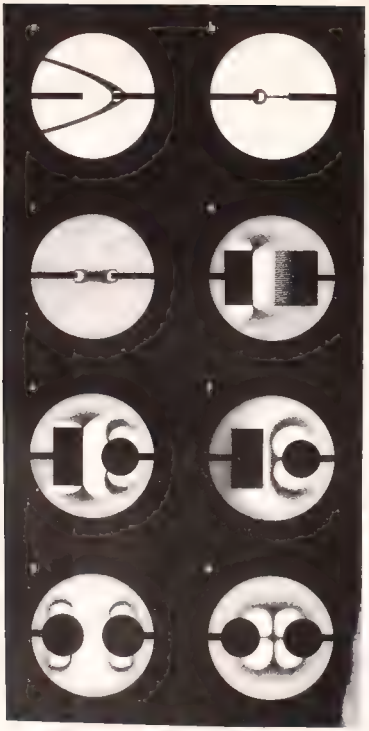
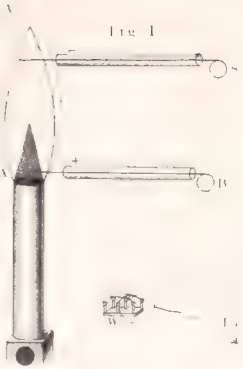


Fig. 1



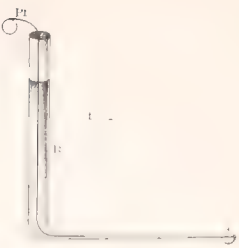


Fig. 2

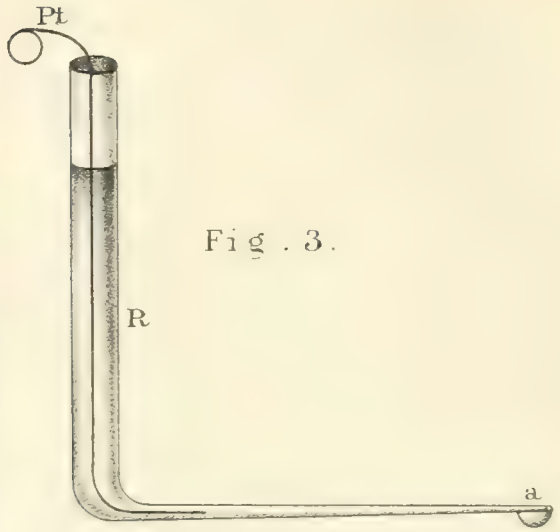
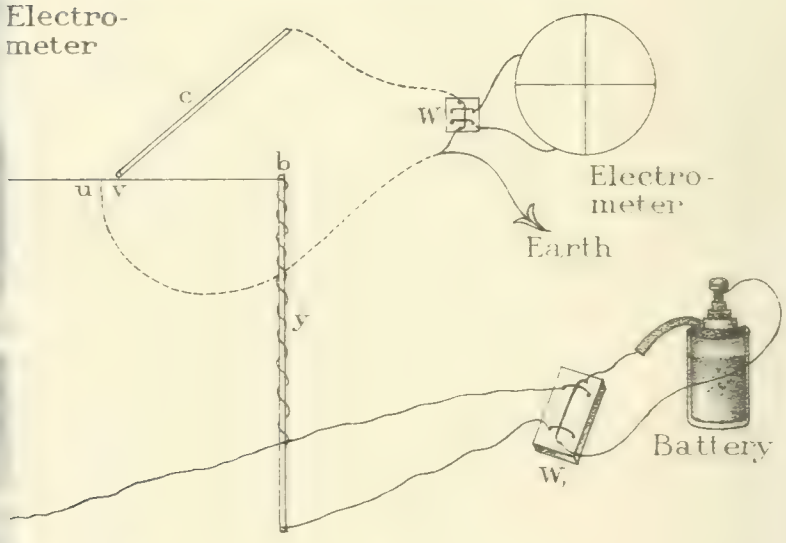
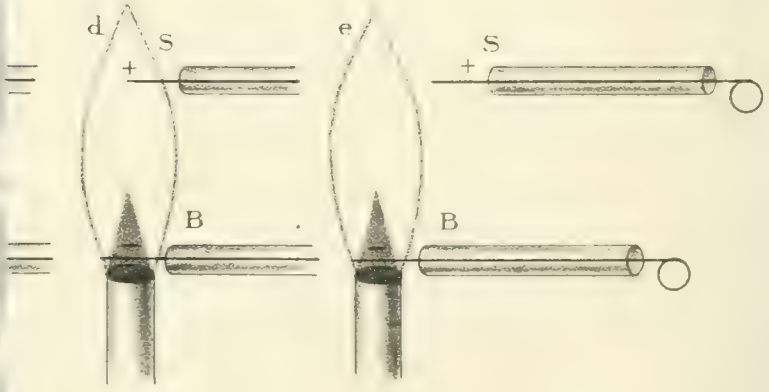


Fig. 3.





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XV. *On Double Refraction, produced by Electrical Influence, in Glass and Bisulphide of Carbon.* By H. Brongersma*.

[Plate IV. fig. 5 *a–h*.]

I.

THE phenomena first observed by Kerr†, and described in his papers entitled "A new Relation between Electricity and Light," I have submitted to a reexamination. The importance of the subject, and the failure of the attempts of various physicists to repeat Kerr's experiments, so far as these relate to solid bodies, have induced me to undertake this investigation.

The willing readiness of the Directors of the "Teyler's Stichting" at Harlem to place an apartment and the necessary instruments at my disposal has made it possible for me to accomplish this labour, which, as I hope, has resulted in proving that the doubt of the correctness of Kerr's results was not well founded.

If I mistake not, Gordon was the first who repeated Kerr's experiments, and, indeed, in spite of all his carefulness, with a negative result‡. In a work published subsequently§, he returns to the subject. After describing the phenomena which he had once observed when the glass was perforated by the spark, he says:—"A fresh glass plate was at once drilled, in hopes of repeating the experiment in the lecture next day; but, *owing to sparks springing round*, we did not succeed in perforating the glass, and therefore saw only the faint return of light described by Dr. Kerr." The words which I have italicized make it in some degree doubtful if the phenomenon observed by Gordon must not be attributed to accidental causes. I have found that a piece of ordinary plate glass is rendered doubly refracting only by the electric spark passing in proximity to it, or when a heated wire is brought near the plate.

Mackenzie‖ also did not succeed in obtaining Kerr's phenomenon. He thought it was produced by heat only.

An investigation by Röntgen¶ likewise gave negative results. He also believes that some accidental influences were at work in those experiments.

* Translated from Wiedemann's *Annalen*, 1882, no. 6, pp. 222–233.
† Phil. Mag. [4] vol. l. p. 337 (1875).
‡ Phil. Mag. [5] vol. ii. p. 203 (1876).
§ A Physical Treatise on Electricity and Magnetism, by J. E. H. Gordon, vol. ii. p. 247.
‖ Wiedemann's *Annalen*, ii. p. 356 (1877). ¶ Ibid. x. p. 77 (1880).

In a copious memoir*, Quincke briefly touches on this part of Kerr's investigation. He says:—"In fact, plate glass and bisulphide of carbon show, according to Dr. Kerr's observations, opposite electric double refractions; in my experiments I found this confirmed for flint glass and bisulphide of carbon." After the many negative results attained by others, it is be regretted that Quincke has not described in detail his method of experiment with glass, as in the other parts of his investigation.

Lastly, Grunmach† did not succeed in observing the phenomenon.

After this I think I ought to communicate my own results, and the more so since, according to them, Kerr himself has not seen these phenomena in their whole extent.

In a piece of common plate glass 14 centim. high, 6 centim. broad, and about 1 centim. thick, two holes about 3 millim. in diameter were drilled coaxially, parallel with the largest lateral surface, so that their ends remained 5 millim. distant from one another. Into each of these openings a small quantity of mercury was introduced, and then thin-drawn-out glass tubes were fixed therein with a mixture of shellac and wax, through which fine copper wires 15 millim. in length were carried till they reached into the mercury. They were then thickly coated to their ends, together with a portion of the glass plate, with the mixture above mentioned. Moreover the plate was varnished, except at the two places which bounded the field of view.

Much care was necessary in doing this, in order to ensure the possibility of obtaining a satisfactory difference of potential. The investigation had to be interrupted several times, partly because the insulation was not sufficient, and partly because the plate was perforated by electric sparks.

The glass plate was set up midway between two nicols 15 and 11 millim. in diameter respectively, so that the incidence of the light-rays was perpendicular, and the space between the wires or electrodes occupied the centre of the field. The light of an albo-carbon lamp (which was placed in a sort of Duboscq lantern), before passing through the polarizing nicol, fell upon a lens, so that the real image of the round aperture of the lantern coincided with the centre of the plate. Afterwards, with the aid of a second lens, mounted in front of the analyzing nicol, a magnified image was observed by the eye; and it was soon found that by this alteration the method was improved. The electric charge was obtained by means of a Holtz machine, with the conductors of which the copper wires were connected.

* Wied. *Ann.* x. p. 537 (1880). † Ibid. xiv. p. 110 (1881).

In the first experiment the planes of polarization of the crossed nicols made an angle of 45° with the horizon (first position). Without a glass plate there was, in the middle of the field, a dark spot; the borders of the field were not completely dark. When the glass plate was inserted, there appeared, principally in consequence of the drilling, phenomena of double refraction as in fig. 5*c*.

Soon after the Holtz machine was brought into action the field changed: first the dark portion between the electrodes divided into two parts; and two dark tails made their appearance, which joined the electrodes to one another above and below, leaving a strongly illuminated space of elliptical form between them. Gradually these tails receded from each other at one of the electrodes, the ellipse approaching more to a parabola. Fig. 5*a* shows the phenomenon as it appeared when the difference of potential was the greatest possible. After about three minutes' powerful working of the machine, the field underwent no further perceptible change; the maximum action had therefore been attained. After a sudden discharge the phenomenon vanished, at first rapidly, and then more slowly; and within three minutes every thing was again as before the experiment.

Having placed before the analyzing nicol a plate of calc spar polished perpendicularly to the axis, I saw, on repeating the experiment, the black cross in the plate change into a hyperbola; so that the light must have been elliptically polarized.

When, instead of the calc-spar plate, I took a vertically compressed glass plate, and slowly increased the pressure, the tails of fig. 5*a* approached one another, at last they assumed the above-mentioned elliptic shape, and with still stronger pressure the phenomenon disappeared.

If the calc-spar plate was now again placed before the analyzing nicol without any pressure taking place upon the compensating glass plate, with increasing difference of potential the black cross, as remarked above, was converted into a hyperbola. By gentle vertical pressure of the compensating glass plate the hyperbola was again converted into the black cross.

If the polarization-planes of the nicols were directed horizontally and vertically (second position), the phenomenon appeared as in fig. 5*b*.

The phenomena of double refraction with the first position were seen by Dr. Kerr incompletely; and he did not succeed in observing those which accompany the second position, perhaps because his field of view was too small to take in the whole. By placing the glass lens in front of the analyzing

prism and mounting the plate movable horizontally and vertically, by which different parts could be successively brought into the field, I succeeded in improving his method. It is, however, very possible that the kind of glass used by Kerr was not without influence upon the result. According to my experiments a piece of very hard English glass, of the same thickness (18 millim.) as that employed by Kerr, becomes strongly doubly refracting in consequence of the drilling. With it the described phenomena were only faintly shown.

Of other glass plates, some, which in consequence of the drilling had become much more strongly doubly refracting than those above mentioned, yielded perceptible but much less distinct results.

The above is besides in complete accordance with the result derived by Kerr from his experiments—namely, that glass under the action of electrical influence behaves like glass which is compressed in the direction of the lines of force.

The difficulties in this investigation are not inconsiderable. For it is necessary to make the difference of potential very great; and then many a glass plate is perforated by sparks. Besides this, it is not easy to insulate sufficiently the conducting connexions. To this must be added that many glass plates become so strongly doubly refracting in consequence of the drilling as to be useless for the investigation. This was the case chiefly with plates whose apertures were drilled with a metallic instead of a diamond drill, presumably in consequence of the great pressure attending the employment of the former. The influence of the nature of the glass I have not yet been able to observe.

Kerr saw in these phenomena a confirmation of Faraday's theory respecting dielectrics. That great physicist already regarded it as probable that under the influence of electricity an isotropic body passes into the anisotropic state, so that it behaves like a doubly refracting crystal. He did not, however, succeed in confirming this by experiment. Still, in my opinion, it is not sufficiently proved that these phenomena cannot be of a secondary order. The motions of the molecules, on their arrangement in a limited portion of the plate, may have for their consequence a development of heat; and this, again, may be the cause of the observed double refraction, as Werner Siemens has already experimentally demonstrated by the heating of the insulating medium of a condenser accompanying the charge and discharge*. I hope that a continued investigation will soon enable me to enunciate a definite view.

* Berl. *Monatsber.* 1864, p. 614.

II.

In his subsequent memoirs* Kerr treats of double refraction of liquids produced by electrical influence. Röntgen has repeated Kerr's experiments†. He made use of a larger nicol than Kerr employed, and by so doing gained the important advantage of being able to have a better view over the whole. The results of the two investigations agreed in the main. Only, with the horizontal and vertical position of the polarization-planes of the nicol, Kerr observed no or only an irregular light-phenomenon; while according to Röntgen's observations the phenomenon with this position of the nicol was complementary to that accompanying the first position. Röntgen, however, appears not to have seen this phenomenon in its entirety, as will be shown subsequently.

In my experiments a square glass jar 5 centim. broad and 9 centim. high, filled with bisulphide of carbon, was employed. In the centres of two parallel sides, apertures of about 3 centim. diameter were made, which were again closed by glass plates 0·2 centim. thick. The jar was set up midway between the nicols, upon an ebonite stand, so that the light-rays fell perpendicularly onto the thin glass plates. Apertures were also made in the centres of the two other sides, to admit the electrodes. The latter were tightly screwed on copper wires, which, inclosed in thin glass tubes, were fixed in the apertures with sealing-wax. Each of these copper wires was connected with one of the conductors of the Holtz machine.

In order to avoid particles of dust, the bisulphide of carbon had to be repeatedly filtered from a bottle into the experiment-vessel and *vice versâ*. Finally the vessel was filled with bisulphide of carbon from a third bottle.

In the first experiment one electrode was a brass disk of 12·7 millim. diameter and 7·8 millim. thickness, the other a sphere of 8·5 millim. diameter. The axis of the disk was horizontal; and its prolongation passed through the centre of the sphere. The polarization-planes of the nicols were constantly perpendicular to one another, and made an angle of 45° with the horizon, and consequently also with the direction of the lines of force in the centre of the field of view. As soon as the machine worked, the phenomenon represented in fig. 5*e*, as nearly as possible, was observed. The middle of the field was brightly illuminated, and most brightly in the immediate vicinity of the electrodes, which is not given in the figure.

* Phil. Mag. [4] l. p. 446 (1875); [5] viii. pp. 85, 229 (1879); [5] ix. p. 157 (1880).

† Wied. *Ann.* x. p. 80 (1880).

Two black tails issue from the sphere, at points the radii belonging to which make an angle of 90°, which is bisected by the axis of the disk. Two other black tails issue from the disk, whose directions at the beginning likewise form an angle of 45° with the horizon. A glass plate placed before the analyzing nicol, upon the centre of which the rays fell perpendicularly, had, with an extremely gentle horizontal pressure, the following influence upon the phenomenon:—With slowly increasing difference of potential, first a small black bow, nearly in the form of a semicircle, with its centre in the point of the surface of the spherical electrode where the prolongation of the disk-axis cut that surface, was observed. Further, this bow divided into two branches connecting two points of the sphere with two points of the disk. These branches receded constantly further from one another till at length fig. 5*e* again came into view. If the glass plate was pressed somewhat more forcibly, the two first-mentioned phases of the phenomenon, and with still stronger pressure the first only, namely the black bow, were observed. If the pressure was still further enhanced, the entire field remained uniformly illuminated, even when the potential-difference was the greatest possible. If, with a great potential-difference, the horizontal pressure upon the glass plate is slowly increased, the same phenomena follow in reverse order, from fig. 5*e* to the above-described black bow, which is seen to become slowly smaller till at last it vanishes.

If on the repetition of the experiment without the compressed glass plate the potential-difference is slowly increased, the same transitions can be remarked, as it appeared to me, as were observed when a gently compressed plate was employed; only the first transitions were slight and less distinctly marked, so that already with a proportionally slight difference of potential the phenomenon is seen as in fig. 5*e*, at first faint, but gradually coming out sharp and clear. If the origin of the black tails at the sphere be joined to its centre, the angle which the lines thereby formed make with the horizon, on the potential-difference diminishing and without a horizontally compressed glass plate, or with the potential-difference constant and with increasing horizontal pressure upon the glass plate, becomes smaller; but in no case, not even with the greatest possible potential-difference, does this angle become greater than 45°.

If the glass plate was exposed to a vertical pressure, with increasing pressure the tails issuing from the sphere and those issuing from the disk removed further from one another, so that the illuminated intervals at the sides of the electrodes

became smaller (as in fig. 5 *e*). At last these tails were completely squeezed against the electrodes; and with still stronger pressure they disappeared. With a greater difference of potential they emerge again, to again vanish in consequence of stronger pressure. Both with horizontal and with vertical pressure of the glass plate, a slight difference of potential with gentle pressure has the same effect as a great potential-difference with stronger pressure. The latter agrees with what was found by Röntgen.

When I substituted for the compressed glass plate a plate of calc spar cut perpendicularly to the axis, I convinced myself that here also the light was elliptically polarized. Before the machine worked, the coloured rings with the black cross appeared. When the machine commenced working, the latter changed into a hyperbola; the ends of the two branches of the hyperbola receded further from each other as the potential-difference increased.

If the polarization-planes of the nicols were brought into a horizontal and a vertical position, the phenomenon of fig. 5 *f* showed itself already at a slight potential-difference; at a greater difference it became sharp and distinct. With this position Kerr did not obtain any evident results, probably because in his experiments the field of view was too small for him to see the whole of the phenomenon. But how it was that Röntgen* saw only the black horizontal line that issues from the sphere is so much the more inexplicable, as he with different electrodes, and with vessels of different widths, always found the same results. I have repeated this experiment with electrodes corresponding exactly in dimensions with those employed by Röntgen. Yet this did not alter the phenomenon, any more than an alteration of the distance of the electrodes from 2·7 to 5 millim. In frequent repetitions of this experiment it was always seen by me unchanged; and even the results obtained with two spherical electrodes agree with this, as will be subsequently shown.

With horizontal and vertical pressure upon the glass plate while it again occupied the previously mentioned position, the phenomenon was unchanged if the glass was placed so that the black cross was midway between the electrodes.

When the direction of the pressure upon the glass plate made an angle of 45° with the horizon, the figure was unsymmetrical. This is also the case when the nicols are gradually moved out of the second into the first position. The figures then form a transition between fig. 5 *e* and fig. 5 *f*. The latter also accords with Röntgen's results.

* See fig 5 of his paper.

In a further experiment the electrodes were brass balls of 8·5 millim. diameter. With the nicols in the first position fig. 5*g* appeared; in the second, fig. 5*h*. Horizontal and vertical pressure upon the glass plate had the same effect as in the first experiment. I obtained completely accordant phenomena also with spherical electrodes of larger dimensions. If one electrode was a sphere of 14 millim. and the other a sphere of 8·5 millim. diameter, this made just as little difference.

At first I thought that a movement of the electricity from one of the electrodes to the other would have a great influence upon the phenomenon. This induced me to make the following experiments.

Two cylindrical electrodes were placed coaxially. One of their opposed surfaces was furnished with fine points. Fig. 5*d* shows the phenomenon which was observed with the nicols in the first position. That which appeared with the second position was very similar to fig. 5*f*.

In the next experiment two thermometers whose spherical bulbs had an external diameter of 13 millim. were employed as electrodes, their mercury being conductively connected with the Holtz machine: thereby neither a dark nor a spark-discharge could take place through the liquid; so that the potential-difference could be raised very high. The striking-distance of the machine was now as great as when the condensers were not connected to the electrodes. The phenomena observed, both with the first and with the second position of the nicols, agreed with those obtained with spherical electrodes (figs. 5*g* and 5*h*). Only with the first position of the nicols was the field illuminated between the electrodes, while the black tails at the sides of the electrodes were much less distinctly visible. With the second position such a difference was not to be perceived. On repeating this experiment with spherical copper electrodes completely inclosed in glass tubes, I obtained the same results.

Further, it is noteworthy that the liquid, which in the experiments with uncovered electrodes was constantly in motion, now remained at rest. Only during a few seconds, as the machine was beginning to work, were very delicate undulations to be seen when the polarization-planes of the nicols formed an angle of about 90° with one another, so that the field of view was feebly illuminated previously.

That with Kerr's and Röntgen's method of experiment a dark discharge takes place through the liquid was ascertained by both physicists; and they deem this fact not unimportant for the explanation of the phenomena. From the experiments

just mentioned, however, it is evident that such a discharge is not requisite, any more than a violent motion of the liquid.

That view would also be untenable for this reason, because we have here to do with elliptically polarized, and not with depolarized light.

If it is further shown that explanations which rest upon the hypothesis that electricity may produce these phenomena indirectly, are not confirmed by an experimental investigation, it becomes more and more probable that we have to do here with a hitherto unknown action of electricity on the light-undulations; and then Kerr's phenomena, as Röntgen justly says, acquire an extraordinarily fundamental significance.

I hope soon to touch this subject again.

Harlem, March 1882.

XVI. *Notices respecting New Books.*

(i) *Mathematical Papers by* William Kingdon Clifford. *Edited by* Robert Tucker, *with an Introduction by* H. J. Stephen Smith. London: Macmillan and Co. 1882. (8vo, pp. lxx, 658.)

(ii) *Mathematical Fragments, being Facsimiles of his unfinished Papers relating to the Theory of Graphs, by the late* W. K. Clifford. London: Macmillan and Co. (Fol., pp. 22.)

TO those who remember the late W. K. Clifford's appearance as a rising mathematician of singularly brilliant promise, one likely to extend the limits of knowledge in any of the various fields of Mathematics to which he might be led to apply his powers, his short career of little more than a decade would seem a mere dream, but for the substantial evidence of his energy and productiveness contained in the thick volume of more than six hundred octavo pages (one third consisting of matter in close type) which we have here to notice. And it is to be borne in mind that the publication of this volume has been preceded by the collection of two volumes of Lectures and Essays produced subsequently to Clifford's removal from Cambridge to London in 1871. The former publication (of more popular matter, but handled invariably with characteristic scientific precision and freshness), together with the collection which forms the subject of the present notice, may interest (and they are deeply interesting) from either of two points of view,—one of which has occupied the pen of Prof. H. J. S. Smith in a masterly Introduction; and the other will be more particularly here dwelt on—the study of the growth of a singularly gifted genius as it derived fresh aliment from extended acquaintance with the work and speculations of kindred minds, and visibly waxed stronger and stronger in its own powers by their exercise on problems of ever increasing height and subtlety.

The absence of any kind of arrangement of these Papers with reference either to subject or date is much to be regretted—"tantum series juncturaque pollet." For the absence of chronological order an apology is offered in the Preface; and some amends are made by a chronological Table therein drawn up, which rather serves to show that the apology might have been rendered needless, as far as any difficulty in assigning, with at least approximate accuracy, their dates to the few doubtful papers. At all events, the chronological order of the greater part is clear of doubt; and had this order been followed, the effect, on a reader able to follow intelligently and appreciatively the matter of the Papers, would have been a sense of harmonious development of an almost unique genius among the English mathematicians of his generation.

Thus, in the earliest papers (contributed to the three leading mathematical serials of this country, and covering the period from 1863 to the completion of his undergraduate career, with the next year or two) reference is made to the higher geometrical text-books only, which occupy the attention of the candidate for a good place on the Tripos: they contain no strikingly new results, but rather reproduce, with extensions and novelties of algorithm, results already known. These papers are of an exclusively geometrical character.

During the three or four years intervening between taking his degree and his removal to London (1871) his mathematical communications were made to the Cambridge University Philosophical, and London Mathematical Societies, or to Section A of the annual British Association Meeting—the one exception being a geometrical paper commenced in the 'Messenger,' October 1869 (erroneously assigned to 1870 in the chronological list), in which he urges the greater cultivation of the methods of Synthetic, or Organic, Geometry; and, drolly enough, makes a reference (no doubt second-hand) to St. Thomas Aquinas, of all authorities on a geometrical question! Among these papers occur two purely analytical:—the "Proof that every Rational Equation has a root" (which attracted much attention at the time), and the closely connected (though separated by some 140 pages in the reprint) "Case of Evaporation in the Order of a Resultant." In both these papers the subjects are treated with a masterly conciseness. The "Lecture Notes," drawn up, as we are informed, for tutorial lectures at Trinity College, Cambridge, in 1870, show a gradually extended course of reading. They commence with the emphatic words "Geometry is a physical science" (probably adopted from Mill, 'Logic,' ch. xxiv.); and in them Riemann is for the first time quoted—a mathematician whose short but brilliant career of Professor Extraordinary at Göttingen had terminated in 1866, after a few years of struggle against the disease which was sapping his life, as, a few years later, it was to lay Clifford in his grave. There is pervading these "Notes" a strong flavour of Hankel's *Vorlesungen über die complexen Zahlen* (1867), which probably accounts for the references to Riemann, Gauss, Argand (otherwise a name almost unknown to mathema-

ticians of the present generation, but whom Hankel characterizes as the true founder of the method of representing a complex geometrically), Cauchy, and Grassmann.

The years 1871, 1872 produced only five short Mathematical Papers, the most considerable being an extension to tridimensional space of a plane theorem of the late Mr. Cotterill's. In the Editorial note to the first of these (p. 234) "nodal *conic*" should be "nodal *cone*." In 1873 appear evidences—from his translation of Riemann's Essay "on the Hypotheses which lie at the basis of Geometry," and the "Preliminary Sketch of Biquaternions"—of Clifford's thoughts having been directed to speculation on hyper-dimensional space, a subject reverted to in a succession of papers down to the close of his career—the last being the unfinished "Classification of Loci" (Phil. Trans. 1878). On the whole of this subject Prof. H. J. S. Smith's masterly analysis will be the reader's best guide. To the same year belongs the first of his two contributions to the Phil. Trans., "On Mr. Spottiswoode's Contact Problems." The year 1874, though one of great activity in other scientific directions, produced no published mathematical papers; the abstracts of his communications to the British-Association Meeting showing that Clifford took his part in the Peaucellier "revival" of the period, and in following up the idea of bringing Chemical Equations under a general formula.

The published mathematical papers of the next three years were contributions to the Mathematical Society's 'Proceedings,' those which attracted most attention being:—(i.) "On the Transformation of Elliptic Functions" (1875)—suggested by a paper of Dr. Lüroth's in the *Mathematische Annalen*, Bd. I.,—followed by "Notes" thereon (1876), in which Darboux's priority both in matter and method on certain points is acknowledged and the geometric proof of the transformation-formulæ is restated and completed; (ii.) "On the Canonical Form and Dissection of a Riemann's Surface"—a most characteristic example of Clifford's powers in its kind. But these three years produced many papers left incomplete by the author and now printed as left. Some, which are mere fragments, were apparently the commencements of papers to be offered to the Societies or Mathematical Journals; others are more probably compilations to form the bases of courses of Lectures planned, but destined never to be carried out. A singular and doubly melancholy contribution to the volume is the late Miss Watson's series of notes of the course of Lectures on Quaternions which, as a student of University College, London, she attended. This was probably the first instance of public instruction in Quaternions offered in England. In Edinburgh it has been for some years a recognized subject in the Mathematical Curriculum, having been introduced by the late Prof. Kelland and by Prof. Tait—the most skilled adept in the method among English mathematicians, and whose elementary Treatise has been the source whence most of the younger mathematicians who have

paid attention to the method have derived their first knowledge of its principles. It is one of the losses which University College, London, has most to lament in Clifford's death, that with him has apparently died the effort to establish the elements of Quaternions in its Mathematical programme. Strangely enough, in France, too, the most flourishing School of Quaternions is not to be found in Paris, but in the provincial Faculty of Bordeaux. Apart from the interest of their authorship, it might be a question whether Clifford would have wished these Lecture Notes to have been published among his own papers. Whatever there is novel in the treatment is probably incorporated in his 'Dynamic;' and a cursory examination of a page or two indicates the necessity of a revision of the Notes by some one conversant with the subject. Thus at p. 502, "a point of no *velocity*" is an error for "no *acceleration*," and the whole paragraph is a corollary to one in the following page: wherein, again, a reference to "equation (2)" should be a reference to another equation not numbered, just above. Such errata as $\dot{O}$ for $\ddot{O}$ are either typographical or consequences of the hurry of note-taking.

Of much more general interest will be the unfinished "Algebraic Introduction to Elliptic Functions," in which, though Prof. Smith finds that it "contains no new results and perhaps no original methods of investigation," the most recent contributions to the subject by Rosenhain, Königsberger, Schröter, Göpel, Cayley, and Smith had been incorporated. This "Introduction" was probably drawn up with immediate reference to his intended course of lectures in University College. The third most considerable paper in the Appendix, "On Power Coordinates in general," should certainly have been preceded by the Notes on the Theory of Powers which follow it. The remarks (p. 555) with which the Editor has introduced these 'Notes' are very unfortunate: if "they are, in places, *apparently* inaccurate" and "it is not *easy* to see how the equations in (1) and (4) are got, nor how the other equation in (1) contains a linear relation between the powers of a point with respect to *a* &c.,"—of course *cela dépend*; but if Mr. Tucker had referred his difficulties in this case, as he did in others, to any of the many mathematicians who would gladly have cleared them up, it would have been pointed out to him that the results are perfectly accurate to a *factor près* (as it might have been expected any such work of Clifford's would be), and that the last equation in (1) has the significance assigned to it. In the last equation but one of (4) as printed there is certainly an erratum of the sign $(-)$ for $(\times)$; but this would probably not be found in the MS.

Of great interest is the collection of Problems and Solutions from the 'Educational Times;' and it appears that there is still a goodly number of Questions proposed by Clifford remaining unanswered, the solutions of some of which would perhaps be found, in substance at least, among the Papers and Fragments now for the first time published.

It would be vain to attempt to convey in a cursory notice any adequate idea of the contents of the thin folio of lithographed facsimiles of the fragments on "Graphs," a subject which formed one of Clifford's communications to the 'Proceedings' of the British Association in 1875, "On the Graphical Representation of Invariants."

To those who are acquainted with Clifford's mathematical papers, it is needless to remark how invigorating, disciplinary, and suggestive the study of them is—and that they are for the most part by no means easy of reading, but, on the contrary, require a good deal of hard work on the part of the reader to cover with shorter steps the long strides with which he gets over his ground. The reader of this collection is deeply beholden to Prof. H. J. S. Smith for the lucid and helpful analysis of the contents with which he has introduced them, and to Professor Cayley for his elucidatory notes to many of the posthumous papers; also to Mr. Spottiswoode, P.R.S., as well for similar assistance as, we believe, for his liberality in undertaking the cost of lithographing the Fragments on Graphs. The labour which Mr. Tucker has contributed as editor it is superfluous to point out; and the circumstances which induced him to undertake so heavy and responsible a charge are explained in the Prefatory Letter. It is the best return we can make an editor for such labour in our behalf, to point out whatever imperfections and errata come under observation, for amendment in the Second Edition, which the interest likely to be taken in this collection may be expected to call for ere long.

[By the Editors' kindness I have been permitted to see the above Review in "proof," and to append to it the few remarks which follow. "Triangular" symmetry, I need hardly say, is Clifford's own title to his paper, though in line 9 he writes, "I call . . . . . rectangular symmetry." In the Bibliography I state that the *lower* date of publication is given: the Reviewer is no doubt aware that alterations are frequently made by authors in their papers in the interval which elapses between composition and publication: I think, but I am open to correction, that the paper (viii.) was not published in its entirety until 1870. The Reviewer implies, in writing of my remarks on p. 555, that I made the arrangement without consulting any one else; but I can assure him that these two papers were submitted to the, I believe, careful consideration of one of my four referees; and it was on his advice that I took the course I adopted and wrote the "very unfortunate" remarks. I am now able to say that my difficulties have been cleared up, or nearly so, by a young mathematician who is working on some of Clifford's lines. For a second edition, if such should be called for, the Reviewer's remarks, and any further ones he may be willing to send me, will be most acceptable.—R. Tucker.]

A Treatise on the Theory of Determinants, with graduated Sets of Exercises for use in Colleges and Schools. By THOMAS MUIR, *M.A.* Macmillan: London, 1882. (Pp. vi+240.)

THE subject of Determinants is every day coming more and more to the front. As evidence of this we may instance the recent works of Mr. Scott and Mr. W. Thomson, and the chapters in Messrs. Burnside and Panton's 'Theory of Equations.' Students are no longer shut up to the advanced works of the masters in the science nor restricted to the small morsels dealt out in two or three of our algebraical textbooks.

In France M. Dostor has recently brought out his *Eléments de la Théorie des Déterminants avec application à l'Algèbre, la Trigonométrie et la Géométrie analytique dans le plan et dans l'espace*; and in this country Mr. Muir, who has done so much good original work, now puts forth the excellent text-book under notice. Without going into any lengthened detail, we may say that the Author does not touch upon the Geometrical applications of Determinants; perhaps he considers that these have been dwelt upon with sufficient fulness elsewhere, and is only anxious to provide his readers with a full Algebraical introductory treatise. The first two chapters dwell at considerable length upon Determinants in general; but the third chapter treats much more concisely of the various forms known as Continuants, Alternants, Symmetric and Skew Determinants, Pfaffians and other Determinants. Much of this chapter is the Author's own work; and the whole of it is very suggestive in its treatment. There is apparently much of discovery still in store for the careful worker in this corner of the mathematical field. Ample practice is furnished for the reader in a capital collection of Exercises, to which answers are appended at the end of the book. The fourth chapter contains a slight historical sketch: this is interesting; but we are inclined to regret that regard to space prevented Mr. Muir from extending it. However, he has made some amends by the publication in the 'Quarterly Journal of Mathematics' (Oct. 1881) of a "List of Writings on Determinants" (1693 to 1880), which was originally drawn up for the present work. We have noticed only a few errata and a few obscurities of expression. These can be removed in a second edition, which will no doubt be soon called for. Could not the work then be printed in a form better adapted for the numerous lengthy formulæ?

XVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

May 10, 1882.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Relations of *Hybocrinus*, *Baerocrinus*, and *Hybocystites*." By P. Herbert Carpenter, Esq., M.A.

2. "On the Madreporaria of the Inferior Oolite of the neighbourhood of Cheltenham and Gloucester." By R. F. Tomes, Esq., F.G.S.

3. "On the Exploration of two Caves in the neighbourhood of Tenby." By Ernest L. Jones, Esq.

4. "Note on the Comparative Specific Gravities of Molten and Solidified Vesuvian Lavas." By H. J. Johnston-Lavis, Esq., F.G.S.

From some experiments made on Vesuvian lava, Prof. Palmieri in 1875 expressed the opinion that its specific gravity, when molten, might be as high as 5·0, though when cooled it is only 2·7. The author described the results of experiments made in December 1881 on some lava flowing across the Atrio del Cavallo. Favourable circumstances enabled him to gain a position above a perfectly molten stream, the surface of which was protected from radiation by the heated walls of a tunnel which the lava had already formed by cooling of the crust. Onto this were dropped, from a height of $1\frac{1}{2}$ yard:—(*a*) light scoria; this floated on the surface until lost to view (the stream could be watched for 150 yards or so); (*b*) fairly solid lava, with some vesicular cavities; this slowly sank, until after some distance it disappeared; (*c*) the most compact lava that could be found, in which, however, were a few small cavities; this sank rapidly, the molten rock welling up round it. The author considered that these experiments demonstrate that the cooled lava is more dense than the molten, and that the apparently contradictory results obtained by Prof. Palmieri were due to the fact that the surface of the stream, by loss of heat, had become viscid, so that the solid material floated, though of greater density. The author concluded by citing other confirmatory evidence of his view.

May 24.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Geology of Costa Rica." By George Attwood, Esq., F.G.S., F.C.S., Assoc. Memb. Inst.C.E.; with an Appendix by W. H. Hudleston, Esq., M.A., F.G.S., F.C.S.

The author commenced his journey at the town of Punta Arenas,

on the Gulf of Nicoya. This stands on a peninsula composed of a calcareous sandstone, covered by a dark sand consisting of quartz grains, magnetite, and decomposed felspar and augite. Inland is an igneous rock which occupies, before long, both banks of the Rio Barranca, and on the left bank extends to the sea; it is a greenstone containing porphyritic crystals of augite and triclinic felspar, and appears to contain too much silica for a true dolerite, being rather a representative of one of the more basic forms of the augite-andesites, resembling, in some respects, specimens from the English lake-district described by the late Mr. Clifton Ward. On this rock, after a time, are found boulders of a black augite-andesite; this appears to be identical with the rock found *in situ* in the Aguacate Mountains. Here are gold- and silver-mines, which were described. In the ravine of the Rio Grande lignites are found. Below this is a series of ancient lakes, which on the Pacific slopes have been tapped by the Rio Grande, on the Atlantic by the Rio Reventazon. Here also the country rock is the greenstone already described; and near Cartago there are boulders of trachyte. The volcano of Irazu is a trachyte, probably a quartz-trachyte, forming an important building-stone. Augite-andesites are found at La Palma, about twelve miles N.W. of the volcano. Irazu, a volcano at present passive, but with blow-holes of gas, is between 11,000 and 12,000 feet in height. Turrialba, of about the same elevation, is still feebly active.

The author is of opinion that the filling of the mineral lodes (ancient fissures) in the Aguacate Mountains took place in Tertiary times, probably Pliocene, and that this infiltration was contemporaneous with the eruption of the augite-andesites in the same region. The quartz-trachytes and sandstones are certainly post-Tertiary.

2. "On a remarkable Dinosaurian Coracoid from the Wealden of Brook in the Isle of Wight, preserved in the Woodwardian Museum of the University of Cambridge, probably referable to *Ornithopsis*." By Prof. H. G. Seeley, F.R.S., F.L.S., F.G.S., &c.

3. "On the Newer Pliocene Period in England." By S. V. Wood, Esq., F.G.S. (Concluding Part.)

In this part the author continued, from the first part of the paper (published in the Journal of the Society for 1880), his examination of the conditions which accompanied the emergence of England during the retreat of the ice of the Chalky Clay, and described the damming-up of the valleys which drain to the Wash by that ice after the water-partings between their systems and those of the Severn and Thames had emerged, whereby the fresh water in these valleys was raised, so as to overflow the minor water-partings within their systems and cover them with gravel, such as that at Casewick, within the Welland system (described by Prof. Morris in vol. ix. of the Journal), and those of Cambridgeshire, described by Mr. Jukes-Browne. He referred the freshwater bed at Casewick, covered by this gravel, and the palæolithic brick-earth of Brandon and Mildenhall (which is overlain as well as underlain by the Chalky Clay) to the time immediately antecedent to this—the slight advance

of the ice which thus blocked up and raised the water-line within the systems of the Welland, Nen, Ouse, and Cam having overridden this brick-earth and covered it with the Chalky Clay.

He then described the gravel (*f* of his figures) of the Thames valley, and showed that it was the continuation of the gravel previously described by him as synchronous with the Chalky Clay, and which, as described by him in the first part of his paper, was *over*lain, and also *under*lain by that clay, it inosculating with those gravels, up the valleys of the Lea and (Middlesex) Colne.

He then described the *Cyrena-fluminalis* formation, which he showed as originating in a depression which raised the water-line in the Thames valley at Grays and Crayford to about 100 feet above the present sea-level, and proportionately higher on the west of London; and described the formation as consisting, at Grays, of four divisions, which in their upward order he called 1, 2, 3, and 4,—No. 1 being the gravel base, No. 2 mostly brick-earth with freshwater shells, No. 3 yellow sand containing freshwater shells in the lower part, but unfossiliferous and false-bedded in the upper, and No. 4, a clay or loam, also unfossiliferous.

These, he showed, are mutually transgressive, both at Grays and at Clacton, No. 3 at Clacton becoming estuarine by the intermixture of marine shells with the *Cyrena*, and No. 4, a loamy gravel which is unfossiliferous, while, from its greater transgression, No. 4 spreads so widely over the gravel *f* that remnants of it occur at Slough, West Drayton, and other places. He then traced the formation northwards in Suffolk; and from the *Cyrena* not being associated there with other than freshwater shells (except at Gedgrave, where the marine shells associated with it are derivatives from the Crag), he inferred that the depression did not bring the sea into Suffolk or East Norfolk. In West Norfolk and around the Wash, however, it did so, the *Cyrena* being associated with the marine gravel at March. The evidences of this depression bringing in the sea around the Wash (which consist of the Nar brickearth and the gravel of Hunstanton, March, and other places in the Fen country with marine shells) extend to about 30 feet elevation. This gravel at Overton, near Peterborough, passes down into a bed with freshwater shells only, thus resembling the Clacton bed, and at March contains the *Cyrena* in abundance. Northwards the formation is represented by the *Cyrena*-gravel of Kelsea Hill, in Holderness: and the evidences of depression rise in that direction to near 100 feet, as a brick-earth, at Kirmington in North Lincolnshire, at between 80 and 90, containing mammalian remains and *Scrobicularia piperata*, with valves united, is regarded by the author as part of the formation; and he instanced the ripple-marked pan beneath this formation at Hessle as evidence of redepresssion or transgression similar to that afforded by the Mollusca at Overton and Clacton. He then described this gravel as extending up the vales of York and Tees to about a similar elevation, and as passing in them, as it does in Holderness, under the clay of the minor glaciation. Southwards he traced the formation as represented by the

shingly sand and gravel of Avisford and Bourne Common in Sussex—the Selsea mud-bed with Lusitanian shells, near the present sea-level, representing the first part of the formation, which the depression carried transgressively to Avisford. In the Thames and lower Lea valleys he described, and showed, by many lines of section, how considerable a denudation accompanied the rise from this depression, so that not only most of the formation but also much of the gravel *f*, of glacial age, on which the uppermost bed of this formation rested, was washed away, the latter having for a great distance been left on an escarpment facing the valley-sides. This denudation, he showed, was in the same places repeated after the formation of the gravel of the minor glaciation.

He then, under another division of the period (which he distinguished as that of the minor glaciation or reindeer age), described the various formations, morainic, atmospheric, fluviatile, and marine, due to a return of glaciation after England had, except in the north-west, become all land. The morainic part in the north-west (which was the Upper Clay of Lancashire and adjoining counties) he regarded as extruded beneath the sea up to that level at which it contains shells, these having been dropped from floe-ice detached from the shores, which drifted over it while thus undergoing extrusion; but in the north-east it was terrestrial, owing to this part having emerged from the depression of the *Cyrena* formation before the moraine reached Holderness, and therefore it contained no shells. The ice giving rise to this moraine was of far less volume than that of the Chalky Clay, and instead of seeking the sea as that did, when the sea lay over the centre and south of England it passed to it in its present position, one stream of it going straight out through the Tees valley, and another down the vale of York and out by the Humber, so as to overspread southern Holderness and the sea-board of Lincolnshire. The fluviatile formation of this minor glaciation is the gravel which overlies the *Cyrena* formation at Crayford and Ilford (Uphallfield) up to the elevation of about 30 feet, and at similar elevation lies up to the foot of that formation at Grays; and it is that which forms the 40–45-foot terrace at Acton, where it has yielded reindeer-remains. Owing to the rise from the depression under which the *Cyrena*-formation accumulated, which had taken place when this gravel was formed, its level does not differ greatly from that of the *fossiliferous* part of the *Cyrena* formation at Grays and Crayford, so that in more inland districts, as at Oxford, the two, though quite different in age, may be undistinguishable. This gravel the author regarded as corresponding in position with the beaches of the buried cliffs of Sangatte, Brighton, Isle of Wight, Portland, and Sili Bay, these beaches and the gravel having originated during a pause in the rise from the depression of the *Cyrena* formation. The floe-ice of this glaciation, driven onto these beaches, left blocks on them, which becoming mixed with loam from rills pouring in summer over the cliffs, is covered by the atmospheric formation which accumulated as, by renewal of emergence, the sea receded from these beaches. At the same time floes grounding on the Pagham

and Selsea flats, which, in correspondence with the shingle of the Isle-of-Wight beach, were then submerged about 30 or 40 feet below their present level, left the great blocks found in the clay-gravel of Pagham and Selsea, which was then forming, and which overlies the mud-bed with Lusitanian shells, and is itself overlain by the atmospheric formation. He also showed by a line of section that this gravel occupies a position several hundred feet below that which the gravel of the great submergence and major glaciation occupies in the adjoining parts of Hampshire.

The atmospheric formation of the minor glaciation he regarded as the brick-earth with angular fragments of stone and splintered flints overlying the buried cliffs and their beaches. This is the "formation of great submergence" (with land shells and Mammalian remains) of Prestwich, and identical with the "warp" of Trimmer and "trail" of Fisher in other parts of England. The origin of this he referred to an annual thawing of the upper layer of the permanently frozen land-surface, such as takes place in arctic countries not occupied by land-ice, such as Siberia. Owing to the subsoil being permanently frozen, no water can penetrate it, so that the thawing surface-layer becomes sludge from the snow-melting and rainfall of summer, and slowly slides from higher to lower places, thus exposing on the higher a continually renewed superficial portion of the permanently frozen soil to this action, and accumulating it in the lower. In sliding, this material has collected not only the bones of animals such as the reindeer and mammoth which lived on this surface, but also those of the hippopotamus, which did not, but had lived during the *Cyrena*-formation stage, from superficial deposits of that stage (from which also they got by derivation into the gravel of this glaciation), in illustration of which he refers to Siberian rivers now receiving the remains of the extinct mammoth and living reindeer alike.

Penetrating fissures in the rocks, this material has formed the amorphous Cave-earth of the districts beyond where the moraine has reached; and the author pointed out that, stalagmite being due to percolation, none could form while the subsoil was thus permanently frozen, which is the reason why the Cave-earth is devoid of it, though always covered by it and sometimes underlain by it, such underlay probably showing that the caves where this occurs were not submerged at the commencement of this minor glaciation.

After giving various reasons which appeared to him to show that the passing away of the minor glaciation took place while Lancashire was still submerged up to an elevation of from 20 to 30 feet, but when the east and south of England was at a somewhat higher level than at present, he described a bed of flattened stones which cover all anterior beds alike in the limestone districts of the south of Lincolnshire, and some gravel with flattened fragments of hard chalk in North Lincolnshire and Holderness, which appear to him to indicate a flooding of the country after the termination of this glaciation. The author then offered some remarks on the coexistence of arboreal vegetation with the land-ice of the first or great glaciation at the

time when it uncovered the plateaux of Norfolk and Suffolk, appealing for the probability of this to the condition of South America, where the inland ice passes in glaciers to the sea in the Straits of Magellan and adjoining channels through dense forests. He also pointed out that the evidences of the Newer Pliocene period, as traced by him, lend no support to the climate-theories of Dr. Croll, Mr. Wallace, or Mr. Murphy, but, on the contrary, conflict with them, as do the respective extensions of the areas of glaciation in Western Europe and Eastern America, while they are equally repugnant to any theory of climate based on changes in geographical conditions; and he concluded by insisting on the British origin of all the ice connected with either glaciation in England, and on the existence of an open north sea throughout.

June 7.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. The President read the following note, forwarded by Don Manuel F. de Castro, Director of the Geological Survey of Spain:—"On the Discovery of Triassic Fossils in the Sierra de Gador. Province of Almeria, Spain."

"The metalliferous limestone of the Sierra de Gador, owing to no fossil remains having been found prior to this occasion, has been a perfect puzzle to all geologists for the last fifty years.

"MM. Maestre, Amar de la Torre, Pernolet, Ansted, and Cooke considered these limestones to belong to the Transition series, the former taking it as a representative of the Mountain Limestones of other parts of Europe. M. Prado hinted that they might be Devonian; whilst M. Willkomm, in the geological map published to accompany his botanical researches in Spain, considered them Silurian. Lately MM. Botella and Vilanova, in their respective maps, have marked them as belonging to the Permian series; whilst M. de Verneuil, coming nearer to the truth, took the whole of the limestones to the south of Granada and the Sierra de Gador as Triassic, though in doubt ("Trias incertain").

"Under these circumstances, I was commissioned by the Director of the Geological Survey of Spain to investigate the S.W. portion of the Province of Almeria, which comprises the Sierra de Gador. In February last I had the good fortune of discovering abundant fossil remains in different parts of the Sierra de Gador, which perfectly fix the age of the metalliferous limestones of this part of Spain.

"The whole series of rocks forming this *sierra*, resting on the mica-schists and slates of the Sierra Nevada, is a succession of black, white, and purple talcose schists at the base, which alternate with some beds of yellowish and porous limestone, and which pass through a considerable thickness of grey limestones and slates, and, precisely where the fossils have been found, to the metalliferous limestone of Sierra de Gador, which appears to form the top of this interesting formation.

"The fossils found belong to the following genera:—*Myophoria*

(*M. lævigata* and *M. Goldfussi*), *Hinnites*, *Monotis*, *Avicula* (*A. Bronni*), *Myacites*, *Rissoa*, and many others difficult to determine.

"The places where the fossils have been found are the following:—on the southern slopes of the Sierra de Gador, in the Rambla del Cañuelo; midway on the road from Felix to Marchal; and in the place named La Solana del Fondon, to the left of the river Andarax, following the track between the mine Sebastopol and the town of El Fondon.

"Joaquin Gonzalo y Xavier."

2. "The Girvan Succession.—Part I. Stratigraphical." By Charles Lapworth, Esq., F.G.S., Professor of Geology in the Mason Science College, Birmingham.

The Lower Palæozoic rocks of the neighbourhood of Girvan, in the south of Ayrshire, have long been famous for the remarkable variety of their petrological features, and for the abundance and beauty of their organic remains; but the strata are so intermingled and confused by faults, folds, and inversions, that it has hitherto been found impossible to give a satisfactory account of the geological structure of the region.

The most remarkable formation in this Girvan area is a massive boulder-conglomerate, several hundreds of feet in thickness, which forms the high ground of Benan Hill, and ranges throughout the district from end to end. Employing this formation as a definite horizon of reference, the author showed, by numerous plans and sections, that it was possible for the geologist to work out the natural order of the strata, both above and below this horizon, and to construct a complete stratigraphical and palæontological scheme of the entire Girvan succession. This succession is composed of the following members, arranged in descending order:—

(I.) Upper Girvan Rocks.

(D.) Dailly Series (1500 to 2000 feet), including the

- (3) *Straiton Group*, consisting of grey flags, shales, and grits, with *Beyrichia Klædeni*, *Cardiola*, &c.
- (2) *Bargany Group*, of pale flagstones, shales, and mudstones, with *Retiolites Geinitzianus*, *Cyrtograptus Grayi*, &c.
- (1) *Penkill Group*, of purple mudstones, grey flags, and greywackes, with *Crossopodia*, *Protovirgularia*, &c.

(C.) Newlands Series (1000 to 1500 feet), embracing the

- (3) *Camregan Group* &c., of yellow thick-bedded grits and dark shales, with a band of calcareous rock; abounding in *Pentamerus oblongus*, *Atrypa reticularis*, *Rastrites maximus*, and *Monograptus Sedgwickii*.
- (2) *Saugh-Hill Group*, composed of alternations of coarse pebbly grit and zones of grey and black shales, with a coarse conglomerate at the base. Its commonest fossils are *Stricklandinia lens*, *Pentamerus oblongus*, *Favosites gothlandicus*, *Monograptus leptotheca*, &c.
- (1) *Mulloch-Hill Group*, formed of shelly sandstones underlain

by a coarse boulder-conglomerate, and containing hosts of Brachiopoda &c., chiefly *Meristella angustifrons*, *Atrypa hemisphærica*, *Nidulites favus*, &c.

(II.) Lower Girvan Rocks.

(B.) Ardmillan Series (1800 to 2000 feet), embracing the

(4) *Drummuck Group* of soft grey mudstones &c., with *Trinucleus seticornis*, *Ampyx*, *Staurocephalus*, *Dicellograptus*, &c.

(3) *Barren or Shalloch Flagstones.*—A great thickness of alternations of grey or green flagstones and shales, generally destitute of fossils.

(2) *Whitehouse Group.*—Purple and green shales and mudstones, striped flagstones and calcareous beds, with *Dionide*, *Dindymene*, *Æglina*, *Agnostus*, *Dictyonema*, *Dicellograptus*, and *Pleurograptus*.

(1) *The Ardwell Group* of dark Graptolitic flagstones and shales, with occasional fossiliferous seams affording examples of *Dicranograptus*, *Leptograptus*, and *Climacograptus*, &c.

(A.) Barr Series (800 to 1000 feet), composed of the

(4) *Balclatchie Beds.*—Highly fossiliferous pebbly grits and nodular shales, with *Lingula Ramsayi*, *L. canadensis*, *Siphonotreta nucula*, *Remopleurides*, *Glossograptus*, &c.

(3) *Benan (or Green) Conglomerate.*—Massive boulder-beds of great thickness, unfossiliferous.

(2) *Stinchar (or Craighead) Limestone Group*, composed of compact limestones, nodular and calcareous flagstones and shales, with *Maclurea Logani*, *Ophileta*, *Orthis confinis*, *Tetradium*, *Didymograptus*, *Clathrograptus*, &c.

(1) *Kirkland (or Purple) Conglomerate.*—Coarse boulder-beds and sandstones, generally of a purple colour.

It was shown that the highest beds of this succession are faulted against strata of Carboniferous age. The discussion of the relationship of its lowest beds to the igneous and metamorphic rocks of Ballantrae was deferred to a future paper. The author pointed out how perfectly this reading of the succession explained the anomalies hitherto supposed to obtain among the fossils of the Girvan region. When the organic remains collected from these strata by previous investigators (notably the magnificent Gray collection) are referred to their natural horizons in this stratigraphical succession, it is found that each of the great petrological divisions of the Girvan series has a collective fauna peculiarly its own, and that the general zoological gradation is identical with that of the acknowledged sequence in the typical Lower Palæozoic areas in Wales and the West of England.

The Lower Girvan rocks are clearly of Ordovician age: while the Upper Girvan rocks as distinctly appertain to the Silurian. The *Barr Series* corresponds to part of the Llandeilo-Bala of Wales; and

the *Ardmillan Series* is of Caradoc-Bala age. The *Newlands Series* answers to the Llandovery formation of Murchison, containing similar fossils and corresponding local breaks in the succession. The *Purple Shales* of Penhill correspond in systematic position, petrological features, and fossils with the Tarannon; and the *Straiton Beds* represent the lower division of the Wenlock.

The development of the palæontological features of the several zones of life in this succession, and the demonstration of their correspondence with the zones already recognized in the synchronous Lower Palæozoic strata of Moffat, the Lake-District, Scandinavia, and elsewhere were reserved by the author for a second part of this memoir.

3. "Notes on the Annelida Tubicola of the Wenlock Shales, from the Washings of Mr. George Maw, F.G.S." By George Robert Vine, Esq.

4. "Description of part of the Femur of *Nototherium Mitchelli*." By Prof. Owen, C.B., F.R.S., F.G.S., &c.

5. "On *Helicopora latispiralis*, a new spiral Fenestellid from the Upper Silurian beds of Ohio, U.S." By E. W. Claypole, Esq., B.A., B.Sc. (Lond.), F.G.S.

June 21.—J. W. Hulke, Esq., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "On *Thecospondylus Horneri*, a new Dinosaur from the Hastings Sand, indicated by the Sacrum and the Neural Canal of the Sacral Region." By Prof. H. G. Seeley, F.R.S., F.G.S.

2. "On the Dorsal Region of the Vertebral Column of a new Dinosaur, indicating a new genus, *Sphenospondylus*, from the Wealden of Brook in the Isle of Wight, preserved in the Woodwardian Museum of the University of Cambridge." By Prof. H. G. Seeley, F.R.S., F.G.S.

3. "On Organic Remains from the Upper Permian Strata of Kargalinsk in Eastern Russia." By W. H. Twelvetrees, Esq., F.G.S.

4. "The Rhætics of Nottinghamshire." By E. Wilson, Esq., F.G.S.

During the last few years several sections of the Rhætic beds, in addition to those already known near Gainsborough and Newark, have been exposed in making railway-cuttings. These were described by the author. The beds usually assigned to the Rhætic consist of:—Lower Rhætic, greyish or greenish marls; Middle or *Avicula-contorta* series; and Upper Rhætic (white Lias), a series of variable shales and light-coloured limestones. The author pointed out that in the Nottinghamshire district there is always a clear line of division, and sometimes indications of erosion, between the *Avicula-contorta* series and the so-called Lower Rhætic beds, while the latter graduate down uninteruptedly into the Upper Keuper Marls. Further, they

are practically unfossiliferous, while in the other series there is evidence of marine life and the remains of a fauna ranging upwards into the Lias. Hence the author proposed to class these Lower Rhætics with the Trias, and regard the Rhætic series as commencing with the base of the *Avicula-contorta* group.

5. "On the Silurian and Cambrian Strata of the Baltic Provinces of Russia, as compared with those of Scandinavia and the British Islands." By Dr. F. Schmidt. Communicated by Dr. H. Woodward, F.R.S., F.G.S.

The Cambrian and Silurian strata in question are found stretching over an area 400 miles long by 80 miles wide. The country occupied by these strata is a nearly uniform plain covered by glacial deposits; but sections are presented by the sea-cliffs, which are from 90 to 150 feet high. The strata consist mainly of marls and limestones, arenaceous deposits being rare; and they form a continuous series from the base of the Cambrian to the top of the Silurian, the whole of these strata being in conformable succession and unconformably overlain by the Devonian. Although the representative of the Cambrian or Primordial Silurian contains neither *Paradoxides* nor *Olenus*, nor, indeed, any Trilobites whatever, but only Lingulidæ and Graptolites, yet its stratigraphical position leaves no doubt as to its age. The Lower Silurian or Ordovician is the richest of the divisions, the strata of this age forming a perfectly continuous series. The author divides these beds into the stages B, C, D, E, and F, several of which are divisible into substages. Of the Stage B the lowest bed, 1 to 10 feet thick, consists of the Glauconite-bands containing the casts of Foraminifera described by Ehrenberg, which correspond to the *Ceratopyge* stage of the Scandinavian geologists. Above this, and closely connected with it, is the Glauconite-limestone, from 12 to 40 feet thick. Next comes the famous *Orthis*-limestone, a thin bed with a very interesting fauna, corresponding to that of the *Phyllograptus*-schists. The author's beds C, D, E, and F can be paralleled with the strata of Scandinavia, but have no exact representatives in the British Islands. The stage E appears to represent the Bala of England or the Trenton of North America. Although there is no stratigraphical break, there is a marked palæontological division between the Upper and Lower Silurian, there being no strata of intermediate age represented. The Upper Silurian is divided by the author into the stages G, H, I, K, which can be exactly correlated with the strata of Scandinavia. The stages G, H do not appear to have precise representatives in the British Islands; but I is undoubtedly equivalent to the Wenlock, and K to the Ludlow. In the Baltic provinces there are no representatives of the passage-beds and Lower Devonian, but the Silurian strata are unconformably covered by the Old Red Sandstone with *Coccosteus*, *Asterolepis*, *Bothriolepis*, *Homosteus*, and *Heterosteus*, there being a marked overlap between the two series. The author argues in favour of considering the Cambrian, Ordovician, and Silurian as forming, in the Baltic provinces, one "system." The author is able to construct a

section connecting the Silurians of Russia and Scandinavia, and passing through the islands of Gothland and Oesel. The paper is accompanied with a map and a tabulated list of the fossils found at the several horizons which have been distinguished by the author. He points out which of these species are found ranging into other areas, and proposes eventually to publish figures and descriptions of the characteristic Russian forms. The first part of the author's palæontological work has just appeared in the shape of a memoir describing 60 species of Trilobites of the genera *Phacops*, *Cheirurus*, and *Encrinurus*. The total number of species of the Trilobites is about 150, of which only about 15 occur in the Upper Silurian.

6. "On Chilostomatous Bryozoa from Bairnsdale (Gippsland)." By A. W. Waters, Esq., F.G.S.

7. "The Silurian Species of *Glauconome*, and a suggested Classification of the Palæozoic Polyzoa." By G. W. Shrubsole, Esq., F.G.S., and G. R. Vine, Esq.

8. "On the Cause of the Depression and Re-elevation of the land during the Glacial Period." By T. F. Jamieson, Esq., F.G.S.

The author commenced by noticing the theory advanced by Adhémar and Croll, according to which the submergence was due to the effect of a polar ice-cap causing a displacement of the earth's centre of gravity and thereby drawing the ocean towards the ice-covered pole, and proceeded to show that this theory is opposed to the geological evidence, according to which the amount of submergence has been unequal in adjacent areas and along the same parallels of latitude, showing that the movement has been in the land and not in the sea. The facts of submergence also prove that no such cap of ice could have existed at the time in the northern regions. Sundry other objections were also pointed out. The author then went on to state his own hypothesis, which is to the effect that the depression of the land was caused by the weight of ice laid upon it, and the re-elevation by the disappearance of the ice. The amount of depression would depend partly on the weight of ice and partly on the elasticity or yielding nature of the ground beneath it. He then proceeded to consider what was the weight of ice that probably existed, and referred to the elastic and flexible nature of the earth's crust, as evinced by earthquakes &c.

He further considered the relation of time to pressure, and touched upon the probable rate of subsidence, which he supposes to have been very slow and gradual. The recovery of level, he thinks, would also be very gradual, and probably, in most cases, not complete.

He next proceeded to show how his hypothesis is borne out by an appeal to geological evidence in various countries, taking England, Ireland, North America, and Greenland as examples. He further pointed out its application to the facts connected with the loess beds, Fjord latitudes, and lake-basins, and concluded with some observations on the remarkable connexion between glaciation and submergence in all countries.

XVIII. *Intelligence and Miscellaneous Articles.*

ON SOME EXPLOSIVE ALLOYS OF ZINC AND THE PLATINUM METALS. BY H. SAINTE-CLAIRE DEVILLE AND H. DEBRAY.

SOME time before the sickness of my dear and illustrious master Henri Sainte-Claire Deville, we had undertaken to return to some points in order to complete our old researches respecting platinum. Our work, as regards the division of the osmides, was almost finished, when it was interrupted by his sickness and death. I have had to terminate it; and today I present the result to the Academy.

It is known that the osmides cannot be divided by mechanical action. If for example, one essays to pound them in a tempered steel mortar, the osmide of iridium penetrates into the material without being either blunted or broken.

If, however, it be fused with 25 or 30 times its weight of zinc, and if after the mixture has been kept for some hours at an incipient red heat it then be more heated in order to volatilize all the zinc, there remains a spongy mass, easily divided, and, in this state, completely and with facility attacked by the mixture of nitrate and binoxide of barium which we have employed for oxidizing the two metals of the osmide and to render them soluble in acids. As it is easy to eliminate baryta from its solutions, the analysis of the osmide then becomes possible.

What is the part played by zinc in this division? by what mechanism is it effected? are questions which we had not then examined, contenting ourselves for the moment with a practical result sought in vain by Berzelius, which facilitated our entering upon that study of the platinum metals which has occupied us for many years.

I. Some osmide of iridium is thrown into some zinc heated to dull redness and which has been previously well cleaned with sal-ammoniac. A brisk disengagement of heat is produced. The mass is kept melted at this temperature during five or six hours, in order to attack completely the large grains of osmide. The cold button is then dissolved in dilute hydrochloric acid. The solution of the zinc takes place with great violence; and there remains a blackish residue having the appearance of graphite, which contains all the noble metals of the osmide. The greater part of the iron existing in this material is dissolved at the same time as the zinc; on the other hand, there remains a notable proportion of zinc combined with the metals of the osmide, and it is not removed by prolonged contact with concentrated hydrochloric acid.

This residue, well washed, and dried at 100°, diffuses in the air a slight odour of osmic acid. Heated to nearly 300° it suddenly ignites, almost with explosion, spreading fumes of zinc and abundant vapour of osmic acid. As this deflagration took place *in vacuo*, without any sensible liberation of gas, and of course without the

production of oxide of zinc and osmic acid, we must conclude that at about 300° the substance undergoes a change of state accompanied by a large disengagement of heat. In air the change of state is immediately followed by combustion, which augments the evolution of heat.

The residue is only partially attacked by concentrated nitric acid or aqua regia. It is quickly and completely oxidized when thrown into a mixture of fused potass and potassium nitrate; ir must even be thrown in in small portions, the mixture being but little heated, to avoid too strong a deflagration when the divided material touches the surface of the oxidizing liquid. If one wishes to make use of this residue either for the analysis of the osmide or to extract the metals which it contains, it is better to mix it with anhydrous baryta and barium nitrate: one can then heat it without fear of losing the substance, which is then attacked totally and easily.

This substance is evidently a complex mixture of various alloys which zinc is capable of forming with the platinum metals: we were therefore induced to examine more closely those different combinations.

II. Osmium simply dissolves in zinc. When the fused button of this metal with osmium in a state of division is acted upon by hydrochloric acid, there remains pure osmium with a crystalline appearance.

Palladium and platinum, treated in the same manner, leave a residue consisting of alloys which undergo no isomeric modification when heated *in vacuo*. Rhodium, on the contrary, and iridium and ruthenium especially, as one of us has already proved *, combine with zinc with much liberation of heat: and when the zinc button is dissolved in hydrochloric acid, residues are obtained which are susceptible of undergoing a true isomeric modification accompanied by a brisk liberation of heat, without loss of gas, when heated in a vacuum above 300°. Before the liberation of heat, the blackish residues would be more or less readily attached by aqua regia. They lose this property after the liberation of heat, and then take the metallic appearance.

The thermal phenomenon which accompanies the change of state of the iridium residue is so marked that it may serve for the recognition of the presence of small quantities of iridium in platinum (1 or 2 per cent. for example). The metal to be assayed is dissolved in a large excess of zinc; and the button is acted upon by dilute hydrochoric acid; the residue, well dried, raised to a temperature above 300° in a platinum capsule becomes incandescent at various points. Ruthenium and rhodium produce similar effects.

III. In brief, osmium is the only platinum metal which does not retain zinc when its alloy with a large excess of zinc is treated with an acid capable of dissolving that metal. The other metals obstinately retain a notable proportion of it (on the average 10 to

* *Comptes Rendus*, t. xc. p. 1150.

12 per cent.); and the metals which are insoluble in aqua regia (rhodium, iridium, and ruthenium) then remain in the state of peculiar products, without metallic lustre, which seem to be an allotropic modification of the true alloys possessing a metallic appearance.

The action of zinc upon the osmide of iridium is thus accounted for naturally. If the heat liberated in the combination of iridium and zinc much exceeds the heat of combination of osmium and iridium, the osmide, in conformity with the laws of thermochemistry, will be destroyed by the zinc; the osmium dissolves and may crystallize in the excess of metal; the iridium and the other metals remain combined with the zinc. It is the residue from the action of the hydrochloric acid upon this alloy with excess of zinc that constitutes the explosive substance above considered. In fact the heat liberated in the union of zinc with iridium, ruthenium, and rhodium, which are the dominant metals of the osmide, is truly enormous: on adding, for instance, 1 part of iridium to 30 or 40 per cent. of melted zinc at a temperature below a red heat, there is combination accompanied by actual incandescence of the whole mass of metal; it is the same with the two other metals. If all the zinc be driven out by heat (as was done by us in our old experiments), the hardly fusible metals of the osmide, free or alloyed, remain in a state of extreme division, in which they are much more readily attacked than the natural osmide.

In an early Note we shall speak of facts observed in the solution of the platinum metals in copper.—*Comptes Rendus de l'Académie des Sciences*, June 12, 1882, t. xciv. pp. 1557–1560.

ON THE REACTION-CURRENT OF THE ELECTRIC ARC. BY M. JAMIN, WITH THE ASSISTANCE OF M. G. MANEUVRIER.

The two currents, in alternately opposite directions, given by Gramme's self-exciting machine are absolutely equal; consequently they do not decompose water, and a tangent-compass intercalated in the circuit undergoes no deflection, since the contrary effects following at very close intervals destroy one another. This destruction of the effects is kept up when one or several burners are put into the circuit, provided that the two carbons are equal, disposed in exactly the same manner, and are heated equally.

If eight or ten Bunsen elements be introduced into the total circuit, they communicate to the compass a deflection δ when the machine is at rest, and a deflection δ', absolutely equal to δ, when the machine is working.

	δ.	δ'.
First experiment	32	33
Second experiment.....	38	38·45
Third experiment	33	34·10

This equality proves that the resistance of the wires of the machine does not vary, whether the machine be at rest or in motion;

it proves also that the two effects of the machine and the battery are superposed and independent.

If we now suppress the battery, but ignite in the circuit a burner formed of two unequal carbons—one stout (0·004 metre), the other thinner (0·002 metre),—this asymmetry suffices to determine a permanent deflection of the compass, just as if a battery had been intercalated. The two systems of mutually inverse currents given by the machine cease, therefore, to be equal; that which is directed from the thick carbon to the thin, from the less hot part to that which is more so, prevails over the system of which the direction is opposite. A differential current results, indicated by the compass, and the more intense as the difference of thickness of the two carbons is more marked; it reaches its maximum between a large mass of retort-carbon, which becomes little heated, and a crayon terminating in a fine point, which attains the highest temperature. The same phenomenon is obtained, and in the same sense, when an arc is produced between a mass of any metal and a carbon point. As it is difficult to maintain the constancy of the arc, the intensity of the differential current is very variable; the following results must be regarded as only a first approximation:—

Lead.	Iron.	Carbon.	Copper.	Mercury.
29°	30°	31°	60°	70°

In general the differential current is weak or none when the arc is but of little extent; it increases with the distance of the electrodes. For zinc it is at first as intense as with copper; but it falls suddenly, probably on account of the oxide with which the metal is soon covered.

The deflection depends on two things:—1, on the mean electromotive force of the differential current; 2, on the resistance introduced into the current by the arc which is formed. It is easy to compare that force and that resistance in the different cases in the following manner:—

We introduce into the total circuit a battery of n pairs, having an electromotive force $n\mathrm{A}$. According to whether it acts in the direction of the differential current or in the opposite direction, we have

$$i=\frac{x+n\mathrm{A}}{\mathrm{R}}, \quad i'=\frac{x-n\mathrm{A}}{\mathrm{R}}.$$

When x is greater than $n\mathrm{A}$ both deflections have the same sign, and we find

$$\frac{i}{i'}=k=\frac{x+n\mathrm{A}}{x-n\mathrm{A}}, \quad x=n\mathrm{A}\frac{k+1}{k-1};$$

but if x is less than $n\mathrm{A}$, the two deflections have opposite signs—

$$\frac{i}{i'}=k=\frac{x+n\mathrm{A}}{n\mathrm{A}-x}, \quad x=n\mathrm{A}\frac{k-1}{k+1}.$$

I employed fresh and well amalgamated Bunsen elements; the experiment, not susceptible of great precision, on account of the variations of the arc, gave the following values of the electromotive force x of the differential current evaluated in Bunsen elements:—

Lead.	Iron.	Carbon.	Copper.	Zinc 1.	Zinc 2.	Mercury.
2·5	3·2	5·0	50·6	66·2	5·7	103·7

The first three substances are nearly equal; the last three are very active. Copper is equivalent to 50 bunsens; zinc to 66 at the commencement of the action, falling to 5. As to mercury, it presents an enormous and very constant value; the differential current has a mean electromotive force equivalent to 103·7 bunsens. The same determinations conduct us to an estimation of the introduced resistance. We have, in fact,

$$i-i'=\frac{2nA}{R}, \quad R=\frac{2nA}{i-i'}.$$

R is in the inverse ratio of $i-i'$.

Now the total resistance R was composed (1) of that of the wires of the machine, (2) of that of the battery, (3) of that of the electric arc. The last alone is variable, and increases or diminishes the value of R: it will therefore be the more the less $i-i'$ is. Here are the calculated values of $i-i'$:—

Carbon.	Iron.	Lead.	Copper.	Mercury.	Zinc 1.	Zinc 2.
0·406	0·307	0·283	1·41	0·89	1·02	0·56

It would follow from these numbers that carbon, iron, and lead offer the greatest resistance, copper and mercury the least.

The differential current can only be explained in two ways—either by a difference in the resistance, or else by inequality in the inverse reactions of the arc in the one direction or the other.

In order to ascertain if the resistance of the arc varies with the change of direction, I caused a continuous current to pass, first from the carbon to the mercury, afterwards from mercury to carbon. A compass, placed in derivation, measured the intensity in the two cases. I could not measure any appreciable difference. But these experiments presented a remarkable peculiarity: when the current passed from mercury to carbon, the arc had a very pronounced green colour, and the volatilization of the metal proceeded vigorously: in the contrary case the arc was reddish, and there was a less abundant production of vapour. This renders evident the asymmetry existing in the two cases. Now, when the alternating currents of a Gramme machine are directed through this burner, the arc is green, which proves that the current going from mercury to carbon predominates over that which goes in the opposite direction: and as there is no difference in the resistance of the arc, it is

in a peculiar property of the alternating currents that the reason of the differential current must be sought.

Each of the two systems of currents stores up, at the instant of its commencement, a certain sum of energy, which is set free when it ceases, and is manifested by a contrary current, or, as Edlund says, by an inverse electromotive force. Thus a first current →, initially very feeble, gradually increases, and, when it ceases, gives rise to an inverse reaction ←, which adds itself to the current ← developed by the machine at the same instant. If, then, one of the systems of current → presents a weaker reaction than the contrary system ←, it will be less weakened and more reinforced, and will determine the direction of the differential current.

Whatever may be the explanation of these facts, it is clear that, once produced by an arc with mercury, the differential current entirely changes the action of the machine, that one of the systems of currents is, if not extinguished, at least considerably weakened, and that the other system is constituted by successive currents of greater intensity and duration. Also every additional electric arc introduced presents the same aspect as that of the batteries—that is to say, greater brightness and heat at the positive pole, with transfer of matter to the negative pole. The machine, previously incapable of decomposing water, becomes capable of decomposing it like a battery with an electromotive force equal to 100 Bunsen pairs: it can, like the batteries, determine all the chemical actions we will, magnetize soft iron, reduce metals, convey force—in a word, replace a continuous-current machine in its applications.

There are two types of magneto-electric machines: those of the one kind, derived from the Gramme system, can give directly currents constant in direction; the others, like those of Nollet or Méritens, can engender only alternating currents: the latter are applicable only to the production of light; it has been in vain attempted to employ them for chemical operations by directing the currents with a commutator. It is evident that this commutator might be replaced automatically by one or more arcs formed between a bath of mercury and a carbon point*. It remains to ascertain what are the economical conditions of that transformation.—*Comptes Rendus de l'Académie des Sciences*, June 19, 1882, pp. 1615–1619.

ON THE MOTION OF A SPHERICAL ATOM IN AN IDEAL GAS.

BY G. LÜBECK †.

The author considers an atom of mass M, moving through a gas at rest, of which the atoms are of mass *m*. With respect to the impact, he avails himself of the principle of *vis viva*, of that of the centre of gravity, and, lastly, of the principle that communication

* These experiments were made at the laboratory of the Sorbonne.

† *Festschrift des Fried.-Werd.-Gymn. Berlin*, 1881, pp. 295–312.

of motion takes place only in the direction of the common normal to the two cylindrical atoms at the instant of the impact. The atoms then behave like perfectly hard elastic spheres; no internal motion takes place. First the number $\mathfrak{N}$ of the collisions is calculated (in the known manner) which the atom M experiences when during a unit of time it moves through the atoms m with the constant velocity Ω; and it is remarked that $\mathfrak{N}$ is a minimum for $\Omega=0$.

It is then assumed that the velocity of M is continually altered in quantity and direction by the collisions, but at the same time a certain mean velocity A, in the direction OX, prevails. Those deviations from the mean motion effected by the impacts the author names the "oscillating motion" of the atom M. [To some extent in this way behaves an atom of one kind of gas which is diffused with a certain velocity through another.—The Reporter.] The probability f_ν that the atom M has the velocity-components U_ν, V_ν, W_ν in the directions of the axes of coordinates the author finds by a method first employed by O. E. Meyer. He first finds the probability that the atom, in n arbitrarily chosen time-elements, has successively the velocity-components

$$U_1, V_1, W_1, U_2, V_2, W_2, \ldots U_n V_n W_n$$

equal to the product

$$f_1, f_2 \ldots f_n.$$

As the most probable distribution of velocities he designates that for which this product is a maximum. But now the sought-for function f is not variated, but the differential quotients of the above product with respect to the variables contained therein are, under the corresponding accessory conditions, put equal to 0, which gives

$$f=ce^{-\lambda[(U-\alpha)^2+(V-\beta)^2+(W-\gamma)^2]}.$$

By F the author denotes the ratio of the time during which the velocity-components of the atom M lie between the limits U and $U+dU$, V and $V+dV$, W and $W+dW$ to the whole time ofthe motion of that atom: and he finds from the above, putting

$$U=\Omega\cos\theta', \quad V=\Omega\sin\theta'\cos\phi', \quad W=\Omega\sin\theta'\sin\phi',$$

$$\omega=\sqrt{km}\Omega, \quad \alpha=\sqrt{km}A, \quad \kappa=\frac{\lambda}{km},$$

$$F=\left(\frac{\kappa}{\pi}\right)^{\frac{3}{2}}e^{-\kappa(\omega^2-a\omega-2\cos\theta'+a^2)\omega^2}\,d\omega\sin\theta'\,d\theta'\,d\phi'.$$

For the quiescent gas, Maxwell's distribution of velocities is assumed:—the number of the atoms in the unit of space for which the X component of the velocity lies between u and $u+du$ is

$$N=\sqrt{\frac{km}{\pi}}\,e^{-kmu^2}\,du.$$

If L be the mean *vis viva* of the atom M, this gives for the mean *vis viva* $L-\frac{1}{2}(MA^2)$ of its oscillating motion the value $3M/4\lambda$. The mean *vis viva*, however, of an atom m is equal to $3/4k$. In order to find the dependence of L on A, the author seeks the probability $\Phi dU_1\,dV_1\,dW_1$ that, if the atom M before the impact had the velocity-components U, V, W, the resulting impact is exactly such that the velocity-components after it lie between the limits U_1 and U_1+dU_1, V_1 and V_1+dV_1, W_1 and W_1+dW_1. Considering first an impact occurring upon any surface-element of the sphere, and then summing all the collisions resulting upon all the surface-elements so that the condition mentioned is fulfilled, he finds

$$\Phi dU_1\,dV_1\,dW_1=\left(\frac{M+m}{2m}\right)^2 R^2 N\sqrt{\frac{km}{\pi}}$$

$$e^{-km}\left[\frac{M+m}{2m}Q+\frac{U(U_1-U)+V(V_1-V)+W(W_1-W)}{Q}\right]^2$$

$$\frac{dU_1\,dV_1\,dW_1}{Q\mathfrak{R}};$$

in which R is the radius of the atom M,

$$Q=+\sqrt{(U_1-U)^2+(V_1-V)^2+(W_1-W)^2}.$$

Now, as the atom M in the course of the time-unit collides FN times with m so that before the impact the velocity-components of the former lie between the limits U and $U+dU$, V and $V+dV$, W and $W+dW$ without any further condition,

$$F\mathfrak{R}\Phi dU_1\,dV_1\,dW_1$$

is the number of impacts which M during the unit of time suffers in such wise that the velocity-components before the impact lying between the limits U and $U+dU$, V and $V+dV$, W and $W+dW$, after the impact lie between the limits U_1 and U_1+dU_1, V_1 and V_1+dV_1, W_1 and W_1+dW_1. If we integrate over all the differentials contained in F, we obtain the number of impacts after which the velocity-components lie between the limits U_1 and U_1+dU_1, V_1 and V_1+dV_1, W_1 and W_1+dW_1 without any other condition: and since this must be equal to the number of impacts at which the velocity-components before the impact lie between the same limits, without any condition for those after the impact, we get

$$F_1\mathfrak{R}_1=dU_1\,dV_1\,dW_1\int_0^{\pi}\int_0^{\pi}\int_0^{2\pi}F\mathfrak{R}\Phi.$$

If in this equation, which holds for all values of U_1, V_1, W_1, we assign to each of these quantities the value 0, we get, after carry-

ing out the three integrations.

$$\left(\frac{2m}{M+m}\right)^2 = \frac{e^{p^2}}{\eta p}\int_0^p e^{-x^2}\,dx,$$

in which

$$p^2 = \frac{\kappa^2 a^2}{\kappa + \left(\frac{M-m}{2m}\right)^2}, \quad \eta = \kappa + \left(\frac{M-m}{2m}\right)^2.$$

From this we find

$$\kappa > \frac{M}{m}, \quad \frac{3M}{4\lambda} = L - \frac{M}{2}A^2 < \frac{3}{4k};$$

that is, the part of the mean *vis viva* expended upon oscillating motions of the atom is in general less than, and for $A=0$ is equal to the mean *vis viva* of an atom m. For $A=\infty$ it is equal to 0. The author demonstrates also that

$$L - \frac{M}{2}A^2$$

with A increasing must always constantly diminish.

By integrating $F\mathfrak{N}$ over the three differentials therein contained, the author finds the number Z of the collisions which the atom M suffers in unit time, and by threefold integration of $F.\Omega$ the sum S of all the lengths of path of the atom during the unit of time. $\rho = S/Z$ is the mean path between two collisions. Simple values were obtained only for $A=0$.

Since the atoms m form a resting gas, it is clear that the atom M will continually lose more and more of its own proper velocity through the collisions. The author calculates, first, how much a collision of any kind changes the X component of the velocity of M. This quantity, multiplied by the number of collisions of that kind in unit time, and integrated over all kinds of collisions, gives the diminution dA/dt which the proper velocity of the atom M undergoes on the average in the unit of time, and which the author, like Stefan (in his theory of gas-diffusion), designates the resistance of the gas to the atom M. If A is very small, the calculation gives

$$\frac{dA}{dt} = -q^2 A, \quad A = A_0 e^{-q^2 t},$$

while

$$p^2 = \frac{\sqrt{\pi}NR^2}{\sqrt[3]{k}M} \cdot \frac{M}{m} \cdot \frac{1 + \frac{13}{4}\frac{M}{m} + 4\frac{M^2}{m^2} + \frac{M^3}{m^3}}{\left(1+\frac{M}{m}\right)^{\frac{7}{2}}}.$$

—Wiedemann's *Beiblätter*, 1882, no. 6, pp. 451–455.

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XIX. *On the Electricity of Flame.*

By JULIUS ELSTER *and* HANS GEITEL*.

[Plate IV. figs. 1–4.]

§ 1. *Introduction.*

ON the electricity of flame there is already a long series of memoirs; but in many respects they contradict one another, both in regard to the results and also to the views advocated by the different authors as to the cause of flame-electricity. As Holtz† has briefly given a very perspicuous digest of all the memoirs which refer to the electrical behaviour of flames, a reiterated historical quotation of them in this place may certainly be dispensed with.

The origin of the electrical difference can be accounted for by the following three causes:—

(1) The electricity of flame is caused by the process of combustion as such (Pouillet‡, Hankel§).

(2) It arises from the flame behaving to the metals introduced as electrodes like an electrolyte (Matteucci‖). To this explanation, evidently, no other meaning can be attached than that the different layers of the flame excite differently by contact the wires immersed in them. For shortness, we will

* Translated from Wiedemann's *Annalen*, 1882, no. 6, vol. xvi. pp. 193–222.

† Carl's *Rep.* xvii. pp. 269–294 (1881).

‡ *Ann. de Chim. et de Phys.* xxxv. p. 404 (1827).

§ Pogg. *Ann.* lxxxi. p. 212 (1850). ‖ Phil. Mag. 1854, viii. p. 309.

in future designate this as the "electrolytic" theory, and, in correspondence therewith, speak also of "electrolytic" excitation by the flame.

(3) Its explanation is found in a thermoelectric difference of the electrodes (Buff)*.

The theories which are deduced from the unipolar conductivity of flame we may be permitted to pass without notice, since, as will result from the following investigation, sources of error may have prevailed here, causing the conclusions drawn from them to appear doubtful.

Besides these differences in respect of theory, however, the different observers also adduce experiments which are absolutely irreconcilable with one another. This goes so far that even in regard to the question whether the positive or the negative electricity is that which is proper to flame no unity prevails.

The reason of this lies in the fact that all the observers have overlooked a point that plays a very essential part in the electricity of flames, namely the behaviour of the shell of air which immediately envelopes the flame.

The maximum of electromotive force is always found when one wire is introduced into the latter, and another into the interior of the flame, as will be shown in the following. At the same time, however, with the electrodes in this position the resistance of the hot layer of gas separating the wires is uncommonly greater; so that we can hardly reckon on measuring the intensity of the current with the aid of a multiplier; it is perhaps a consequence of this circumstance that all the observers who have investigated flame-electricity by means of that instrument have lost sight of the point above named.

As hitherto, so far as is known to us, the electricity of flame has not been examined with an electrometer permitting exact measurements, it appeared to us of importance to test the electric behaviour of flame with Thomson's quadrant-electrometer, and eventually to verify the correctness of one of the theories above cited.

§ 2. *Apparatus and Method.*

In order to keep the charge of the needle of the electrometer constant, it was connected with one of the poles of a Zamboni's battery consisting of 2400 pairs of plates, the other pole of which was led to earth. The double deflection produced by a normal daniell varied 4 or 5 scale-divisions during

* Lieb. *Ann.* lxxx. p. 1 (1851) & xc. p. 1 (1854).

the time of the investigation, and amounted in the mean to 112. To this normal daniell, putting its electromotive force =100, all the data given in the following communication are referred. The scale was placed at 2 metres distance from the mirror; and the deflection left and right from the position of rest was measured by means of a suitable turn-plate.

In order to convey the electricity of the flame to the quadrants, of which one was, as usual, connected with the earth, straight wires were mostly employed, or electrodes from liquids, which, fixed in suitable stands well insulated upon cakes of resin, could be raised and lowered at pleasure.

The experiments were made with well-insulated Bunsen burners and with alcohol flames. The flames of the former issuing from apertures of the usual width proved too flickering, and therefore the measurements too uncertain. On this account a very small Bunsen burner was prepared from a glass tube of 4 millim. width. Its upper extremity was surrounded by a platinum sheath, in order to avoid colouring the flame by the gradually heating glass. When one electrode [the "base-electrode"] (B, fig. 1) was immersed in the foot of the flame, and the other in its apex [the "apex-electrode"] (S, fig. 1), with the turn-plate in one position the foot of the flame was connected with the earth, and the apex insulated; with the other position the reverse took place.

§ 3. *Longitudinal Polarization of the Flame.*

Hankel states that when one platinum wire is introduced into the apex of a flame and one into its base, a galvanometer indicates an electric current passing from above downwards. From this it might be inferred that the flame is polarized lengthwise.

The corresponding experiment with the electrometer gives apparently the same result; but in this case two very striking points are to be remarked.

The first point is this:—If the experiment be arranged as represented in fig. 1, the apex mostly appears, as in Hankel's experiment, negative to the base; but very often, and apparently without any external cause, the reverse takes place.

The second is that when the metal from which the flame issues is connected with that quadrant which is conducted to earth, while the insulated electrode is introduced at different heights s above the base, by suitably shifting the insulated electrode within a cross section the appearance of a constant potential within the flame is easily attained.

Thus the electromotive force (E) was determined as follows:—for

millim.	
$s=1$	$E=104$
$s=20$	$E=104$
$s=60$	$E=94$ (wire at the extreme apex).

Lastly, as a third noteworthy point may be added that the electromotive force is independent of the size of the flame, and, consequently, of the amount of the burning gas. This is evident from the following experiment:—

By regulating the admission of the gas, over the same aperture of the burner three flames of different height h were produced, and their apices connected with the insulated pair of quadrants by a clean plate of platinum. There were found, for

$h=20$	$h=35$	$h=70$
$E=73{\cdot}7$	$E=75$	$E=73{\cdot}9$,

consequently the electromotive force E independent of the height h of the flame. Two subsequent series of experiments gave the same result. Here the arrangement of the experiment was that shown in fig. 2*a*. There were found, for

	1st series.	2nd series.
$h=20$	$E=142$	$E=213{\cdot}2$
$h=40$	$E=145$	$E=219{\cdot}2$
$h=70$	$E=142$	$E=216{\cdot}0$

The reason of the value of E being here so much higher will appear subsequently.

§ 4. *Polarization of the Flame in the Cross Section.*

If the flame were polarized lengthwise, the surfaces of equal potential would be given by planes perpendicular to the axis of the flame. On examining cross sections of this sort, the surprising result was obtained that, if the two platinum wires laterally introduced penetrate the flame to an equal depth, the difference of potential within one and the same cross section $=0$, but that with a slight horizontal displacement of either electrode a difference of potential often appears, which equals that produced by the vertical displacement or far exceeds it. The latter takes place whenever one of the electrodes no longer dips into the flame, but into the enveloping shell of hot air (A A′, fig. 1). The layer of air immediately enveloping the flame plays therefore an essential part in the electrical behaviour of the flame.

Hence, in order to avoid possible errors, it appeared advisable to cover as much of the electrodes as was not within the flame with an insulating coat, which could easily be done by fusing the platinum wires into glass tubes. The wire projecting out of the glass was just long enough to reach from one margin of the flame to the other.

If, now, two such platinum wires were placed opposite one another in one and the same cross section of the flame, and one of them was continually moved further and further from the other, with its complete withdrawal from the flame a very considerable increase of the electromotive force occurred; it rose from 12 to 192; and the electrode which was in the layer of hot air was positive.

Accordingly, a flame gives the maximum of action when the apex-electrode is introduced into the hottest part of the flame, and the base-electrode into the sensitive hot layer of air, about as represented in fig. 2 *a*.

Let it be further remarked that with this arrangement of the experiment a reversal of the polarity of the flame was never observed by us, and that all carburetted-hydrogen flames exhibited the same behaviour.

§ 5. *On the Change of the Polarity of a Flame.*

It was mentioned in § 3 that one and the same flame appears sometimes positive, sometimes negative. Since, then, the wire introduced into the air envelope becomes highly positively electric, it is clear that uncommonly much will depend on how the electrodes are introduced into the flame. If it is wished to have the apex of the flame negative, the apex-electrode must be completely enwrapped in the flame; in the other case, especially if the base-electrode dips quite into the flame, the electric excitation of the enveloping air stratum may preponderate, and consequently the flame appear positive. If this is the true explanation, it must be possible artificially to change the polarization of a flame.

Of the experiments with alcohol-flames we note the following; the positions of the platinum electrodes are given in fig. 2 *a*, *b*, *c*, *d*, *e*. In fig. 2 *a* the base-electrode B is in air at about $\frac{1}{2}$ millim. distance from the margin of the flame, and will now be gradually pushed in till (fig. 2 *c*) it touches both margins of the flame. The apex-electrode S has had this same position from the beginning. The electrode B was then left in this situation and S gradually drawn out of the flame until it was all in air (fig. 2 *e*). The deflections were as follows:—

Position of rest of the electrometer-needle: 511·0.

Daniell = 100.

Position of electrodes.	Orientation of needle.	Electromotive force.
a	439·0	+144
b	485·0	+ 52
c	510·5	+ 1
d	547·0	− 72
e	572·0	−122

(The sign placed before E gives the direction of the electric excitation of the electrode B.)

Or, in words:—

As long as B is outside of the flame it is positive, S negative (fig. 2 *a*).

When B is in the margin of the flame, the deflection diminishes, but B still remains positive, S negative (fig. 2 *b*).

When B and S are equally immersed in the flame the needle returns to its resting-position (511); deflection 510·5. Therefore, with the position fig. 2 *c*, E=0.

If S is now drawn back into the margin of the flame (fig. 2 *d*), it becomes positive, B negative; the polarity of the flame is therefore reversed. And when, finally, S is quite outside of the flame (fig. 2 *e*), it is strongly positive; consequently it behaves exactly as did the electrode B in the initial position (fig. 2 *a*).

With a suitable position of the electrodes, consequently, the flame is shown to be not polarized lengthwise at all. This proves that the longitudinal polarization of the flame is only apparent, called forth by the unequal immersion of the two electrodes. At the same time the second point, the constancy of the value of the potential in the flame, is hereby explained.

The reversal of the polarity can likewise be shown with a gas-flame; only it does not bring back the electrometer-needle quite to its position of rest, a small ± deflection of from 5 to 10 scale-divisions always continuing to subsist.

A bisulphide-of-carbon flame shows the reversal of the polarity in like manner as a spirit-flame, which is interesting inasmuch as in it the chemical process is fundamentally different.

Leaving quite out of consideration provisionally a proper electricity of flame, the electrical phenomena in question might be essentially conditioned by contact of the metals with the hot air and the gases of the flame. It might then be expected that the electromotive force would depend on the nature of the metals, as well as on that of the burning gases.

A series of very carefully made experiments have most decisively confirmed both these conclusions.

§ 6. *Dependence of the Electromotive Force upon the Nature of the Metals.*

If a platinum electrode is brought into the base of the flame or into the sensitive stratum of air while the apex is, as exactly as possible, at the same place conducted to earth, considerably different values are obtained, according to the nature of the conducting metal. In the series of experiments recorded in the following table the position of the electrodes was that represented in fig. 2 *a*. B, as well as the flame itself, remained unmoved during the whole time of a series of experiments, while the apex-electrode S consisted successively of wires of platinum, iron, copper, and aluminium. The experiments were made with the non-luminous flame of the small Bunsen burner described at the commencement.

Table I.

The flame-apex electricity conducted away by	Electromotive force for D=100. Series of experiments.			
	I.	II.	III.	IV.
Platinum . .	49·6	116·0	157·2	188·4
Iron	64·3	139·0	173·8	232·0
Copper . . .	...	153·2	208·8	264·0
Aluminium . .	171·0	237·0	268·8	364·0

In all four series the flame shows itself very highly electric when conducted away by aluminium, less so with conduction by copper, still more feebly on the employment of iron; and the smallest values are obtained with conduction by platinum.

When both electrodes dip into the flame the result is completely analogous; and it is just the same when, instead of the non-luminous gas-flame, a luminous gas-flame or a spirit-flame is employed. The peculiar position of aluminium also with such an arrangement of the electrodes and the employment of such flames follows from Table II.

Table II.

Kind of flame.	Apex of flame conducted-from by Platinum.	Aluminium.	Electrodes as in
Ordinary Bunsen burner	74·2	149·2	Fig. 2 *b*
Luminous gas-flame . .	49·6	112·2	Fig. 2 *b*
Spirit-flame	160·0	278·0	Fig. 2 *a*

The base-electrode, in all the series of experiments, consisted of a platinum wire. As the wires employed were not of equal thickness, we made another series of experiments with three plates, of exactly equal thickness, of aluminium, copper, and

platinum, which were introduced into the apex of a pure-alcohol flame. In the gas-flame there was always a fusion of the aluminium, altering the shape of the electrode. The possible source of error herein contained also disappears when a spirit-flame is employed. The determinations of the electromotive force of the flame were:—when its apex was conducted-from by

The platinum plate . .	120·1
The copper plate . . .	166·0
The aluminium plate .	301·5

Sodium and magnesium are more negative than aluminium, as will be seen from the following numbers:—

Flame conducted-from by	Electromotive force.
Platinum wire	119
Aluminium wire	198
Magnesium ribbon	221
Sodium	338

The last two metals were introduced into the lower part of the flame, in order to prevent their ignition. The sodium was a piece of the size of a bean, with a bright cut surface.

The relative position to one another of all the metals employed is specified by the following numbers :—

Purchased wires of	Gold (not pure), Platinum, Silver (not pure) . .	150
	Iron	170
	Copper	200
	Aluminium	300
	Magnesium	320
	Sodium	500
	A Daniell's element =	100

It is consequently put beyond question that the electric condition of a flame depends essentially upon the nature of the metal conducting from it ; but that nevertheless the quality of the surface of the electrode which is in air plays also an essential part was evidenced by the following experiments:—

If the insulated base-electrode in air be wetted with water or a salt-solution while the apex of the flame communicates with the earth, a very considerable diminution of the electromotive force immediately takes place, especially when a solution of potassium chloride is employed.

A perfectly clean platinum wire, employed as the base-electrode, gave E = 184 ; when it was wetted with distilled water, E instantly fell to 134, passed through the values 148,

160, and was finally constant at 170. That the former value 184 was not again reached after the evaporation of the water may probably be accounted for by a slight impurity of the (commercial) distilled water.

Still more striking was the phenomenon when a solution of potassium chloride was employed. Here likewise an instantaneous diminution of the electromotive force from 184 to 74 took place; and when the wire was once hastily drawn through the flame so that small particles of fused potassium chloride overspread it, the electromotive force fell quite to 16. This value could not be increased by any shifting of the electrode; so that the cause of this great diminution cannot possibly be the unavoidable change of place of the electrode concerned.

Finally, let us mention one more circumstance belonging to this—namely that freshly annealed wires, used as electrodes in the air stratum, always give higher values than those which have remained a longer time (say ten minutes) exposed to the air—the explanation of which behaviour, even in the case of platinum, can only be found in an alteration of the quality of the surface.

§ 7. *Repetition of the Experiments with Liquid Electrodes.*

In order to completely avoid the contact of the flame-gases with metals, liquid electrodes, of the form represented in fig. 3, were employed. By the pressure of the liquid column in the glass tube R a drop was pressed out of the fine aperture *a*, which was then brought into the air surrounding the flame, and as near as possible to its base. A U tube served to put the flame of a Bunsen burner constructed entirely of glass into communication with the earth, one leg of which ascended the inner cavity of the burner. Both the glass electrodes were filled with distilled water, into which clean platinum wires (Pt) dipped. When the two water columns in the electrodes were connected directly with each other, the electromotive force called forth by the heterogeneity of the platinum electrodes amounted, at the maximum, to 0·05 daniell.

Now, in all the experiments, the electrode which was in air was charged in the same sense as a metallic electrode; it likewise was positive, though the electromotive force was certainly much weaker. The mean values from three series of experiments were the following:—

$$E=51, \quad E=51, \quad E=56,$$

while two platinum electrodes at the same flame gave values which lay between 150 and 180.

A similar series of experiments were performed with a

spirit-flame, in which direct contact of any metal with the flame was likewise avoided. The conduction to earth took place by means of a platinum plate dipping into the alcohol of the lamp, and connected with the earth-conduction by a platinum wire. In addition to the electrode represented in fig. 3, a wet string wrapped tightly round a glass rod was employed. The electromotive force was then determined:—For the

Water electrode in air . E=24,
Wet string in air . . . E=58;
For a platinum electrode E=99.

That lower values likewise result for the latter than in the previous experiments cannot be surprising, since in this arrangement of the experiment there is no *second* metal dipping into the flame itself.

A direct determination of the combination platinum, water, alcohol, platinum gave a maximum of 11·6 for a daniell=100; so that the observed electromotive force cannot be produced by this.

In employing the wet string, care must be taken not to place it tangentially near the flame; for then small fibrils might project into the flame itself, by which, for the reasons above discussed, a reversal of the polarity of the flame might easily be induced.

The above-communicated values being so much lower than on the employment of platinum electrodes may be accounted for by the conduction to earth by distilled water or alcohol being always very imperfect. On this account it seemed advisable to examine the behaviour of a water electrode over against a platinum electrode. If the apex of the flame is conducted-from to earth by a platinum wire, the water electrode which is in air is positive only so long as the platinum wire is completely enveloped by the flame; if it be drawn so far out that it also is entirely in the hot-air stratum, the polarity of the electrodes is reversed—the platinum wire being positive, the water electrode negative. For example, in an experiment of this kind, by the drawing-back the electromotive force was raised from −142 to +60 (the signs refer to the metal electrode). From this it follows that metals in contact with hot air become more strongly positive than liquids, but that liquids in contact with heated gases exhibit nevertheless a similar behaviour to that of metals. Accordingly we must not directly infer, from the fact that flames show themselves electric even when all metals are avoided, the existence of a peculiar electricity of flame.

The complete analogy of behaviour between metal and water

electrodes appears also from the kind of metal which conducts from the flame to the earth having an essential influence upon the result. When the base-electrode was formed by water, and the electricity conducted from the apex of the flame by different metal plates of equal thickness, determination of the electromotive force E, when the conductor was

A platinum plate, gave $E = 73{\cdot}6$,
An aluminium plate, gave $E = 176{\cdot}8$.

The electromotive force of the combination aluminium, flame, hot air, water, platinum is therefore as much again as that between platinum, flame, hot air, water, platinum, completely in accordance with the previous experiments with two metal electrodes.

§ 8. *Dependence of the Electromotive Force on the Nature of the Flame.*

Since the electromotive force of flame depends on the nature of the metal introduced into it, it is to be expected that, when the constituents of the flame-gases are changed, an alteration of the electromotive force must also occur. Such an alteration can be readily brought about by introducing, for example, a bead of soda into the flame, on a well insulated wire. Indeed a deflection of the electrometer-needle then takes place immediately; only with sodium there is the great inconvenience that within a short time the entire atmosphere of the room is so impregnated with sodium vapour that the flame burns with a strong resemblance to a sodium-flame, which affects the trustworthiness of the results. On this account a salt to which flame is less sensitive was chosen, namely potassium chloride.

It was first ascertained, by a series of careful experiments, that the introduction of a well-cleaned and insulated platinum wire into the flame did not alter the electromotive force. It may be sufficient to allude to this point here, as we shall subsequently return to it.

When the electrodes are in the position shown in fig. 2 *a*, and a bead of potassium chloride is introduced on an insulated platinum wire, the needle receives an impulse which indicates an increase of the electromotive force; but it quickly goes back again, and, indeed, far below the value of the deflection which had been given with a pure flame. According to this, there was a diminution of the electromotive force; but it was only an apparent diminution; for if the bead of potassium chloride be now taken out of the flame, the needle approaches still nearer to its resting-place. This indicates that with the electrode which was in air an alteration must have taken place. Upon it a thin dash of potassium chloride has formed,

so that the platinum wire, introduced into the flame, burns for a moment like potassium. As soon as the colouring of the flame is over, the same wire, used as the electrode in air, again gives the usual (mostly somewhat higher) values.

The apparent diminution of the electromotive force is consequently caused by the coating of the electrode which is in air with potassium chloride, corresponding to the experiment recorded in § 6.

In the following Table, E denotes the electromotive force of an absolutely pure non-luminous gas-flame; E_k, the electromotive force of the same flame when a bead of potassium chloride is introduced; E′, the electromotive force of the flame after removal of the bead, but with the electrodes B and S (fig. 2 *a* & *e*) covered with Ka Cl. Accordingly $E_k - E'$ represents the increase of electromotive force produced by the introduction of the Ka Cl.

Series.	E.	E_k.	E′.	$E_k - E'$.	Position of the electrodes as in
I.	150	60	26	34	Fig. 2 *a*.
II.	171	111	80	31	
III.	182	142	30	112	Fig. 2 *e*.
IV.	174	132	75	57	

The reason that the values of $E_k - E'$ show so little accordance lies in the impossibility of making two series of experiments under exactly the same conditions. Besides depending on the position of the electrodes, $E_k - E'$ depends, in a more complicated manner, on this—into which part of the flame the potassium-chloride bead is introduced. Nevertheless the above numbers prove that an increase of the electromotive force is produced by the vaporization of the potassium chloride in the flame. This can also be verified on the employment of liquid electrodes.

Different flames being employed, the following values were obtained for the electromotive force when platinum electrodes were introduced in the position fig. 2 *a*:—

Flame.	E.
(1) That of a Bunsen burner . .	180–200
(2) A luminous gas-flame . . .	180–200
(3) Stearine candle-flame	180–200
(4) Spirit-flame	180–200
(5) Magnesium-flame	20– 30
(6) Bisulphide-of-carbon flame . .	85–100

With magnesium the experimental difficulties are very great. Here it could not be ascertained with certainty whether the air-electrode was excited positively, *as was at other times the case with all flames*, or negatively.

§ 9. *Combination of several Flames.*

We have still to mention that flames can be combined in exactly the same manner as galvanic cells—the base of one flame being connected with the apex of the second, the base of the second with the apex of the third, and so on, by wires. Three Bunsen burners, connected in this manner by copper wires, gave the following deflections at the electrometer:—

1 burner . . .	80 scale-divisions.		
2 burners . . .	156	„	(160)
3 „ . . .	245	„	(240)

With perfect equality of the burners the numbers in brackets might have been expected. Twenty-five spirit-flames, combined in this way into a battery, produced at the electrometer a deflection too small to be measured by mirror and scale. At all events the intensity of the current which set in was, on account of the great resistance within the flames, very little. A sufficiently sensitive multiplier to prove the latter point was not at our disposal.

§ 10. *Summary of the Results.*

1. The longitudinal polarization of flame is only apparent, and is called forth by unequal immersion in the flame of the wires employed as electrodes.

2. The flame appears to be strongly polarized in its cross section; and the electrode which is in the stratum of air immediately enveloping the flame is always positive to the electrode in the flame.

3. In agreement with the points 1 and 2, the electromotive force is independent of the magnitude of the flame.

4. Change of polarity of the flame can be called forth by a suitable displacement of the electrodes, and likewise finds its explanation in points 1 and 2.

5. The electromotive force of the flame is dependent on the nature of the metals used as electrodes, and on that of the burning gases. It appears singularly powerfully electric when aluminium or magnesium is made use of as the metal conducting from it; singularly feebly when the electrode in air is covered with a salt (potassium chloride).

6. Unequivocal electrical effects are likewise obtained from the flame when water electrodes are employed and every metal excluded; and the electrode which is in air is likewise positive to that in the flame. All the above propositions can be confirmed with liquids, so far as their nature permits.

7. Flames can be combined after the manner of galvanic

elements; consequently a number of them can be united to form a "flame battery."

§ 11. *Thermoelectrical Behaviour of Platinum Wires separated by a Stratum of Hot Air**.

The phenomena above discussed can be naturally explained both from the thermoelectric and the electric theory; but we soon arrived at the conviction that, so long as experiment was made on the flame itself, a decisive experiment for the one or the other theory could not be instituted, especially if the existence of a peculiar flame-electricity (hitherto excluded from our considerations) were assumed in order to aid in explaining the phenomena. Hence it was necessary to consider the matter from another point of view, and to discover a method by aid of which wires at different temperatures in hot air could be examined as to their respective electrical behaviour. Of course in this case the hot air would not proceed from a flame, and therefore would not be mixed with the products of combustion.

Starting from these views, we employed the apparatus represented in fig. 4. $a\,b$ is a fine platinum wire stretched between two copper wires x and y, which can be rendered incandescent by a battery of two Bunsen elements B. At the point u, its electricity, and with it also that of the battery, was conducted to earth and connected with one pair of quadrants of the electrometer. A second platinum wire, c, was connected with the insulated pair of quadrants, and could be brought to any degree of proximity to the wire $a\,b$. This movable wire was placed so as to be as near as possible to the point u; if a thermoelectric difference then arose from the incandescence of the wire $a\,b$, it was necessarily announced by the electrometer.

There is, however, in this experiment a source of error to be mentioned. As it would be inadmissible, and even (with precision) impossible, to place the wire c exactly opposite to the point u, a difference of potential might also possibly arise from the circumstance that the potential-difference of the points u and r on the stretched wire $a\,b$ traversed by the current would be measured through the intervention of the conductivity of the heated air. In order to be independent of this, a turn-plate W_1 was inserted in the circuit, by which the direction of the current in the wire $a\,b$ could be altered. If

* The electric excitation here occurring is taken into consideration in this place *only so far* as it is *immediately* connected with the electricity of flame. The general treatment of this phenomenon is reserved for a future communication.

with one position of the turn-plate the value of the potential is $+x$, it will be converted into $-x$ by rotating the turn-plate; that is, the direction of the electrometer-deflection must be right or left according to the position of the turn-plate W_1.

The circumstance here discussed, indeed, rendered a small correction necessary. Assuming that the wire c would behave like a platinum electrode introduced into the hot stratum of air of a flame, we can denote the value of the potential upon it by $+e$. To this value $+e$ the value of the potential at the point c will be added or subtracted from it, according to the position of the turn-plate W_1. If we denote by s_1 and s_2 the deflections of the electrometer corresponding to the two positions of the turn-plate, we have

$$e+x=s_1, \quad e-x=s_2,$$

consequently

$$e=\frac{s_1+s_2}{2}.$$

If the earth-conduction is brought, not to the point a, but, say, to the point r, this method still remains applicable; only now x frequently $\geqq e$, which for the moment slightly disturbs the clearness of the experiment.

The result was now obtained that a platinum wire c, brought near to the incandescent wire, received a powerful positive charge, consequently behaved like the base-electrode of the flame. The agreement goes so far that even the values of the electromotive force lie within the same limits. Nay, the analogy between the two phenomena is still closer; for nearly all the experiments above given for the flame can be repeated with such an incandescent wire.

The electromotive force, besides depending on the distance of the wire c from the wire $a\,b$, turns out to be dependent on:—1, the state of incandescence of the wire $a\,b$; 2, the quality of the surface of the wire c.

The correctness of these propositions follows from the following Table, in which the numbers are, for clearness, reduced to equal sensitiveness of the electrometer. (A double deflection of the daniell = 100.)

TABLE III.

Series 1 ($x \geqq e$). Conduction to earth in the point r (fig. 4).

$x+e$.	$x-e$.	e.	Position of the wire c.
+151·0	−10·0	161·0	$\frac{1}{2}$ millim. above $a\,b$ (fig. 4).
+150·6	−16·1	166·7	
+169·5	+ 6·0	163·5	
+155·0	− 3·0	158·0	$\frac{1}{2}$ millim. laterally from $a\,b$.
+163·9	+ 0·8	163·1	

When the wire c was brought half a millimetre below the wire $a\,b$, there was no constant orientation of the electrometer-needle. The following are the maximum values obtained:—

$x+e$.	$x-e$.	e.
129	−9	138

Series 2 ($e>x$). Conduction to earth in the point u. Dependence on the state of incandescence of the wire $a\,b$.

Incandescence of the wire $a\,b$.	$e+x$.	$e-x$.	e.
Dull red	54	42	96
Bright red	86	83	169
White	74	42	116

The last series of experiments show that, with the same position of the wire c ($\frac{1}{2}$ millim. above $a\,b$), the value of e is lower at a white heat than at a bright red heat—a very surprising circumstance, but confirmed by many control experiments.

If the wire c be covered with a coat of potassium chloride, the electromotive force sinks considerably; in one experiment it fell from 188 to 34.

The wire c was now replaced by a water electrode (fig. 3) or a wet string. The experiments showed indubitably that liquids also, separated from glowing platinum by a stratum of hot air, become electrically excited; only this excitation, exactly as with the flame, is much less. The results were, with

	$e+x$.	$e-x$.	e.
c a platinum electrode . .	85	84	169
c a water electrode . . .	26	15	41
c a wet string	26	24	50

Accordingly, from the fact that flames show themselves distinctly electric even when all contact with metals is avoided, it must not at once be concluded that they have an electricity peculiar to them.

Let it be further remarked that, both when the wet string and when the water electrode was employed, the amount of the potential-difference between $a\,b$ and the wire in contact with the liquid of the electrodes, when directly connected by water, was determined before the definitive experiments. The deflection of the electrometer-needle amounted for it to only a few divisions of the scale; so that no source of error could spring from this.

The phenomena here discussed, which had not, to our knowledge, been before observed, stand evidently in the closest con-

nexion with an experiment described by Edlund *—namely, that when the incandescent carbon points of the electric lamp are connected by a multiplier immediately after the extinction of the flame-arc, a strong thermoelectric current is indicated. It follows also from the above-communicated experiments that if the carbon points in the flame-arc are in different degrees of ignition (it is well known that the positive is the hotter; this condition is therefore fulfilled), an electromotive counterforce must arise, the quantity of which essentially depends on the nature of the conductors between which the flame-arc passes—a deduction which has already been verified by Edlund.

The positive electrode, as the hotter, must behave like the incandescent wire in our experiment—that is, be thermoelectrically negatively excited,—which indicates the rise of an electromotive counterforce.

§ 12. *Dependence of the Electricity of Flames on the State of Incandescence of the Electrodes.*

Having thus shown that platinum wires, as well as water electrodes, in contact with hot air are electrically excited, we return to the electricity of flame.

The method discussed in the preceding section permits also the determination of the electromotive force of the element "incandescent platinum, hot air, flame-gases, incandescent platinum," if the wire $a\,b$ is introduced into the stratum of hot air, and the wire c into the apex of the flame.

As long as the wire $a\,b$ (fig. 4) does not glow, it is positive to the wire c in the flame; but as soon as it becomes incandescent a negative value is added to the positive value of the potential; therefore the potential-difference between the two electrodes must undergo a diminution. This inference was completely verified by experiment.

In the following, E denotes the electromotive force of the flame when c is incandescent and the wire $a\,b$ not, and e the electromotive force of the flame when both wires are incandescent. Of necessity e would be $=$E if the incandescence of the wire $a\,b$ had no influence; yet there resulted :—

Series I.		Series II.	
E.	*e*.	E.	*e*.
254	148	134	47
246	147	122	50
216	116	114	37

* Pogg. *Ann.* cxxxi. p. 586 (1850), & cxxxiii. p. 353 (1851).

consequently always a considerable diminution of the electromotive force.

The difference of the numbers in one and the same column arises from the circumstance that for each new determination the wire $a\,b$ was brought into a somewhat different place at the margin of the flame.

An experiment which likewise proves the dependence of the electromotive force on the state of incandescence of the electrodes, but which is not so free from objection as the above, is the following:—The electrode S (fig. 2 *a*) was replaced by a platinum pan, and the electromotive force E determined. Water was then introduced into the red-hot pan, and, as soon as it boiled, the quantity E measured again. When the whole of it was evaporated and the pan again red-hot, the first experiment was repeated for a control. In this the turn-plate W (fig. 1) was placed so that the platinum pan was conducted-from to earth. A long series of experiments constantly gave the same result, namely a considerable diminution of the electromotive force with diminution of the temperature of the pan. For example,

(1)	With the pan red-hot		E=216
	„ „ at 100°		E=152
	„ „ red-hot (control-experiment) .		E=213
(2)	„ „ red-hot		E=196
	„ „ at 100°		E=118
	„ „ red-hot (control-experiment) .		E=197

These experiments, without the confirmation afforded by the preceding experiment, did not appear to us definitive, because an alteration of E might possibly be effected by the evaporation of the water and by the wetting of the outside of the pan. But from the former experiment it was already evident that the electromotive force is the greater the greater the difference of temperature between the electrode in the flame and that in the air.

§ 13. *Thermoelectrical Behaviour of Wires within a Flame.*

In contradiction to the fundamental experiments adduced in the last section stands the fact that, in spite of great differences of temperature, no thermoelectric excitation takes place when *both* electrodes dip equally into the flame. With the electrodes arranged as represented in fig. 2 *c* it is easy to place the electrode B so that it does not glow while S is intensely white-hot; but in spite of this the electrical forces which arise are very slight, as the following experiment shows:—

Electrode S (fig. 2 *c*) white-hot. Base-electrode B

In the flame (fig. 2 *c*).		In air (fig. 2 *a*).
Dark . . .	$E' = +4$	
Red-hot . .	$E' = +3$	$E = +150$
White-hot . .	$E' = -6$	

The sign prefixed refers to electrode B.

Also when the electrodes are in one cross section of the flame and at the same time dip completely into it, in spite of great differences of temperature the electromotive force is very small. If the wires *a b* and *c* (fig. 4) were brought into the coolest (lowest) part of an alcohol-flame, E was ascertained to be 23. By a suitable shifting of the wire *c* this could be reduced to 6, notwithstanding that *c* was not red-hot while *a b* was put into a dazzling white incandescence by an electric current. The reason for this surprising behaviour appears to be that the flame-gases, being relatively good conductors in comparison with the hot air, prevent the electrical difference from being completed. This would also be confirmed by the fact that, in the experiments with wires in air, the maximum is found when the wire *a b* is bright red-hot. White-hot wires, when the electrodes are in the same position, constantly give lower values for the electromotive force, as we have already mentioned above—which, we think, can only be accounted for by the conductivity of the surrounding air being so augmented by the strong heating that it forms as it were a secondary closing of the circuit.

§ 14. *Cases in which the Thermoelectric Excitation predominates over the Electrolytic, and* vice versâ.

The view last discussed explains also very naturally the experiment mentioned in § 4, that the electromotive force rises from 12 up to 190 as soon as the electrode B (fig. 2 *a*) is drawn quite out of the flame. The moment this takes place, the secondary closing formed by the flame is removed, and the thermoelectric force corresponding to the temperature-differences of the electrodes comes fully into action.

If this explanation be not admitted as the correct one, it may appear doubtful if *with a flame* the thermoelectric excitation does not entirely *fall away*, and the electrolytic exclusively condition the electric behaviour of the flame. But if the experiments related in § 12 tell against this, others also can be instituted which it would be difficult to explain without admitting a thermoelectric excitation. They are the following:—

If the apex of the flame be conducted-from to earth by a

platinum pan, and a fine platinum wire, dazzlingly white-hot, be brought below it into the extreme margin of the flame, the platinum wire shows free *positive* electricity. According to the previous experiments, however, a white-hot wire ought to appear *negatively* charged with respect to the red-hot pan. In this case, therefore, the electrolytic excitation outweighs the thermoelectric. But if the difference of temperature between the two electrodes be made still greater (which can easily be done by cooling the platinum pan with water), *the polarity of the flame is reversed*, the white-hot platinum wire is consequently now negative, as the thermoelectric theory requires it to be.

In an experiment of this sort the following values were obtained:—

(1)	Pan red-hot	(−)	E = +20
	Platinum wire white-hot .	(+)	
(2)	Pan cooled by H_2O . .	(+)	E = −32
	Platinum wire as above .	(−)	

The values of E are considerably lower, because in this experiment *both* electrodes are in the flame. At the same time the margin appears *negative* to the interior of the flame—a behaviour which cannot be observed under ordinary conditions.

It can, further, be shown that an intensely white-hot platinum wire *in air* is negative to one not red-hot *in the flame*. If the wire *a b* (fig. 4) was stretched at about 3 millim. distance from the margin of the flame, and *c* introduced into the foot of the flame so as to be completely enveloped by the flame-gases but at the same time not to become red-hot, then likewise the polarity of the flame was reversed as soon as *a b* was rendered brilliantly incandescent by the current. With the wire *a b* dark the electromotive force amounted to about 1 daniell; with it white-hot, to about 0·17–0·2 daniell, but with the deflection in the opposite direction to the former: therefore in this case the thermoelectric again outweighs the electrolytic excitation.

From these and all the preceding experiments it follows that we cannot explain the electric behaviour of flame by assuming either an exclusively thermoelectric or an exclusively electrolytic excitation, but that we must perforce regard *both* as cooperating in producing the total electrical state of the flame.

15. *On the proper Electricity of the Flame.*

If we imagine two platinum wires introduced into a flame and the stratum of air which envelopes it, there arises, accord-

ing to the above conception, a thermoelectrico-electrolytic element composed of

Cold platinum | Hot air + Hot air | Flame-gases + Flame-gases | Red-hot platinum.

If we fix our attention upon the single members of this combination, it is proved by the experiments that an electric excitation takes place between cold platinum with hot air, on the one hand, and incandescent platinum with flame-gases, on the other; while the question is still undecided whether an electrical difference exists between the hot air and the flame-gases even without wires or liquids being in contact with those gases. This question is identical with that whether a proper electricity does or does not belong to flame.

In order to bring this point also to a decision, let us here adduce a few more experiments, *which decidedly speak against the existence of a proper electricity of flame.*

On the hypothesis that to the stratum of air A A′ (fig. 1) enveloping the flame a certain quantum of positive electricity is brought by the process of combustion or by mere contact, at least a partial equalization of the electricities must take place, even if we take into consideration the bad conductivity of the two strata of gas, as soon as one or more well-insulated wires are passed transversely through the flame. But the potential-difference existing between the electrodes S and B (fig. 1) is not at all or only very slightly altered thereby. Let E be the electromotive force without the transverse wire, and E_D the electromotive force with it (platinum).

One series of experiments gave

E = 169·0	164·8	168·4	mean 167·4,
E_D = 162·0	161·2	.....	mean 161·6,

consequently a diminution of about 3 per cent.

A second series, when two very carefully cleaned and well-insulated platinum wires were passed transversely through the flame, gave:—

	E.	E_D.	$E-E_D$.
Experiment I.	168·5	168·0	+0·5
II.	158·4	158·9	−0·5
III.	182·0	182·0	0·0

Each of the above numbers is the mean of five readings; and the position of the electrodes was somewhat altered from one experiment to another; hence the difference in the numbers in the same column. Therefore a partial equalization of

the electricity does not take place through the introduction of the transverse wires.

Further, in contradiction to the existence of a proper electricity of flame is the fact that when platinum wires as homogeneous as possible are used as electrodes, and are also approximately in the same state of incandescence, the electromotive force of the flame sinks to a minimum. The electromotive force of the combination

White-hot platinum | Hot air + Hot air | Flame-gases + Flame-gases | White-hot platinum

was ascertained to be 0·0013 daniell—a value which lies within the limits of errors of observation, and consequently may be put = 0. In this determination, of course, it was necessary to employ the method discussed in § 11.

For e the following values were obtained:—

$e+x$.	$e-x$.	e.	
+9·5	−9·0	+0·5	Daniell = 100;
−1·7	−0·2	−1·9	
+7·3	−5·5	+1·8	

consequently $e = 0{\cdot}0013$ daniell. According to this, the electromotive force of the member

Hot air | Flame-gases

may be put = 0, and consequently a proper electricity of the flame be left out of consideration.

The most important circumstance contradicting the existence of a proper electricity of flame may be that the reversal of the polarity of the flame is not connected with the reversal of the combustion-process.

A flame of air burning in an atmosphere of illuminating-gas exhibits the same polarity as illuminating-gas burning in air.

An incandescent platinum wire in burning air was strongly negative to the metal (copper) out of which the flame issued; and a second wire, introduced into its sensitive stratum, received a strong positive charge, just like the base-electrode of an ordinary flame. The details of this experiment were as follows:—

First the electromotive force of an ordinary gas-flame issuing from a glass tube provided with a copper jet, with the electrodes in a determined position (fig. 2 *a*), was measured. A current of air was then passed through the tube, which was placed in a space filled with illuminating-gas, and the air-current ignited by the spark of an induction-apparatus. No source of error was given rise to by this (as we convinced

ourselves by numerous preliminary experiments), since the two electrodes were metallically connected with one another and with the earth before the flame was kindled. As the shape of the small bluish flame was quite different from that of a gas-flame in air, it was necessary to shift the base-electrode somewhat, in order to attain an analogous position to that in the first experiment.

For the electromotive force of the

Gas-flame in air, we found E = 148
Air-flame in gas, „ E = 152

In both cases the glowing wire was negative, the not glowing one positive, consequently the apparent polarity of the flame the same.

Lastly, another noteworthy circumstance should be mentioned: namely, the polarity of the air-flame appears reversed when the flame-electrode is in the lowest and, therefore, coolest part and is not red-hot. We have seen, in § 13, that, with an analogous position of the electrodes in an ordinary flame, the dark wire in the flame was always negative to one in hot air. There we had the combination

Platinum, *Hot gas*, *Hot air*, Platinum.

But with the air-flame we have

Platinum, *Hot air*, *Hot gas*, Platinum,

consequently the same elements in inverse order, from which the reversal of the polarity of the flame results spontaneously.

§ 16. *Theory and Conclusions.*

On the basis of the above experiments the following theory on the electricity of flame can be set up.

By the process of combustion in itself free electricity within the flame is not generated; on the other hand, the flame-gases and the air stratum immediately enveloping the flame possess the property, when in contact with metals or liquids, of exciting it similarly to an electrolyte. To this electrolytic excitation is added a thermoelectric excitation, produced by the state of incandescence of the electrodes. The quantity and kind of the electric excitation is then

(1) Independent of the size of the flame;

(2) Dependent on the nature, and the quality of the surface, of the electrodes;

(3) Dependent on the nature of the combustion-gases;

(4) Dependent on the state of incandescence of the electrodes.

These conclusions are confirmed by numerous experiments; and no experiment has been found to contradict them.

The decision in favour of this theory was supplied by the fact that glowing and cold wires separated only by heated air, with the exclusion of combustion-gases, showed an electrical difference. Here also the latter is dependent on the nature, and the quality of the surface, of the electrodes employed, and on their state of incandescence. A too strong heating of the wires, and, therewith, also of the separating air stratum, proved unfavourable to the development of free electrical tension—a circumstance probably due to the augmentation of the conducting-power of that separating stratum. In accordance with this, wires introduced into the flame, so long as they are both immersed in the combustion-gases (which are relatively good conductors), never give the maximum of potential-difference; rather this enters only when one of the wires comes into contact with only the outer air stratum of the flame (which is endowed with a very high resistance).

The occurrence of a thermoelectric counterforce within the galvanic flame-arc is also naturally explained by the above theory.

The questions proposed at the commencement are therefore to be answered thus:—Hankel's theory is not in accordance with experiment; and the two kinds of excitation assumed by Buff and Matteucci must be regarded as *simultaneously* causing the apparent electricity of flame.

Wolfenbüttel, February 1882.

XX. *On the Equilibrium of Liquid Conducting Masses charged with Electricity.* *By* Lord Rayleigh, *F.R.S.**

IN consequence of electrical repulsion, a charged spherical mass of liquid, unacted upon by other forces, is in a condition of unstable equilibrium. If a_0 be the radius of the sphere, Q the charge of electricity, the original potential is given by

$$V = \frac{Q}{a_0}.$$

If, however, the mass be slightly deformed, so that the polar equation of its surface, expressed by Laplace's series, becomes

$$r = a(1 + F_1 + F_2 + \ldots + F^n + \ldots),$$

* Communicated by the Author.

then

$$V = \frac{Q}{a_0}\left\{1 - \Sigma(n-1)\iint \frac{F_n^2 d\sigma}{4\pi}\right\};$$

and the potential energy of the system reckoned from the equilibrium position is

$$P' = -\frac{Q^2}{8\pi a_0}\Sigma(n-1)\iint F_n^2 d\sigma.$$

In actual liquids this instability, indicated by the negative value of P′, is opposed by stability due to the capillary force. If T be the cohesive tension, the potential energy of cohesion is given by

$$P = \tfrac{1}{2}a_0^2 T\,\Sigma(n-1)(n+2)\iint F_n^2 d\sigma\,^*.$$

If $F_n \propto \cos(pt+\epsilon)$, we have for the motion under the operation of both set of forces,

$$p^2 = \frac{n(n-1)}{\rho a_0^3}\left\{(n+2)T - \frac{1}{4\pi}\frac{Q^2}{a_0^3}\right\}.$$

If $T > \dfrac{Q^2}{16\pi a_0^3}$, the spherical form is stable for all displacements. When Q is great, the spherical form is unstable for all values of n below a certain limit, the maximum instability corresponding to a great, but still finite, value of n. Under these circumstances the liquid is thrown out in fine jets, whose fineness, however, has a limit.

The case of a cylinder, subject to displacement in two dimensions only, may be treated in like manner.

The equation of the contour being in Fourier's series

$$r = a(1 + F_1 + \ldots + F_n + \ldots),$$

we find as the expression for the potential energy of unit length

$$P' = -\frac{Q^2}{l^2}\Sigma(n-1)\int \frac{F_n^2 d\theta}{2\pi},$$

Q being the quantity of electricity resident on length l.

The potential energy due to capillarity is

$$P = \tfrac{1}{2}\pi a T\,\Sigma(n^2-1)\int \frac{F_n^2 d\theta}{2\pi},$$

and for the vibration of type n under the operation of both

* See Proc. Roy. Soc. May 15, 1879.

sets of forces,

$$p^2 = \frac{n^2 - n}{\rho a^3}\left\{(n+1)\,\mathrm{T} - \frac{2\mathrm{Q}^2}{\pi l^2 a}\right\}.$$

The influence of electrical charge in diminishing the stability of a cylinder for transverse disturbances may be readily illustrated by causing a jet of water from an elliptical aperture to pass along the axis of an insulated inductor-tube, which is placed in connexion with an electrical machine. The jet is marked with a recurrent pattern, fixed in space, whose wave-length represents the distance travelled by the water in the time of one vibration of type $n=2$. When the machine is worked, the pattern is thrust outwards along the jet, indicating a prolongation of the time of transverse vibration. The inductor should be placed no further from the nozzle than is necessary to prevent the passage of sparks, and must be short enough to allow the issue of the jet before its resolution into drops.

The value of T being known (81 C.G.S.), we may calculate what electrification is necessary to render a small raindrop of, say, 1 millimetre diameter unstable. The potential, expressed in electrostatic measure, is given by

$$\mathrm{V} = \frac{\mathrm{Q}}{a_0} = \sqrt{(16\pi a_0 \mathrm{T})} = 20.$$

The electromotive force of a Daniell cell is about ·004; so that an electrification of about 5000 cells would cause the division of the drop in question.

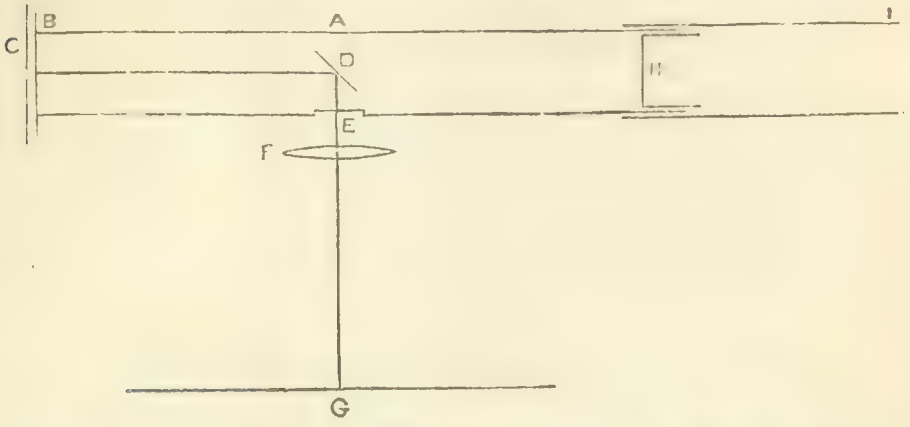
XXI. *On an Instrument capable of Measuring the Intensity of Aerial Vibrations.* *By* Lord Rayleigh, *F.R.S.**

THIS instrument arose out of an experiment described in the 'Proceedings of the Cambridge Philosophical Society'†, Nov. 1880, from which it appeared that a light disk, capable of rotation about a vertical diameter, tends with some decision to set itself at right angles to the direction of alternating aerial currents. In fig. 1, A is a brass tube closed at one end with a glass plate B, behind which is a slit C backed by a lamp. D is a light mirror with attached magnets, such as are used for reflecting-galvanometers, and is suspended by a silk fibre. The light from the slit is incident

* Communicated by the Author.

† See also Proc. Roy. Soc. May 5, 1881, p. 110.

upon the mirror at an angle of 45°, and, after reflection, escapes from the tube through a glass window at E. It then



falls upon a lens F, and throws an image of the slit upon a scale G. At a distance DH, equal to DC, the tube is closed by a diaphragm of tissue paper, beyond which it is acoustically prolonged by a sliding tube I.

When the instrument is exposed to sounds whose half wave-length is equal to C H, H becomes a node of the stationary vibrations, and the paper diaphragm offers but little impediment. Its office is to screen the suspended parts from accidental currents of air. At D there is a loop; and the mirror tends to set itself at right angles to the tube under the influence of the vibratory motion. This tendency is opposed by the magnetic forces; but the image upon the scale shifts its position through a distance proportional to the intensity of the action.

As in galvanometers, increased sensitiveness may be obtained by compensating the earth's magnetic force with an external magnet. Inasmuch, however, as the effect to be measured is not magnetic, it is better to obtain a small force of restitution by diminishing the moment of the suspended magnet, rather than by diminishing the intensity of the field in which it works. In this way the zero will be less liable to be affected by accidental magnetic disturbances.

So far as I have tested it hitherto, the performance of the instrument is satisfactory. What strikes one most in its use is the enormous disproportion that it reveals between sounds which, when heard consecutively, appear to be of the same order of magnitude.

June, 1882.

XXII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part VI. *By* C. R. Alder Wright, *D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School**.

On the Relations between the Electromotive Forces of various kinds of Cells analogous to Daniell's Cell but differing therefrom in the nature of the Metals used, and the Chemical Affinities involved in the Action of these Cells.

I. *Cells containing Cadmium as one of the Metals, the Salts used being Sulphates.*

118. THE experiments described in Part V. (§§ 106–109) were repeated, using, instead of normal Daniell cells, analogous arrangements containing plates of cadmium, opposed in some instances to copper, in others to zinc, solutions of the respective sulphates being employed to surround the various plates used. With each of these two classes of cells (cadmium-copper and zinc-cadmium cells) the same result was obtained as that already recorded in the case of Daniell cells containing zinc- and copper-sulphate solutions—viz. that so long as the two solutions are of the same strength† the actual state of concentration of the fluids does not exert any appreciable influence on the E.M.F. generated with given plate-surfaces; at least the influence exerted is considerably less than the errors of observation and the variations due to unavoidable variations in the nature of the plate-surfaces, and does not amount to as much as $\pm$·0015 volt even

* Communicated by the Physical Society, having been read at the Meeting on June 24, 1882.

† It is convenient to define solutions "of the same strength" not as solutions of the kind usually spoken of by chemists as "equivalent" to one another, *i. e.* containing in a given volume quantities of dissolved matter in the ratio of the chemical equivalents of the substances dissolved (*e. g.* 159·5, 161, and 208 parts of anhydrous copper, zinc, and cadmium sulphate respectively), but as solutions in which *the dissolved matter and the water present are in the same molecular ratio*, i. e. which are expressible by parallel formulæ, such as $CuSO_4 . 50H_2O$, $ZnSO_4 . 50H_2O$, and $CdSO_4 . 50H_2O$. With weak solutions the two definitions are practically the same—but not so with more concentrated fluids, especially when the molecular weights of the dissolved matters are considerably different (like $CuSO_4$ and $CdSO_4$). Solutions of zinc and copper sulphate of the same molecular strength are practically identical in specific gravity; but a solution of cadmium sulphate is considerably more dense than one of either zinc or copper sulphate of the same molecular strength. Thus solutions of the strengths $ZnSO_4 . 50H_2O$, $CuSO_4 . 50H_2O$, $CdSO_4 . 50H_2O$ have at 18° specific gravities respectively close to 1·170, 1·167, and 1·208; with stronger solutions the excess of density of the cadmium solution is still more apparent.

Fig 1

Fig 4

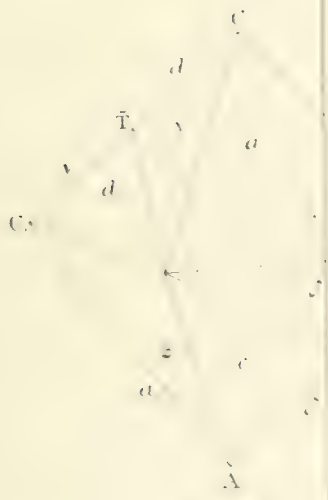


Fig 5

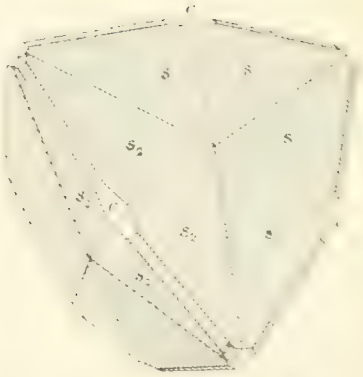


Fig 8

Fig 9

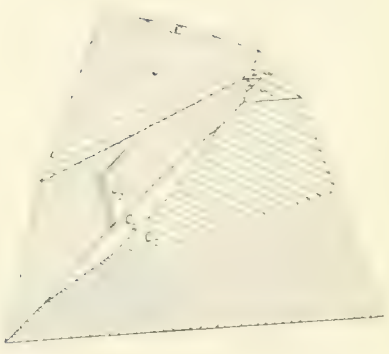


Fig: 12.

when tolerably concentrated solutions of strength $MSO_4 50H_2O$ are compared with similar solutions of only one twenty-fifth the strength, $MSO_4 1250H_2O$.

On varying the nature of the surface of the cadmium plate (by employing bright cast metal, electro-deposited cadmium, or amalgamated cadmium), it was found that whatever result was produced in the cadmium-copper cells by a given alteration of the cadmium plate, every thing else remaining unaltered, *precisely the same numerical result, but with the opposite sign, was produced in the zinc-cadmium cells* by that alteration. Thus, substituting electro-cadmium for bright cadmium plates in the cadmium-copper cells caused an *increase* in the E.M.F. varying from ·002 to ·006 volt in numerous experiments, and averaging ·004 volt; whilst with the zinc-cadmium cells the same substitution caused a *decrease* in the E.M.F. varying between almost the same limits, ·002 and ·007 volt, and averaging almost the same value as before, viz. ·0045 volt. Similarly, on substituting amalgamated cadmium plates for bright cadmium in the cadmium-copper cells, the average effect was a *decrease* of ·0415 volt when the mercurial amalgam was fluid, and of ·015 volt when it had become solid and crystalline on standing; whilst with the zinc-cadmium cells, substitution for bright cadmium of amalgamated metal caused on an average an *increase* in E.M.F. of ·043 volt when the amalgam was fluid, and of ·016 volt when it had become solid and crystalline.

Cells containing Cadmium opposed to Copper.

119. On comparing together a number of similar pairs of cells containing in the one case electro-copper and in the other amalgamated copper, it was found that the average difference was sensibly the same as that observed when the same two kinds of copper plates were opposed to zinc (§ 107), viz. that, *cæteris paribus*, the cell containing amalgamated copper read on an average ·001 volt lower than the one containing freshly deposited electro-copper: the actually observed differences ranged from +·003 to −·003 volt, but were more usually negative.

As just stated, when the cadmium plate was amalgamated a decrease in E.M.F. was brought about, averaging ·0415 volt when the amalgam on the surface of the plate was fresh and perfectly fluid, and ·015 when perfectly solid and crystalline. Intermediate numbers were given by plates on the surface of which crystallization of the amalgam had begun but was not complete, the gradation being regular as the crystallization progressed.

The following table gives the average result, in volts*, of upwards of forty series of observations and comparisons, mostly extending over three to four hours, during which time the readings of each particular cell remained sensibly constant:—

Variation in E.M.F. due to the use of cadmium and copper sulphate solutions of different strengths, both solutions being of equal molecular strengths in any given case (strengths varying from $MSO_4\ 47H_2O$, to $MSO_4\ 1250H_2O$)†	Less than ±·0015.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for electro-copper:—				
Amalgamated copper (surface wet with liquid mercury)	+·003	−·003	·006	−·001
Effect of substituting for bright cadmium:				
Fresh electro-cadmium	+·006	+·002	·004	+·004
Amalgamated cadmium (surface wet with liquid mercury)	−·050	−·033	·017	−·0415
Amalgamated cadmium (solid and crystalline)	−·020	−·005	·015	−·015
Electromotive force of combinations:—				
Electro-copper—Electro-cadmium	·756	·750	·006	·7525
,, ,, Bright cadmium	·753	·745	·008	·7485
,, ,, Amalgamated cadmium (liquid amalgam)	·717	·701	·016	·707
,, ,, Amalgamated cadmium (solid amalgam)	·740	·727	·013	7335
Amalgamated copper—Electro-cadmium	·754	·749	·005	·7515
,, ,, Bright cadmium	·752	·744	·008	·7475
,, ,, Amalgamated cadmium (liquid amalgam)	·715	·701	·014	·706
,, ,, Amalgamated cadmium (solid amalgam)	·737	·727	·010	·7325

When cells containing bright or electro-cadmium and electro-copper plates were allowed to stand for twelve hours or

* All the observations given in this paper are reduced to the same standard as that employed in Part V.—viz. the average reading at 15°·5 of a number of Clark's cells taken as 1·457 volt, the particular Clark's cells used being the same throughout.

† The specific gravities at about 15° of these fluids are close to the following:—

$MSO_4\ 47H_2O$ when M is cadmium: spec. grav. = 1·217.
do. when M is copper: spec. grav. = 1·175; solution nearly saturated.

$MSO_4\ 1250H_2O$: in each case below 1·01.

longer periods, a slight alteration in the E.M.F., due to formation of films of oxide on the surfaces of the plates, was usually noticeable. As with the normal Daniells (§ 108), the effect of the oxidation of the copper plate was to reduce the E.M.F. by a few thousandths of a volt; on the other hand, the formation of a film of oxide on the surface of the cadmium plate produced an *increase* in the E.M.F. of from ·001 to ·004 volt; so that in many cases the cell with partially oxidized plates gave sensibly the same value as a newly set-up cell, the diminishing effect of the oxidation of the copper being just about counterbalanced by the increasing effect due to the oxidation of the cadmium. In this respect cadmium behaves in the opposite way to zinc (§ 108).

Relations between the E.M.F. of Cadmium-Copper Cells and that corresponding to the net Chemical Action taking place therein.

120. According to Julius Thomsen's determinations (*Journ. prak. Chem.* ii. p. 233, and xi. p. 271), the heat of displacement of copper from copper-sulphate solution ($CuSO_4$,400H_2O) by cadmium is as follows, in gramme-degrees per gramme-molecule:—

Cd, O, SO_3 aq. . . .	89,500
Cu, O, SO_3 aq. . . .	55,960
Difference =	33,540

the difference corresponding to 16,770 gramme-degrees per gramme equivalent, or ·740 volt*. As with normal Daniell cells (§ 114), a small quantity (x) is to be added to this, representing a variable correction dependent on the physical condition of the deposited copper. Evidently the average values above cited (·7475 to ·7525), obtained with bright and electro-cadmium, are sensibly the same as the value ·740 + x, thus deduced as representing the net chemical action taking place in the cell; *i. e.*, as with zinc-copper cells, the whole of the energy developed in the cell is adjuvant under the conditions obtaining in the above experiments.

In order to compare the results obtained with the amalgamated-cadmium cells with Julius Thomsen's figures, the heat of solution of cadmium (precipitated from the sulphate by zinc, crystalline) in twenty-five times its weight of mercury was determined by means of the calorimeter, 20 grams of

* The value 4410, used in the former parts of these researches for the factor for converting gramme-degrees into C.G.S. units, is employed throughout the present paper; *vide* § 103, footnote.

cadmium and 500 of pure mercury being employed for each experiment. To insure solution it was found necessary to wash the cadmium with dilute sulphuric acid just before use; otherwise portions remained unwetted and undissolved by the mercury. The final result arrived at as the average of several concordant observations was, that an evolution of heat to the extent of 610 gramme-degrees per gramme-molecule (112 grammes) of cadmium took place during solution. Hence, were cadmium sulphate formed from mercurial solution of metal instead of crystalline precipitated metal, the heat of formation expressed as Cd, O, SO_3 aq. would be $89{,}500-610=88{,}890$ (admitting that Thomsen's value 89,500 applies, without correction, to the metal in the crystalline condition of that experimented with). Consequently the heat of displacement of copper by cadmium from the sulphate is 32,930 per gramme-molecule, or 16,465 per gramme-equivalent, corresponding to ·726 volt. The observed values varied between ·701 and ·717, averaging ·707 with electro-copper and ·706 with amalgamated copper—again not differing from the value deduced from the thermal data by an amount materially outside the limits of experimental errors, especially those due to variation in the heat of formation of salts according as the physical state of the metal employed varies.

It is, however, to be noticed that the above heat of solution of crystalline cadmium in mercury only corresponds to an E.M.F. of ·0135 volt; whilst the average difference in E.M.F. caused by the substitution of fluid amalgamated cadmium for crystalline electro-metal was $·7525-·707=·0455$ volt, a considerably greater amount; so that amalgamating the crystalline metal appears to produce a greater effect on the E.M.F. than corresponds to the heat of solution. Just the same result is produced when cadmium and zinc are opposed (§ 121); on the other hand, the effect on the E.M.F. of amalgamating silver is sensibly the same as that corresponding to the heat of solution of silver in mercury (§ 129). Probably the difference in the cases of silver and cadmium is due to the oxidizability of the latter by dissolved air, thus rendering the outer surface of the crystalline masses somewhat different from the interior.

Cells containing Cadmium opposed to Zinc.

121. The following table exhibits in brief the results of upwards of thirty series of observations, mostly lasting over several hours, during which period the E.M.F. developed by any given cell remained sensibly steady:—

Variation in E.M.F. due to the use of cadmium and zinc sulphate solutions of different strengths, both solutions being of equal molecular strengths in any given case (strengths varying from $MSO_4\,50\,H_2O$ to $MSO_4\,1250\,H_2O$)	Less than $\pm$·0015 volt.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for bright cadmium:—				
Fresh electro-metal	−·002	−·007	·005	−·0045
Amalgamated cadmium (liquid)	+·052	+·036	·016	+·044
„ „ (solid, crystalline)	+·024	+·009	·015	+·0165
Electromotive force of combinations:—				
Amalgamated zinc—Bright cadmium	·367	·361	·006	·364
„ „ Electro-cadmium	·362	·358	·004	·360
„ „ Amalgamated cadmium (liquid)	·414	·401	·013	·4075
„ „ Amalgamated cadmium (solid)	·388	·373	·015	·3805

These figures accord closely with the results deducible from Julius Thomsen's thermochemical data, together with the heat of solution of cadmium in mercury above quoted (§ 120); thus:—

	Free metallic cadmium.	Cadmium dissolved in mercury.
Zn, O, SO_3 aq. . . .	=106090	106090
Cd, O, SO_3 aq. . . .	= 89500	88890
Difference . .	16590	17200
Difference per gramme-equivalent . . .	8295	8600
Corresponding with volt	·365	·379

The observed electromotive forces* thus do not differ from

* Regnault has shown (*Ann. de Chim. et de Phys.* [3] xliv. p. 453) that the E.M.F. of a cell containing "concentrated" solutions of zinc and cadmium sulphates and plates of these metals was 55, when that of a similar cell with zinc and copper sulphates and plates was 175 (a particular thermopile being employed as unit). Taking the E.M.F. of the latter cell as 1·115 volt, that of the former must have been ·350 volt—a value differing from those observed by an amount not outside that possibly due to inequality in the molecular strengths of the two metallic solutions. For, by the use of more dilute cadmium-sulphate solutions (the zinc-sulphate solution remaining the same) an appreciable fall in E.M.F. was found to be produced, the lowest value being 42=·268 volt with solution diluted to $\frac{1}{100}$; on the other hand, decreasing the strength of the zinc-sulphate solution produced far less effect. These and the author's somewhat different results on this point will be discussed in a future paper.

those corresponding with the thermal values by amounts materially outside the experimental errors. As with the copper-cadmium cells, however, the observed difference in E.M.F. between electro-cadmium (crystalline) and amalgamated cadmium (liquid) is notably greater than that corresponding with the heat of solution of precipitated crystalline cadmium in mercury, being ·407−·3595=·0475 as compared with ·0135 volt.

Volta's Law of Summation of Electromotive Forces.

122. The foregoing experiments clearly show that, as far as cells containing zinc, cadmium, and copper plates are concerned, Volta's law of summation holds, at any rate when the plates are immersed in solutions of their respective sulphates, the solutions being of equal molecular strength; that is, the sum of the electromotive forces generated with a given pair of zinc and cadmium plates, and with that same cadmium plate and a given copper plate, is equal to the E.M.F. generated with the given zinc and copper plates; or, otherwise,

$$\left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Cd}\end{matrix}\right. + \left\{\begin{matrix}\mathrm{Cd}\\ \mathrm{Cu}\end{matrix}\right. = \left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Cu}\end{matrix}\right.$$

where the symbol $\left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Cd}\end{matrix}\right.$ represents the E.M.F. generated with a given kind of zinc plate opposed to a given kind of cadmium plate, each plate being immersed in a solution of its sulphate of constant molecular strength.

Thus the average results for Daniell cells quoted in Part V. and the above figures give the following comparisons:—

Nature of Plate-surfaces.			Electromotive Force developed.			Normal Daniell.
Zinc.	Cadmium.	Copper.	Zinc-Cadmium.	Cadmium-copper.	Sum.	
Amalgamated.	Electro.	Electro.	·3595	·7525	1·112	Amalgamated zinc and electro-copper 1·111 to 1·116, averaging 1·114.
,,	Bright.	,,	·364	·7485	1·1125	
,,	Amalgamated (fluid).	,,	·407	·707	1·114	
,,	Amalgamated (solid).	,,	·380	·7335	1·1135	
				Mean ...	1·1130	
Amalgamated.	Electro.	Amalgamated.	·3595	·7515	1·111	Amalgamated zinc and amalgamated copper 1·110 to 1·115, averaging 1·113.
,,	Bright.	,,	·364	·7475	1·1115	
,,	Amalgamated (fluid).	,,	·407	·706	1·113	
,,	Amalgamated (solid).	,,	·380	·7325	1·1125	
				Mean ...	1·1120	

A number of direct experiments were also made on this point, using *twin cells* constructed as follows:—Three beakers were arranged containing solutions of copper, cadmium, and zinc sulphates of the same molecular strengths, and plates of electro-copper, bright (or electro-) cadmium, and amalgamated zinc respectively. The copper and cadmium beakers were connected by a siphon tube (with ends covered with bladder) filled with the cadmium sulphate solution; and the cadmium and zinc beakers were similarly connected by a siphon tube containing the zinc sulphate solution. The copper, cadmium, and zinc plates were then connected with cups Nos. 1, 2, and 3 respectively of a switch-board like that represented in fig. 3, Part V. (§ 106); so that by connecting cups 1 and 2 with the electrometer the E.M.F. of the cadmium-copper cell was determined, whilst when cups 2 and 3 were connected the E.M.F. of the cadmium-zinc combination was determined. These readings having been made several times, the zinc and copper plates were transferred to another pair of beakers, containing the same zinc and copper sulphate solutions united by a zinc-sulphate siphon, so as to constitute a normal Daniell cell after Raoult's pattern, and the E.M.F. of this combination determined. Several pairs of zinc and copper plates were thus used—each pair being read first in the zinc-cadmium-copper combination, then in the normal Daniell cell, and then again in the ternary combination. In each case the difference between the sum of the average electromotive forces of the zinc-cadmium and cadmium-copper couples differed from that of the zinc-copper combination by quantities no greater than the errors of observation of the electrometer-scale (about $\pm 0{\cdot}1$ per cent. when a sufficient number of readings were taken); whilst the average of the small differences observed with different pairs was actually 0, *the small + and − differences due to errors of observation completely balancing one another.* This final result (that no discernible difference was to be found between the sum of zinc-cadmium and cadmium-copper couples, and zinc-copper couples containing the same plates) was obtained in each of several sets of experiments made respectively with solutions of molecular strength $MSO_4\,47\,H_2O$, $MSO_4\,100\,H_2O$, and $MSO_4\,1250\,H_2O$.

Rate of Fall in E.M.F. through so-called Polarization occurring in Zinc-Cadmium and Cadmium-Copper Cells for definite amounts of Increase in the Rates of Current-flow.

123. The experiments made with normal Daniell cells described in Part V. (§§ 103–105) were repeated with zinc-cadmium and with cadmium-copper plates (exposing surfaces

of 2·5 and 5·0 square centimetres). The results were similar in character to those obtained with the Daniell cells, no appreciable falling-off in E.M.F. occurring with a current-density of less than some 5 to 10 microampères per square centimetre of plate-surface, but very considerable amounts being observed with stronger currents.

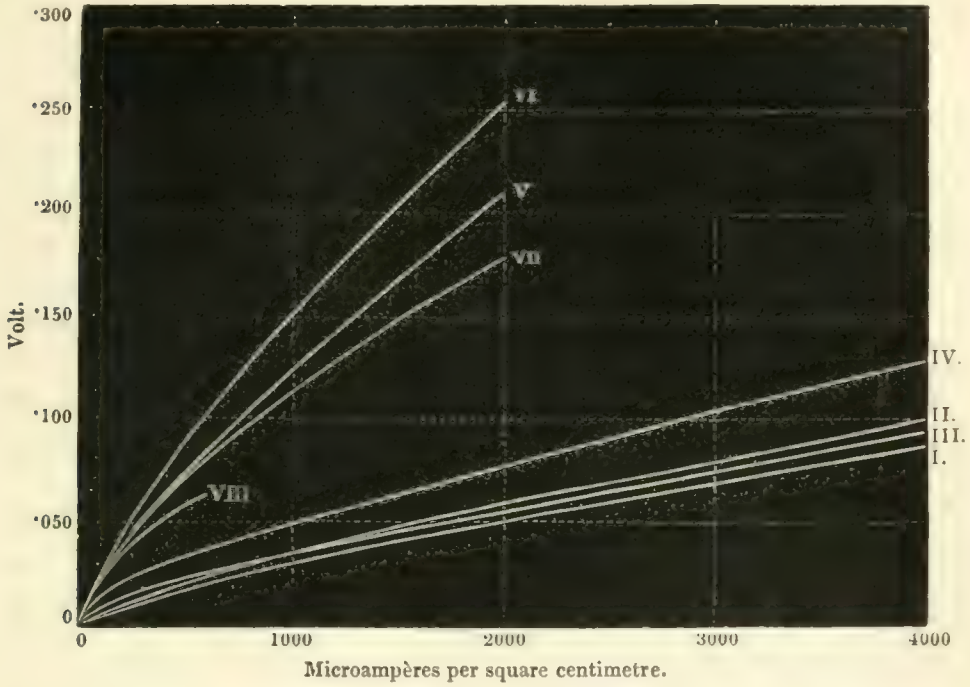
Thus the following table exhibits the values obtained in four experiments—the first three with zinc-cadmium plates, the fourth with cadmium-copper plates, solutions of sulphates of the respective metals employed being used throughout:—

Zinc plates	Amalgamated.	Amalgamated.	Amalgamated.	.....
Cadmium plates ...	Electro	Amalgamated. Fluid amalgam.	Amalgamated. Solid amalgam.	Bright.
Copper plates	.....	.....	.....	Electro.
Sp. gr. of zinc sulphate solution...	1·17	1·17	1·17	.....
Sp. gr. of cadmium sulphate solution	1·25	1·32	1·32	1·32
Sp. gr. of copper sulphate solution	.....	.....	.....	1·17
Resistance of cell, in ohms	9·5	10·5	11·0	7·4
MaximumE.M.F., in volts	·366	·418	·395	·741
Current-density, in microampères per square centimetre.	Observed amounts of Fall in E.M.F.			
10	0	0	0	·001
20	0	·001	·001	·004
40	·001	·002	·003	·008
100	·002	·004	·007	·014
200	·004	·007	·011	·020
400	·012	·013	·017	·030
1000	·026	·030	·030	·049
2000	·049	·059	·055	·077
4000	·088	·099	·094	·128

The values are represented graphically by the curves marked I., II., III., and IV. respectively in the annexed figure.

A number of experiments were made with cells containing solutions not of equal molecular strengths (like those described in § 110), for the purpose of finding how much of the diminutions observed with the larger current-densities might possibly be due to the strengthening of the solution round the dissolved plate, and the weakening of the liquid round the other plate, which takes place whilst the cell is in action. The general result was that the maximum possible diminution due to these causes could not exceed about ·04 volt. The

details of these experiments and of others allied thereto will be discussed in a future paper.



Effect of varying the Size of one of the Plates, the other remaining constant.

124. The experiments described in Part V. (§ 115) were repeated with various cells containing copper-cadmium or zinc-cadmium plates instead of zinc-copper ones. The following values obtained in four such experiments illustrate the results obtained, indicating that the effect of halving the area of the plate on which metal is deposited is, as with the Daniell cells, greater than the effect of halving the area of the dissolved plate, when the former plate is not mercurialized*. The zinc-cadmium cells, however, differ from ordinary Daniell cells in this respect—that, whilst amalgamating the copper plate of a Daniell cell does not materially alter the relative effect of halving its area, amalgamating the cadmium plate of a zinc-cadmium cell greatly diminishes the relative effect of halving its area, in such sort that, when solid crystalline amalgam is used, the effect of halving the area of the cadmium plates, instead of exceeding, becomes sensibly equal to the effect of halving the area of the zinc plate, whilst when liquid amal-

* It is noteworthy in this connexion, that when nearly pure wrought-iron plates, immersed in ferrous sulphate solution, replace the zinc plates and zinc sulphate solution of normal Daniell cells, the effect of halving the area of the iron plates sometimes *exceeds* that produced by halving the area of the copper plates

gam is used the effect of halving the area of the cadmium plate becomes sensibly less than the effect of halving that of the zinc plate.

Current, in micro-ampères.	Cadmium-Copper.		Zinc-Cadmium.					
	Bright cadmium—Electro-copper.		Amalgamated zinc—Electro-cadmium.		Amalgamated zinc—Fluid amalgamated cadmium.		Amalgamated zinc—Solid amalgamated cadmium.	
	Effect of halving area of		Effect of halving area of		Effect of halving area of		Effect of halving area of	
	Cadmium.	Copper.	Zinc.	Cadmium.	Zinc.	Cadmium.	Zinc.	Cadmium.
1000	·005	·008	·004	·005	·005	·003	·005	·006
2000	·010	·013	·007	·008	·009	·005	·011	·015
5000	·016	·022	·012	·013	·018	·011	·021	·019
10000	·020	·031	·020	·025	·027	·015	·026	·025
20000	·039	·051	·030	·040	·035	·022	·032	·032

II. *Cells containing Silver as one of the Metals, the Salts used being Sulphates.*

125. Three sets of cells, after Raoult's pattern, were constructed, containing respectively zinc-silver, cadmium-silver, and copper-silver couples, the respective plates being immersed in solutions of silver sulphate saturated at ordinary temperatures, and of copper and zinc sulphate of strengths molecularly equal thereto (the silver solution contained 7·25 grammes of Ag_2SO_4 per litre, and had a sp. gr. near to 1·0067); the composition was uniformly $MSO_4\,2360\,H_2O$.

On making series of determinations of the E.M.F.'s of these cells, the following results were arrived at as the effects of varying the nature of the silver surfaces, deduced from the average values of a large number of observations (upwards of 50 sets of comparisons).

Effect of substituting Electro-Silver for Bright Silver.

	Zinc opposed.	Cadmium opposed.	Copper opposed.
Maximum ...	+·017	+·015	+·012
Minimum ...	+·003	+·005	+·004
Range.....	·014	·010	·008
Average.....	+·008	+·009	+·0075

Effect of substituting Amalgamated Silver (fluid) for Bright Silver.

Maximum ...	+·102	+·110	+·105
Minimum ...	+·092	+·095	+·092
Range.....	·010	·015	·013
Average.....	+·099	+·101	+·1025

Considering the perceptibly wider ranges of variation in these experiments than those usually observed in the zinc-cadmium-copper cells previously described, it is evident that the effect of varying the nature of the silver plate is sensibly independent of the nature of the other metal.

Sometimes, but not invariably, the amalgamated silver plates became solid and crystalline (greenish yellow) on the surface : this result was apparently brought about much more rapidly when the silver was immersed in concentrated zinc sulphate solution than under any other of the conditions obtaining in the various experiments. When this change took place the E.M.F. set up by opposing such a plate to zinc in cells containing sulphates of zinc and silver was always intermediate between that set up in the same fluids by plates of bright silver and of fluid amalgamated silver. The average of several comparisons was as follows :—

Effect of substituting Amalgamated Silver (solid) for Bright Silver.

Maximum.....	+·025
Minimum.....	+·014
Range	·011
Average	+·021

In this respect silver is analogous to cadmium when the latter is opposed to zinc (§ 121) ; but the average amounts of increase in E.M.F. due to fluid and solid amalgam are in each instance considerably greater with silver than with cadmium (·099 and ·021 for silver as compared with ·043 and ·016 for cadmium).

On substituting electro- for bright cadmium in the cadmium-silver cells, identically the same average effect was observed as in the cadmium-copper cells (§ 119), viz. an increase in the E.M.F. of from ·002 to ·006, averaging ·004 volt. Similarly, on substituting amalgamated for electro-copper in the copper-silver cells, practically the same numerical difference in the E.M.F. was brought about as was formerly observed in the zinc-copper cells (§ 107) and the cadmium-copper cells (§ 119), but in the opposite direction, the E.M.F. being *raised* in the

copper-silver cells and lowered in the other two kinds : the alteration in the E.M.F. varied between +·005 and −·003, averaging +·0005.

126. On allowing newly set-up cells to stand for several hours, different results were brought about in each of the three cases according as the silver was opposed to zinc, cadmium, or copper. In the first case the E.M.F. invariably fell; the maximum value was observed immediately after the cell was set up, and continued sensibly steady for a variable period of time, a distinct diminution becoming perceptible sometimes after half an hour, sometimes only after two or three hours. With cadmium the value after several hours was somewhat greater than that set up at first and during the subsequent hour or so ; and with copper the value attained after several hours was still greater than that exhibited during the first hour or two. The following numbers represent the average alterations thus observed, being the differences between the average readings during the first hour and during a period of from 3 to 5 hours after setting up :—

Bright silver.	Electro-silver.	Amalgamated silver.	Mean.
	Amalgamated Zinc opposed.		
−·010	−·014	−·012	−·012
	Bright Cadmium opposed.		
+·003	+·001	+·003	+·002
	Electro-Cadmium opposed.		
+·003	+·004	+·003	+·003
	Electro-Copper opposed.		
+·009	+·007	+·007	+·008
	Amalgamated Copper opposed.		
+·011	+·009	+·005	+·008

These alterations were traced to the variations in the nature of the surfaces of the plates opposed to the silver ; for on taking out, for instance, an amalgamated zinc plate after 5 hours, and replacing by a freshly amalgamated plate, the E.M.F. was restored to sensibly the same value as at first ; and similarly with the other metals. On the other hand, on taking out from two cells, for instance, a zinc and a copper plate after 5 hours, and replacing them respectively in two beakers containing zinc and copper sulphate solutions of the same molecular strengths, and connected by a siphon tube, the E.M.F. of the cell thus formed was found to fall short of the

average value of a normal Daniell cell with fresh plates by an amount sensibly equal to the sum of the numerical alterations that had occurred in the zinc-silver and copper-silver cells jointly. It is specially noticeable that, whilst in zinc-copper cells the alteration in the surface of the copper (probably through oxidation) on standing diminishes the E.M.F., in copper-silver cells the alteration is in the opposite direction : with zinc and cadmium the direction of this alteration when opposed to silver is the same as when opposed to copper.

It would seem from all these results that the effect of a given alteration of the surface of one of the plates of a voltaic pair upon the E.M.F. of the pair is independent of the nature of the other plate as regards its numerical value, although the nature of this second plate regulates the direction of the variation in the E.M.F. produced (increase or decrease), and also exerts an influence upon the rate at which the alteration of the plate-surface takes place. Thus it was repeatedly observed that, whereas an amalgamated zinc plate (or an electro-copper one), when forming part of a normal Daniell cell, did not become sensibly oxidized, so as to diminish the E.M.F. of the cell, until after several hours at least had elapsed, a precisely similar plate immersed in the same liquid, but forming part of a zinc-silver cell (or of a copper-silver cell), did become perceptibly oxidized in much less time. In other words, although no measurable current was generated in either case, yet the different amounts of strain (so to speak) set up in the chain of liquid particles between the two plates, according as one was silver or not, did affect the rate of change in the surface of the more oxidizable metal (presumably by varying the rate at which it combined with the oxygen dissolved in the fluid).

127. The following Table exhibits the average results obtained in about 150 sets of observations and comparisons, only those values made during the first hour (or sometimes less) after setting up being taken into account, and all subsequent values being rejected where any diminution through oxidation &c. began to be perceptible *. The values cited as the average effects of substituting for bright silver electro- and amalgamated (liquid) silver are the means of the three sets above quoted (§ 125) obtained respectively with zinc, cadmium, and copper :—

* Notwithstanding all care, it is probable that the average results with the zinc-silver cells are too low by a few thousandths of a volt, and those with the other cells slightly too high (vide § 128).

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for bright silver:—				
Electro-silver	+·017	+·003	·014	+·008
Amalgamated silver (liquid)	+·100	+·092	·008	+·101
„ „ (solid)	+·025	+·014	·011	+·021
Effect of substituting for electro-copper:—				
Amalgamated copper	+·005	−·003	·008	+·001
Effect of substituting for bright cadmium:—				
Electro-cadmium	+·006	+·002	·004	+·004
Electromotive force of combinations:—				
Amalgamated zinc—Bright silver	1·540	1·518	·022	1·528
„ „ Electro-silver	1·550	1·529	·021	1·536
„ „ Amalg.silver(liquid)	1·640	1·615	·025	1·627
„ „ „ „ (solid).	1·555	1·544	·011	1·549
Bright cadmium—Bright silver	1·173	1·163	·010	1·1675
„ „ Electro-silver	1·185	1·169	·016	1·1765
„ „ Amalg. silver (liquid).	1·278	1·261	·017	1·2685
Electro-cadmium—Bright silver	1·176	1·164	·012	1·1715
„ „ Electro-silver	1·186	1·176	·010	1·1805
„ „ Amalg. silver (liquid)	1·277	1·267	·010	1·2725
Electro-copper—Bright silver	·422	·411	·011	·416
„ „ Electro-silver	·429	·411	·018	·4235
„ „ Amalg. silver (liquid)...	·535	·513	·022	·5185
Amalgamated copper—Bright silver	·420	·411	·009	·4165
„ „ Electro-silver	·430	·414	·016	.424
„ „ Amalg. silver (liquid)	·535	·513	·022	·519

Volta's Law of Summation.

128. The values in this table, together with those quoted above for the zinc-cadmium and cadmium-copper cells (§§ 119 and 121) and those given in Part V. for zinc-copper cells (§ 107), clearly prove that Volta's law holds in the case of the sets of combinations

$$\text{(A)}\quad \begin{Bmatrix}\text{Zn}\\ \text{Cd}\end{Bmatrix} + \begin{Bmatrix}\text{Cd}\\ \text{Ag}\end{Bmatrix} = \begin{Bmatrix}\text{Zn}\\ \text{Ag}\end{Bmatrix}$$

$$\text{(B)}\quad \begin{Bmatrix}\text{Zn}\\ \text{Cu}\end{Bmatrix} + \begin{Bmatrix}\text{Cu}\\ \text{Ag}\end{Bmatrix} = \begin{Bmatrix}\text{Zn}\\ \text{Ag}\end{Bmatrix}$$

$$\text{(C)}\quad \begin{Bmatrix}\text{Cd}\\ \text{Cu}\end{Bmatrix} + \begin{Bmatrix}\text{Cu}\\ \text{Ag}\end{Bmatrix} = \begin{Bmatrix}\text{Cd}\\ \text{Ag}\end{Bmatrix}$$

at any rate within the range of possible error due to the somewhat larger ranges of fluctuation in the E.M.F. of silver-containing cells than were observed with zinc-cadmium-copper cells, and to the fact that, although alterations of the oxidizable plates in the cells containing silver was avoided as far as possible by only carrying on the observations for one hour and sometimes less, still it was not practicable wholly to avoid this source of inaccuracy. Thus in the three cases respectively the following figures are obtained :—

Case A.

Nature of Plate-surfaces.			Electromotive Forces developed.				
Zinc.	Cadmium.	Silver.	Zinc-cadmium.	Cadmium-silver.	Sum.	Zinc-silver.	Difference.
Amalgamated.	Bright.	Bright.	·364	1·1675	1·5315	1·528	+·0035
,,	Electro.	,,	·3595	1·1715	1·531		+·003
,,	Bright.	Electro.	·364	1·1765	1·5405	1·536	+·0045
,,	Electro.	,,	·3595	1·1805	1·540		+·004
,,	Bright.	Amalgamated (liquid).	·364	1·2685	1·6325	1·627	+·0055
,,	Electro.	,,	·3595	1·2725	1·632		+·005
						Average difference	= +·0042

Case B.

Zinc.	Copper.	Silver.	Zinc-copper.	Copper-silver.	Sum.	Zinc-silver.	Difference.
Amalgamated.	Electro.	Bright.	1·114	·416	1·530	1·528	+·002
,,	Amalgamated.	,,	1·113	·4165	1·5295		+·0015
,,	Electro.	Electro.	1·114	·4235	1·5375	1·536	+·0015
,,	Amalgamated.	,,	1·113	·424	1·537		+·001
,,	Electro.	Amalgamated (liquid).	1·114	·5185	1·6325	1·627	+·0055
,,	Amalgamated.	,,	1·113	·519	1·632		+·005
						Average difference	= +·0027

Case C.

Nature of Plate-surfaces.			Electromotive Forces developed.				
Cadmium.	Copper.	Silver.	Cadmium-copper.	Copper-silver.	Sum.	Cadmium-silver.	Difference.
Bright.	Electro.	Bright.	·7485	·416	1·1645	1·1675	−·003
,,	Amalgamated.	,,	·7475	·4165	1·164		−·0035
,,	Electro.	Electro.	·7485	·4235	1·172	1·1765	−·0045
,,	Amalgamated.	,,	·7475	·424	1·1715		−·005
,,	Electro.	Amalgamated (liquid).	·7485	·5185	1·267	1·2685	−·0015
,,	Amalgamated.	,,	·7475	·519	1·2665		−·002
Electro.	Electro.	Bright.	·7525	·416	1·1685	1·1715	−·003
,,	Amalgamated.	,,	·7515	·4165	1·168		−·0035
,,	Electro.	Electro.	·7525	·4235	1·176	1·1805	−·0045
,,	Amalgamated.	,,	·7515	·424	1·1755		−·0050
,,	Electro.	Amalgamated (liquid).	·7525	·5185	1·271	1·2725	−·0015
,,	Amalgamated.	,,	·7515	·519	1·2705		−·0020
						Average difference	= −·0032

Average difference in case			A	= +·0042
,,	,,	,,	B	= +·0027
,,	,,	,,	C	= −·0032
			Mean.....	= +·0012

On carrying out twin-cell experiments like those described in § 122, it was found that when a silver plate was placed in the central beaker, and either copper and zinc, copper and cadmium, or zinc and cadmium plates were used in the other beakers, together with solutions of the respective metallic sulphates of the same molecular strengths, the difference between the electromotive forces determined in the twin cell was always sensibly equal to the E.M.F. developed by the pair of plates other than silver employed when taken out and opposed to each other in an ordinary cell containing the same metallic solutions; and this was found to be the case, not only with freshly-prepared plates, but also with plates that had been immersed for hours and had become oxidized on the surface. For instance, in a pair of experiments with amalgamated zinc, bright silver, and electro-copper plates:—

	Cells newly set up.	After 24 hours.
Zinc-copper in single cell . .	1·115	1·098
Copper-silver in twin cell . .	·417	·425
Sum	1·532	1·523
Zinc-silver in twin cell . . .	1·534	1·522
Difference . .	−·002	+·001

Similar results were obtained in several other experiments of the same kind, both with zinc-copper plates opposed to silver and with the other two pairs (zinc-cadmium and cadmium-copper). The average difference in each case was considerably less than ±·001 volt.

Relations between the Electromotive Forces of Zinc-Silver, Copper-Silver, and Cadmium-Silver Cells and those corresponding to the net Chemical Actions taking place therein.

129. In all the cells hitherto examined in this series of researches there has been shown to be a sensible equality between the electromotive forces generated with clean plate-surfaces of pure metals and those corresponding to the net chemical and physical actions taking place therein. *A priori*,

there does not seem to be any evident reason why the same state of things should not exist in the case of cells containing silver plates. Thomsen's thermo-chemical valuations, however, indicate that the electromotive forces corresponding to the three equations

$$\begin{aligned} Zn &+ Ag_2SO_4 = 2Ag + ZnSO_4, \\ Cd &+ Ag_2SO_4 = 2Ag + CdSO_4, \\ Cu &+ Ag_4SO_4 = 2Ag + CuSO_4, \end{aligned}$$

are considerably higher than those actually observed and tabulated above, thus:—

Zinc-silver.		Cadmium-silver.		Copper-silver.	
Zn, O, SO_3 aq. =	106090	Cd, O, SO_3 aq. =	89500	Cu, O, SO_3 aq. =	55960
Ag_2, O, SO_3 aq. =	20390	Ag_2, O, SO_3 aq. =	20390	Ag_2, O, SO_3 aq. =	20390
Diff., per gramme-molecule	85700		69110		35570
Diff., per gramme-equivalent.....	42850		34555		17785
Corresponding to volt	1·890		1·524		·784

In each of the three cases the calculated E.M.F. is from ·34 to ·37 volt above the observed values when silver plates not amalgamated are used, indicating that even when only infinitesimal currents are generated a large amount of energy is nonadjuvant. It will be shown in a future paper that this behaviour is more or less marked in other kinds of cells containing silver plates and silver compounds, although the minimum amount of nonadjuvancy observed in any given case is variable with the nature of the saline compounds in the cell*. In order to see whether the rise in E.M.F. produced by amalgamating the silver plate is due simply to the heat of formation of silver sulphate being less when the silver is dissolved in mercury than when it is free, determinations of the heat of solution of silver (precipitated from the nitrate by copper, crystalline) were made. It was found a little difficult to get every trace of silver used dissolved in mercury when 20 grams of silver and 600 of mercury were employed, even though the surface of the former was washed with dilute nitric acid. With smaller amounts of mercury only incomplete solution was

* Raoult has already obtained numbers (*Ann. Chim. et Phys.* [4] ii. 317 and iv. 392) indicating that the "galvanic heat" (§ 17) of a cell containing copper, silver, and the nitrates of their metals is sensibly below the value due to the net chemical action taking place. These and other similar observations will be discussed in a future paper.

effected, more or less pasty amalgam being formed, and a smaller heat-development being then noticeable. The most trustworthy determinations made indicated that during the solution of 108 grams of silver, 2070 gramme-degrees are evolved. Hence the heat of formation of silver sulphate, when the silver is dissolved in mercury, is, per gramme-molecule, $20390 - 2 \times 2070 = 16250$; so that the heat of displacement of silver from silver sulphate, when the metal is ultimately obtained in mercurial solution, is greater than the values above calculated for zinc, cadmium, and copper respectively as precipitating metals by 4140 gramme-degrees per gramme-molecule, or 2070 per gramme-equivalent, corresponding with ·0915 volt. The average difference in E.M.F. between cells otherwise alike, in which crystalline electro-silver and amalgamated silver (fluid) are respectively used, is almost identical with this, being $\cdot 101 - \cdot 008 = \cdot 093$ volt. As already noticed (§ 120), silver and cadmium differ in their behaviour in this connexion when amalgamated, the inequality between the effect on the E.M.F. actually produced by amalgamation of cadmium and that corresponding with the heat of solution in mercury being probably due to the oxidation of cadmium by dissolved air.

Rate of Fall in Electromotive Force through so-called "Polarization" for definite Amount of Increase in Rate of Current-flow.

130. In order to see whether the electromotive forces of the cells above described are rendered less (when no current passes) than they otherwise would be, through the interfering action of dissolved air or some other similar obscure cause, a number of experiments were made like those described in Part V. (§§ 103–105), with the general result of showing that the deficiency observed in the E.M.F. actually generated when no current passes (as compared with that calculated from the thermo-chemical data) is at any rate not due to any such cause, inasmuch as the E.M.F. generated when a current does pass is always more or less *below* that set up when no current passes, just as with normal Daniell cells and with the zinc-cadmium and cadmium-copper cells above described. Thus, for example, the following numbers were obtained in three experiments, in each of which bright zinc plates were employed, and one in which electro-copper plates were used, the silver plates being in each instance uppermost and surrounded by saturated silver sulphate solution.

Fluid surrounding the zinc plates.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·10.	.....
Fluid surrounding the copper plates	.....	.....	.....	Copper sulphate sol., sp. gr. 1·17.
Nature of silver plates.	Electro-silver (crystalline.	Solid crystallized amalgam.	Solid crystallized amalgam.	Electro-silver (crystalline).
Resistance, in ohms, of column of fluid between plates	66·9	76·0	76·6	71·0
Maximum E.M.F.	1·500	1·511	1·546	·401
Current-density, in microampères.	Observed amounts of fall in Electromotive Force.			
20	.....	.....	·012	·005
50	·016	·010	·021	·011
100	·025	·018	·026	·019
200	·041	·036	·037	·030
400	·062	·067	·059	·047
600	·080	·097	·077	·063
1000	·116	·144	·112	
2000	·209	·258	·177	

These numbers are represented graphically by the curves marked respectively V., VI., VII., and VIII. in the figure.

On trying experiments, like those described in § 110, to see how far the falling-off in E.M.F. when a current is generated could be due to the accumulation of zinc (cadmium or copper) sulphate round the plate opposed to the silver, it was found that the maximum possible effect due to this cause could not exceed about ·04 volt with zinc and cadmium, and ·02 with copper. These experiments, and others of a similar nature, will be discussed in a future paper.

On comparing the eight curves represented in the figure with those previously described as obtained with various forms of Daniell cell (Part V. § 105), it is noticeable, first, that the curves obtained with the zinc-cadmium cells underlie all the others (I., II., and III.); secondly, that the curve with the cadmium-copper cell (IV.) is practically identical with one of the Daniell-cell curves—indicating consequently that, whilst the substitution of copper for cadmium in a zinc-cadmium cell raises the position of the curve (*i. e.* increases the rate of fall in E.M.F. according as the current-density increases), the substitution of cadmium for zinc in a Daniell cell does not materially alter the position of the curve ; thirdly, the curves with the zinc-silver and copper-silver cells overlie all the others, whilst the copper-silver curve (VIII.) is not widely different from the zinc-silver curves (V., VI., and VII.)—indicating that, whilst the substitution of silver for copper in a

Daniell cell largely raises the position of the curve, the effect of substituting copper for zinc in a zinc-silver cell is very much less marked. In other words, *the nature of the dissolved metal affects the rate of decrease in E.M.F. with increasing current-density much less than does the nature of the deposited metal; whilst the less the heat of formation of the salt of the latter that is decomposed by the passage of the current, the more rapid appears to be the rate of fall in the E.M.F. of the cell as the current-density increases.* As regards the first part of this general conclusion, it is precisely what also results from the majority of the previously described experiments on the effect of halving the area of the dissolved plate, as compared with that produced by halving the area of the plate on which metal is deposited. The following experiments with cells containing silver plates also give the same general results.

Effect of Varying the Size of one of the Plates in Cells containing Silver as one of the Metals, the other Plate remaining unaltered.

131. By operating in the way described in § 115, the following results were obtained in four sets of observations with zinc-silver and copper-silver cells, showing that, in all cases, halving the area of the silver plate produces a sensibly greater decrease in the E.M.F. set up with a constant rate of current-flow than is effected by halving the area of the plate opposed to the silver.

Current-density, in micro-ampères.	Zinc-Silver.						Copper-Silver.	
	Amalgamated zinc—electro-silver.		Amalgamated zinc—crystalline amalgamated silver.		Amalgamated zinc—crystalline amalgamated silver.		Electro-copper—electro-silver.	
	Effect of halving area of		Effect of halving area of		Effect of halving area of		Effect of halving area of	
	Zinc.	Silver.	Zinc.	Silver.	Zinc.	Silver.	Copper.	Silver.
1000	·009	·011	·007	·026	·008	·019	·004	·011
2000	·012	·024	·009	·045	·012	·036	·010	·023
3000	·014	·038	·011	·059	·015	·047	·017	·028
5000	·016	·064	·020	·103	·021	·070		
8000	·028	·093	·038	·186	·029	·122		
10000	.....	.....	.....	.....	·034	·151		

Summary of Results.

132. The foregoing results may be thus summarized :—

Cells containing zinc and cadmium or cadmium and copper

plates, immersed in solutions of the sulphates of these metals respectively, are closely analogous to ordinary Daniell cells (containing zinc sulphate solution). Slight variations in the E.M.F. generated are introduced by varying the condition of the plate-surfaces; but in all cases the maximum E.M.F. actually generated with clean pure plate-surfaces and with solutions of equal molecular strengths is close to that calculable from the net chemical action taking place in the cell when generating a current. When the cadmium plates are not amalgamated, or are covered with crystalline solid amalgam, the electromotive forces are close to ·75 and ·36 volt for cadmium-copper and zinc-cadmium cells respectively, the values corresponding to the net chemical actions as deduced from Thomsen's thermo-chemical results being substantially the same. When the cadmium plates are covered with fluid amalgam, the electromotive forces are lower in the first case and higher in the second by upwards of ·04 volt—a quantity distinctly exceeding in magnitude the E.M.F. corresponding with the heat of solution of cadmium in mercury, although of the same sign.

(2) The electromotive forces of zinc-silver, cadmium-silver, and copper-silver cells containing the respective sulphates of these metals differ from those of zinc-cadmium, zinc-copper (Daniell), and cadmium-copper cells in this respect, that the maximum electromotive forces generated (the fluids being of equal molecular strength) are not sensibly the same as those calculated from Julius Thomsen's thermal data, but in every case fall short by an amount not far from ·35 volt. When the silver plates are not mercurialized, or are coated with crystalline amalgam, the electromotive forces (which vary slightly with the precise nature of the plate-surfaces) are, in the three cases, near to 1·53, 1·17, and 0·42 volt respectively, the metallic solutions being of equal molecular strength. When the silver plates are covered with fluid amalgam, the electromotive forces are in each case about ·09 volt higher than the values obtained with electro-deposited crystalline metal, this increase almost exactly coinciding with the increment corresponding with the heat of solution of silver in mercury.

(3) As long as the cadmium and zinc [or copper] solutions employed are of the same molecular strength within the limits indicated respectively by $MSO_4\,50H_2O$ and $MSO_4\,1250H_2O$, the E.M.F. developed with a given pair of cadmium and zinc [or copper] plates is sensibly independent of the actual strength of the solutions, these cells behaving precisely like Daniell cells in this respect. With Daniell cells the solutions are practically of the same molecular strength when they

are of the same specific gravity; but with the other cells containing cadmium this is not so, cadmium-sulphate solution being uniformly more dense than either zinc or copper solution of the same molecular strength.

(4) The effect on the E.M.F. of a cell of a given alteration in the nature of the surface of either a zinc, copper, cadmium, or silver plate is sensibly the same numerically whichever other one of these four metals be opposed to it; but the direction of the alteration is opposite according as the plate is the anode or the kathode of the combination.

(5) Volta's "Law of Summation" universally holds within the limits of experimental error in all the cases examined; that is, the electromotive forces of zinc-cadmium, cadmium-copper, and copper-silver combinations are such that, for any given kinds of plate-surfaces, the sums of the two first, of the two last, and of the three together are respectively equal to the electromotive forces of zinc-copper, cadmium-silver, and zinc-silver combinations.

(6) Zinc, copper, and cadmium plates alter superficially (probably in consequence of oxidation by dissolved air) more rapidly when opposed to silver than when opposed to any other one of these four metals, on being immersed in solutions of their respective sulphates forming one half of a cell on Daniell's principle—no current being generated by the cell, the measurements being made by means of a quadrant electrometer.

(7) With all the cells examined the behaviour when generating a current is analogous to that of a normal Daniell cell: when the current-density exceeds a few microampères per square centimetre of plate-surface, a more or less marked diminution in the E.M.F. ensues, the falling-off being the greater the greater the current-density. With moderately strong currents the diminution far exceeds the maximum possible amount due to accumulation of dissolved salt round the plate dissolved, and exhaustion of solution round the other plate. *Cæteris paribus*, the rate of fall in E.M.F. as the current-density increases is the more rapid the lower the heat of formation of the metallic salt decomposed in the cell so as to deposit the metal, and is comparatively but little affected by the nature of the dissolved metal.

(8) The effect of halving the area of the plate on which metal is deposited is usually to cause a greater diminution in the E.M.F. than is produced by halving the area of the dissolved plate; amalgamated cadmium plates in zinc-cadmium cells, however, form an exception to this rule.

XXIII. *An Integrating Anemometer.* *By* WALTER BAILY*.

[Plate V.]

THE object of the instrument described in this paper is to resolve the velocity of the wind in two directions at right angles to one another, and to obtain the time-integral of each part separately.

The instrument contains a horizontal plane, in which are two slits N S and E W, forming a cross to be placed with its arms towards the cardinal points. In these slits are sliders F, G, connected by a bar of constant length. O is the centre of the cross, H the centre of the bar. The locus of H is a circle with centre O. A weathercock or some equivalent mechanism is to keep H in such a position that the radius O H is in the direction of the wind. The sliders carry beneath them wheels, B, C, whose planes are perpendicular to their respective slits, and whose centres are beneath the pivots joining the slits to the bar. [See figs. 1, 2, 3, Plate V. Fig. 1 gives a perspective view of the instrument, omitting some points; fig. 2 gives a view of the top of the instrument; and fig. 3 gives a section of a slit and slider, and shows the wheel carried by the slider.] The wheels B, C rest on a disk, A (fig. 1), which revolves about a vertical axis immediately below O. The disk A is to be rotated by Robinson's cups, or some equivalent mechanism, so as to have a velocity proportional to that of the wind. The pieces which carry the wheels B, C should be allowed some play in a vertical direction; and the contact of B and C with A can then be maintained either by their own weight or by the use of a spring. The number of rotations of B in a given time is proportional to the time-integral of the resolved part of the wind in one direction (say, north); and the number of rotations of C is proportional to the time-integral of the resolved part of the wind in a direction at right angles to the first (say, west).

Let Ω be the angular velocity of the disk A; ω, ω' the angular velocities of the wheels B, C; m, m' the number of their rotations in a given time t; b their radius, a the length of the bar; θ the angle between the direction of the wind and (say) the north; then $b\omega = a \sin\theta \,.\, \Omega$, and $b\omega' = a \cos\theta \,.\, \Omega$; and the integrals required are

$$\int_0^t \Omega \sin\theta \, dt = \int_0^t \frac{b}{a} \omega \, dt = \frac{b}{a} \pi m$$

* Communicated by the Physical Society, having been read at the Meeting on June 10, 1882.

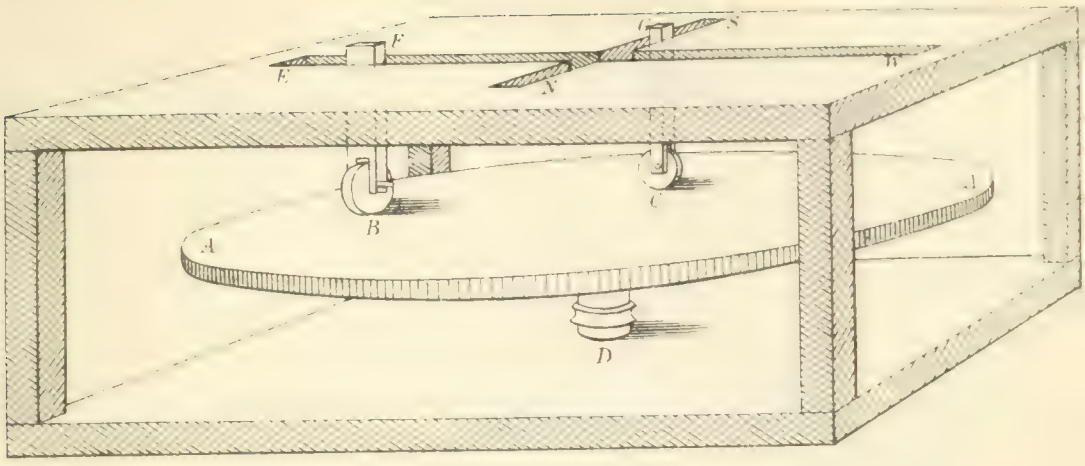


Fig. 1.

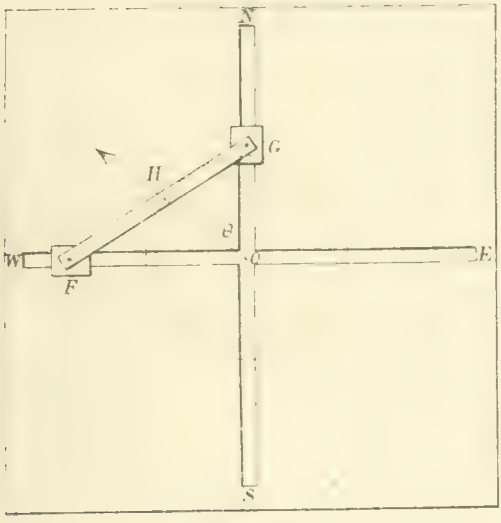


Fig. 2.

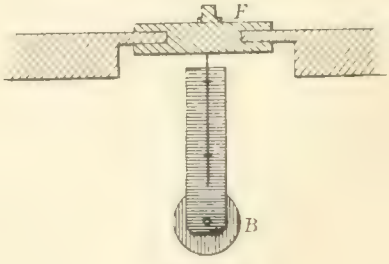


Fig. 3

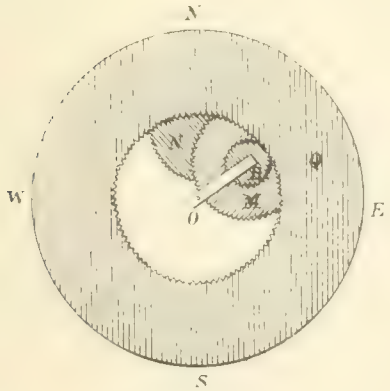


Fig. 4.

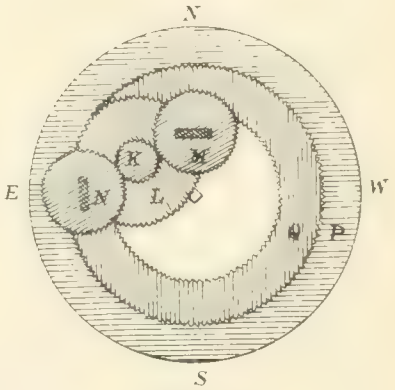


Fig. 5

Mintern Bros. lith.

and

$$\int_0^t \Omega \cos\theta\, dt = \int_0^{t_0} \omega' dt = \frac{b}{a}\pi a'.$$

Therefore a, a' are proportional to the required integrals.

Each slider might carry a train of wheels to record the number of rotations; or an electrical arrangement might be made in which each wheel should complete a circuit at each rotation and the number of contacts should be recorded. In the latter case, as no distinction is preserved as to the direction in which the wheels revolve, it becomes necessary to have four circuits, one for each cardinal point, with a recorder in each, and to have one connected with each arm of the cross.

A working model of the instrument above described was exhibited at the Meeting, and was fitted with an electrical arrangement such as I have mentioned.

I have since discovered that the slits, sliders, and bar above described may be replaced by a train of cogged wheels. (Fig. 4 represents the upper, and fig. 5 the under surface of the train.) A bar turns in a horizontal plane about O, and is kept in the direction of the wind. This bar carries three wheels, H, K, L, having the same axis. The length of the bar from one pivot to the other is supposed to be an inch and a half. The wheels H, K are rigidly connected; and L lies between them and turns independently. H and K are 1 inch, and L is 3 inches in diameter. L rolls on the inner edge of P, and H rolls on the inner edge of Q, the diameters of P and Q being 6 and 4 inches respectively. Two wheels, M and N, whose diameters are 2 inches, are carried by the wheel L, and have their centres at the extremities of a diameter of L. M and N are in the same plane as K, and are therefore touched by it. As the bar rotates, M and N move without rotation, and their centres move in straight lines passing through O at right angles to one another, and are at a fixed distance apart, and have the line joining them bisected by the bar, which is the direction of the wind. Hence M and N may be used to carry the wheels B, C (fig. 1) instead of their being carried by the sliders E and G.

XXIV. *On the Effect upon the Ocean-tides of a Liquid Substratum beneath the Earth's Crust.* By the Rev. O. Fisher, *M.A., F.G.S.**

(1) IN a work which I have lately published, entitled 'Physics of the Earth's Crust' †, I have maintained the theory that the crust is thin, and floats in equilibrium upon a slightly

* Communicated by the Author.

† Macmillans, 1881.

denser substratum of molten rock, the elevations on the surface of the crust being due to compression, and being supported through flotation by corresponding protuberances (which I call "roots of the mountains") projecting downwards into the denser liquid—a mode of support long ago suggested by Sir G. B. Airy *.

The most formidable difficulty in the way of this theory has been said to be the necessary occurrence of tides in such a substratum; and it has been thought that, the crust being carried up and down in sympathy with the substratum, the ocean-tides would be almost entirely masked, and that there would be no appreciable rise and fall of the water relatively to land †.

The explanation of the difficulty which I had offered in the book itself was, that the tides in the substratum would involve a horizontal transference of fluid backwards and forwards, and might be expected to be of small amplitude, owing to the viscosity of the substance and to its confinement beneath the crust ‡; and I felt so convinced, from geological considerations, that the substratum must be at least plastic, if not liquid, that I did not think it needful to go further into the question. But in consequence of the weight of authority by which this objection has since been enforced, I have been induced to examine it more closely, and to endeavour to discover what indications, if any, the ocean-tides might be expected to give of the existence, or otherwise, of such a substratum.

(2) The "canal theory" of the tides appears to be the most suitable to solve, in a general way, the question at issue; for what we have to do is to investigate the motion of layers of liquids (the substratum and the ocean) under the influence of tide-producing forces.

An article upon the tides by Mr. D. D. Heath appeared in this Magazine § in 1867. He intimates that it was founded upon Airy's treatise in the *Encyclopædia Metropolitana*. I shall take the liberty of adopting the introductory paragraphs of his analysis, merely changing a symbol. The liquid is treated as "confined to a narrow channel running round the equator, supposing the moon vertical over it and moving uniformly in her orbit, so that her apparent motion will be also uniform and somewhat less than that of the earth's rotation."

"§ 3. And first as to the geometrical characteristics of a fluid wave uniformly propagated westward at any rate (α).

* Phil. Trans. Roy. Soc. vol. cxlv. p. 101.

† 'Nature,' vol. xxv. p. 423, 1882; also New-York 'Nation,' June 15, 1882.

‡ 'Physics of the Earth's Crust,' p. 23.

§ Fourth Series, vol. xxxiii. p. 165, March 1867.

"Taking any point on the equator as origin, let x measure the longitude [linear] westward of any other point, and let κ be the mean depth of the water [or liquid] and y the small elevation of the surface above the level at x as a definite moment of time.

"For the wave to be propagated with a persistent form and at the rate α, the height at x must, in a time dt, change from y to the value which y now has at a distance αdt behind it, or $x-\alpha dt$ from the origin; that is,

$$\frac{dy}{dt}dt + \&c. = -\frac{dy}{dx}\alpha dt + \&c.$$

or

$$\frac{dy}{dt} = -\alpha\frac{dy}{dx}. \quad \ldots\ldots\ldots \quad (A)$$

And if this relation exist everywhere between the differential coefficients, the condition will be fulfilled for finite intervals.

"Not only the heights, but every other measure or mark of disturbance must be propagated onwards at the same rate, if the wave is to have a permanent character; so that if v be the average forward velocity of the particles in a vertical section at x, we must have

$$\frac{dv}{dt} = -\alpha\frac{dv}{dx}. \quad \ldots\ldots\ldots \quad (B)"$$

It is assumed in Mr. Heath's paper, and, I believe, usually, that κ, the depth of the canal, is "but a very few miles." This assumption is avoided in what follows, not being compatible with the problem we have to solve; for although it will appear that the assumption that κ is small might have been made, yet this could not have been readily foreseen.

(3) The assumptions which will be made are:—That the horizontal velocity of all the particles of water in a vertical column are the same: that the vertical and horizontal velocities are small, and that the elevation or depression of the surface above or below the mean level is also small; so that products of these quantities and of their derivatives may be neglected.

From this it follows that, expressing partial differential coefficients by brackets, since

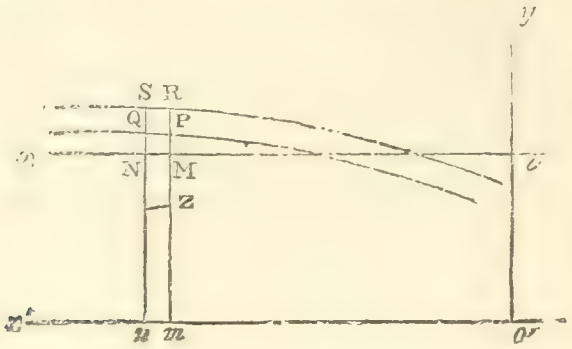
$$\frac{dv}{dt} = \left(\frac{dv}{dt}\right) + \left(\frac{dv}{dx}\right)v,$$

$$\therefore \quad \frac{dv}{dt} = \left(\frac{dv}{dt}\right), \text{ approximately;}$$

and

$$\therefore \quad \left(\frac{dv}{dt}\right) = -\alpha\left(\frac{dv}{dx}\right).$$

Henceforth the brackets will be omitted.



Let O x be the mean level of the surface,
O O′ be the depth of the canal $=\kappa$,
O M $=x$,
M P $=y$,
M Z $=z$.

Let v be the vertical velocity of the liquid at Z at the time t, and in the time dt let the surface rise from P Q to R S.

Then the volume of liquid which enters Q Z in the time dt is

$$v(\kappa+y-z)dt + w\,dx\,dt :$$

and that which goes out is

$$v'(\kappa+y'-z)dt,$$

v' and y' being the values of v and y at $x+dx$, or

$$v+\frac{dv}{dx}dx, \&c.$$

And their difference is SP, or

$$\frac{dy}{dt}dx\,dt.$$

Therefore, neglecting the products vy and $v'y'$, we get

$$w-\frac{dv}{dx}(\kappa-z)=\frac{dy}{dt}.$$

It is obvious that when $z=0$, then $w=0$, and $\frac{dy}{dt}$ is not a function of z; whence the two relations,

$$-\kappa\frac{dv}{dx}=\frac{dy}{dt}, \quad \ldots\ldots\ldots \quad (1)$$

and
$$w = -\frac{dx}{dx}z. \quad \ldots \ldots \ldots \quad (2)$$

But we know (A) that $\frac{dy}{dt} = -\alpha\frac{dy}{dx}$.

Wherefore from (1),

$$\frac{dv}{dx} = \frac{\alpha}{\kappa}\frac{dy}{dx};$$
$$\therefore \frac{dw}{dx} = -\frac{\alpha}{\kappa}\frac{d^2y}{dx^2}z;$$

and also
$$r = \frac{\alpha}{\kappa}y. \quad \ldots \ldots \ldots \quad (3)$$

By the same reasoning as that by which we have concluded that $\frac{dv}{dt} = -\alpha\frac{dv}{dx}$, we likewise conclude (B) that

$$\frac{dw}{dt} = -\alpha\frac{dw}{dx};$$
$$= \frac{\alpha^2}{\kappa}\frac{dy^2}{dx^2}z. \quad \ldots \ldots \ldots \quad (4)$$

(4) The equations of fluid motion in two dimensions will be, ρ being the density and p the pressure at Z:—

$$\begin{cases} \frac{1}{\rho}\frac{dp}{dx} = X - \frac{dv}{dt} - v\frac{dv}{dx} - w\frac{dv}{dz}, \quad \ldots \quad (1) \\ \frac{1}{\rho}\frac{dp}{dz} = Z - \frac{dw}{dt} - v\frac{dw}{dx} - w\frac{dw}{dz}. \quad \ldots \quad (2) \end{cases}$$

In our problem ρ is constant. Considering a tide formed over a rigid bottom, the horizontal forces are the difference between the moon's horizontal attractions at the point x, z, and in a parallel direction at the earth's centre, and the friction.

The moon's differential horizontal attraction is $-\frac{3aM}{2D^3}\sin 2\omega$, M being her mass, a the earth's radius, D the moon's mean distance, and ω the longitude west of the moon's meridian. It will be negative, because it acts in an easterly direction. Call it $-\mu\sin 2\omega$.

The horizontal friction, if taken as proportional to the velocity v of the liquid (which is true of low velocities), f being the coefficient of friction, is fv, or (by (3) of § 3), $f\frac{\alpha}{\kappa}y$. When

y is positive, or the liquid flowing westward, this force acts eastward, and *vice versâ*; so that its sign is always opposite to that of y. It will therefore be expressed by $-f\frac{\alpha}{\kappa}y$. Hence

$$X=-\mu\sin 2\omega-f\frac{\alpha}{\kappa}y;$$

also $$Z=-g.$$

Therefore, neglecting the small products, and substituting for $\frac{dv}{dt}$ and $\frac{dw}{dt}$ the values lately found, the equations are reduced to

$$\begin{cases}\frac{1}{\rho}\frac{dp}{dx}=-\mu\sin 2\omega-f\frac{\alpha}{\kappa}y+\frac{\alpha^2}{\kappa}\frac{dy}{dx}, & \ldots\ (1)\\ \frac{1}{\rho}\frac{dp}{dz}=-g-\frac{\alpha^2}{\kappa}\frac{d^2y}{dx^2}z. & \ldots\ (2)\end{cases}$$

Integrating (2), and taking the integral from 0 to z, we obtain

$$\frac{1}{\rho}p=g(\kappa+y-z)+\frac{\alpha^2}{2\kappa}\frac{d^2y}{dx^2}\left((\kappa+y)^2-z^2\right);$$

$\therefore$ differentiating with respect to x, and neglecting the small products,

$$\frac{1}{\rho}\frac{dp}{dx}=g\frac{dy}{dx}+\frac{\alpha^2\kappa}{2}\frac{d^3y}{dx^3}.$$

If there should be a floating crust of any kind capable of adapting itself freely to the wave, the pressure arising from this cause must be added to p; but it disappears when differentiated.

Hence, putting $a\omega$ for x, and equating the values of $\frac{1}{\rho}\frac{dp}{dx}$, the differential equation to the wave-surface is

$$f\frac{\alpha}{\kappa}y+\left(g-\frac{\alpha^2}{\kappa}\right)\frac{1}{a}\frac{dy}{d\omega}+\frac{\alpha^2\kappa}{2a^3}\frac{d^3y}{d\omega^3}=-\mu\sin 2\omega.$$

Assume

$$y=A\cos 2\omega-B\sin 2\omega,$$

$$\therefore\ y=\sqrt{A^2+B^2}\cos(2\omega+2\delta),$$

where

$$\tan 2\delta=\frac{B}{A}.$$

And we find

$$\tan 2\delta=\frac{f\frac{a\alpha}{\kappa}}{2\left(g-\frac{\alpha^2}{\kappa}\right)-4\frac{\alpha^2\kappa}{a^2}}; \quad \ldots\ (3)$$

whence

$$y = \frac{a\mu}{2\left(g - \frac{\alpha^2}{\kappa}\right) - 4\frac{\alpha^2}{a^2}\kappa} \cos 2\delta \cos(2\omega + 2\delta).$$

(5) Suppose c to be the maximum value of y when friction is not taken account of. Then

$$c = \frac{a\mu}{2\left(g - \frac{\alpha^2}{\kappa}\right) - 4\frac{\alpha^2}{a^2}\kappa}.$$

And the maximum value of y when friction acts will be

$$c' = c \cos 2\delta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Equations (3) and (4) show that, as friction is increased, 2δ tends towards 90° or δ towards 45°; while at the same time c', the maximum tide above mean level, diminishes to zero. Hence as friction (or viscosity) increases, the vertex of the tidal spheroid moves eastward, the ellipticity of the tidal spheroid simultaneously decreasing, until, when friction is infinite, its vertex reaches 45° east of the moon, and the tide disappears altogether. The same general result appears from Mr. Darwin's table (p. 16)* to hold good in the case of bodily tides, if there should be such in the earth.

(6) We notice that our result is independent of the density of the liquid, and that the weight of a floating crust, if considered flexible, would not affect it—the reason being that such a crust would aid in depressing the hollows just as much as it would hinder the elevation of the ridges. It would have an effect analogous to an additional load to the bob of a pendulum.

The coefficient of κ in the denominator of the expression for c shows that the term may be neglected, although κ itself be not small. For α is the space over which the wave travels in one second, while a is the radius of the earth.

Neglecting this term, c and likewise c' are positive or negative according as

$$\kappa > \text{ or } < \frac{\alpha^2}{g}.$$

Consequently, when $\kappa < \frac{\alpha^2}{g}$ there will be low tide under the

* "On the Bodily Tides of Viscous and Semi-elastic Spheroids, and on the Ocean-Tides upon a yielding Nucleus," Phil. Trans. Roy. Soc. part i. 1879.

moon, and when $\kappa > \frac{a^2}{g'}$ there will be high tide under the moon. In the case of the semi-diurnal tide $\frac{a^2}{g'}$ is about 12 miles.

When $\kappa = \frac{a^2}{g'}$ the result fails: for e becomes infinite, which is contrary to the assumptions on which the solution has been obtained.

(7) Let us now look to the effect of a tide in the crust of the earth upon the ocean-tide, to see whether the tide formed in a liquid substratum would so far diminish the ocean-tide that the observed amount of the ocean-tide would disprove the existence of a liquid substratum. The manner in which such a diminution of the ocean-tide would be produced in an extreme case appears thus:—Suppose that the earth were liquid, and that there were an extensible film within it at a depth from the surface equal to the ocean depth. Then, on the equilibrium theory, the entire sphere would be deformed as a whole, and the measurable tide would be merely the excess of the deformation at the surface beyond that at the depth at which the film lay; which excess would be inappreciable.

In considering this question, it is necessary to take account of the attraction upon the ocean of the part of the tidal earth-spheroid exterior to the sphere to which it is tangential. The problem has been worked out by Mr. G. H. Darwin in part ii. of his paper "On the Bodily Tides of a Viscous Spheroid"*. He considers the moon as moving uniformly in the equator and raising tide-waves in a narrow equatorial canal. The greatest range of the bodily tide is taken as $2E$: and it is supposed to be retarded after the passage of the moon by an angle $\frac{\epsilon}{2}$ which corresponds to δ in this paper. The expression at which he arrives for the motion of the wave-surface of the ocean relatively to the bottom of the canal (observing that he measures the ordinate downward instead of upwards as I have done), when the symbols are replaced by those here used, becomes

$$\kappa + \frac{\kappa}{a^2 - \mu k}\left\{\left(\frac{\sigma\mu}{2}\cos 2\delta - \frac{2}{5}\rho E\right)\cos(2\omega + 2\delta) + \frac{\sigma\mu}{2}\sin 2\delta \sin(2\omega + 2\delta)\right\}.$$

The apparent tide relatively to land can therefore be written,

* Phil. Trans. Roy. Soc. part i. 1879, p. 22.

putting, as before, $\frac{g\mu\kappa}{2(a^2-g\kappa)}=s$.

$$h=e\left\{\cos 2\omega-\frac{4}{5}\frac{gE}{g\mu}\cos(2\omega+2\delta)\right\}.$$

To find its maximum height put $\frac{dh}{d\omega}=0$:

$$\therefore\ 0=e\left\{-\sin 2\omega+\frac{4}{5}\frac{gE}{g\mu}\cos(2\omega+2\delta)\right\}.$$

Squaring, and adding, and calling the high tide H, we have

$$H^2=e^2\left\{1-\frac{4}{5}\frac{gE}{g\mu}\left(2\cos 2\delta-\frac{4}{5}\frac{gE}{g\mu}\right)\right\},$$

from which the diminution of the ocean high tide by the earth-tide, or $e-H$, can be found.

(8) The canal theory of the tides is doubtless less applicable to the fortnightly than to the semidiurnal tide. Nevertheless a certain fortnightly tide would be raised in an equatorial canal: and since we are seeking the *proportional* diminution only, and not the absolute height of the tide, we may assume that this result will be applicable to the fortnightly tide if we assign corresponding values to the symbols. It is evident that the earth-tide must be of the same period as the ocean-tide which is affected by it: so that H, e, and E will belong to tides of the same period.

The foregoing equation is suitable to find the diminution of the ocean-tide by the earth-tide, whether we consider the tidal deformation of the ocean-bottom to arise from a bodily tide in a non-rigid, but solid, earth, or from a tide in a liquid layer beneath the crust, and of a depth which is small compared with the radius. The latter is the theory of the constitution of the earth which I have maintained in my 'Physics of the Earth's Crust,' and against which the tidal argument has been held to present a formidable objection. It is therefore with this supposition that I am concerned. The canal theory of the tides, dealing with a layer of liquid, seems to be the suitable one upon which to estimate the deformation of the crust through the tide in the liquid substratum—that is, to give the value which we ought to assign to E. Let, then, η be the tide which would be formed in such a substratum were it of uniform depth, perfectly liquid, and frictionless. Then, from what has been already proved,

$$E=\eta\cos 2\delta.$$

Now

$$\eta = \frac{a\mu}{g} \frac{\kappa}{2\left(\kappa - \frac{\alpha^2}{g}\right) - 4\frac{\kappa^2}{a^2}\frac{\alpha^2}{g}}.$$

Substituting for E, and observing that $\frac{a\mu}{g}$ divides out, we have

$$H^2 = c^2 \left\{ 1 - \frac{2}{5} \frac{\kappa \cos^2 \delta}{\kappa - \frac{\alpha^2}{g} - 2\frac{\kappa^2}{a^2}\frac{\alpha^2}{g}} \left(2 - \frac{4}{5} \frac{\kappa}{\kappa - \frac{\alpha^2}{g} - 2\frac{\kappa^2}{a^2}\frac{\alpha^2}{g}} \right) \right\}.$$

This expression is therefore applicable to find the diminution of an ocean-tide of any period, if we give a corresponding value to α, because the moon's force is not involved in it*.

(9) The theory of "mountain-roots," described at the commencement of this paper, requires that the liquid substratum should be at least about 60 miles deep. Now if α be the velocity of the semidiurnal tide, $\frac{\alpha^2}{g}$ is about 12 miles. Hence κ will be at least five times $\frac{\alpha^2}{g}$. And if we omit the consideration of friction, then $\cos 2\delta = 1$, and $\frac{\kappa^2}{a^2}\frac{\alpha^2}{g}$ is very small, so that

$$H = c\left\{1 - \tfrac{2}{5}\tfrac{5}{4}\right\} = \tfrac{1}{2}c.$$

The semidiurnal tide would therefore, under these circumstances, be diminished to one half.

If the depth of the substratum were, say, 120 miles, the tide, when friction is neglected, would be diminished by $\frac{4}{9}$, or it would have $\frac{5}{9}$ of its undiminished value.

(10) But, no doubt, friction would have a very considerable effect upon a tide of short period in such a substance as molten rock, confined both above and below between a rigid nucleus and the floating crust. The factor $\cos^2 2\delta$ would therefore greatly lessen the term expressing the diminution of the tide. The mountain-roots likewise, following the contour of the land masses of the globe, would so confuse the tidal phenomena, both in the substratum and in the ocean, that little

* "If Ω be the moon's orbital velocity, and I the inclination of the plane of the orbit to the earth's equator, then the part of the tide-generating potential on which the fortnightly tide depends is

$$\frac{9}{8}\frac{m}{c^3} w r^2 \sin^2 \mathrm{I}(\tfrac{1}{3} - \cos^2\theta)\cos 2\Omega t,"$$

where m is the moon's mass, c her distance, w the mass per unit of volume of the earth, r the radius vector, and θ the colatitude (Darwin, "On the Bodily Tides," &c., *loc. cit.* p. 15).

reliance can be placed upon the above estimates. It is, however, necessary to remark that all the accidents to which I have referred would tend to diminish the earth-tide, and to lessen the consequent diminution of the ocean-tide, and so to weaken any argument against the theory of a liquid substratum derived from the circumstance that a diminution of the semidiurnal tide has never been detected.

(11) It appears, however, to be chiefly upon the fortnightly tide that reliance is placed for answering the question whether the earth is rigid or not, because friction and the interference of continents will have a much smaller effect upon that slow tide. Now the fortnightly tide in an equatorial canal would have a velocity of about 54 feet per second. If we substitute this value for α, neglecting the effect of friction, and putting $\kappa = 60$ miles, we obtain

$$H = e \times 0{\cdot}6.$$

If we use a larger value for κ, the result will be but little affected.

(12) The effect of friction would not altogether disappear even in the case of the fortnightly tide; and the supposed mountain-roots, which appear to be a necessary accompaniment of a liquid substratum, would still interfere with the formation of the tide, and diminish the value of E, and lessen the diminution of the ocean-tide.

Suppose, then, that if there were no friction in the substratum, the ocean-tide would, as shown above, be diminished to 0·6 of its undiminished value. Let us inquire what the coefficient of friction in the substratum would need to be to bring the ocean-tide up to 0·7 of its undiminished value. Now

$$H = e\{1 - \tfrac{2}{5}\eta \cos^2 2\delta(2 - \tfrac{4}{5}\eta)\} \text{ nearly.}$$

Hence the diminution varies as $\cos^2 2\delta$. By our supposition

$$\frac{1-0{\cdot}7}{1-0{\cdot}6} = \cos^2 2\delta,$$

or

$$2\delta = 30°.$$

Now (§ 4)

$$\tan 2\delta = \frac{fa\alpha'}{2(g\kappa - \alpha'^2)};$$

and α', the equatorial velocity of the fortnightly tide, $= 54{\cdot}28$ feet per second; $\kappa = 60 \times 5280$ feet; and $a = 209 \times 10^5$ feet; $g = 32{\cdot}2$ feet. From this equation we find

$$f = 0{\cdot}0178 \tan 2\delta.$$

And if $2\delta = 30^\circ$,

$$f = 0{\cdot}010316,$$

$$= \frac{1}{100} \text{ nearly.}$$

What is meant by such a coefficient of friction may be thus realized. Mr. Heath remarks that it is only necessary to consider the friction on the bottom of the canal. The effect is assumed to be to check equally all the liquid in each vertical column, as if it were all directly subject to the action of the bottom (and of the crust at the top also in the case of the substratum). This, as he says, "can hardly be true, though probably the resulting calculation exhibits something closely resembling the case of nature: for it is the *average* forward velocity on which the form of the wave depends." But it seems to me that the reciprocatory nature of the motion of the liquid will nearly confine the effect of friction to the two surfaces, because scarcely will the retardation have been propagated far before it will be reversed in direction. The case will be somewhat analogous to the propagation of seasonal variations of temperature into the earth's surface.

We have then, integrating the equation

$$\frac{dv}{dt} = -fv,$$

$$t = \frac{1}{f} \text{ h.l. } \frac{C}{v},$$

where C is the initial velocity. Whence, if $f = 0{\cdot}01$, such an amount of friction would reduce the velocity to one half in $100 \times$ h.l. 2, or 69·3 units of time. For instance, if a stream of the material were flowing over level ground with a velocity of one mile an hour, it would be reduced to half a mile an hour in 69·3 hours. As the layer of liquid may, from what has just been said, be likened to a solid layer whose upper and under surfaces are lubricated by a sufficient thickness of the liquid, this would seem to be a low degree of viscosity for such a substance as molten rock.

In the case of the semidiurnal tide $\alpha = 1520$ feet per second; and if we put $f = 0{\cdot}01$, 2δ will be about 87°, and $\cos^2 2\delta = 0{\cdot}0024$. This factor will render the diminution of the ocean-tide practically inappreciable.

(13) The final result therefore is that, upon the canal theory of the tides, if there be a liquid substratum of 60 miles or more in depth beneath the crust, and resting on a rigid nucleus, upon the supposition (of course impossible) that the

substance is perfectly without friction, the fortnightly ocean-tide in an equatorial canal would be reduced to about 0·6 of its calculated amount. It would require a coefficient of friction of 0·01 to bring this tide up to 0·7 of its calculated value. But the same amount of friction would have so great an effect in reducing the semidiurnal tide in the substratum, that the ocean-tide of that period would not be perceptibly affected. This is entirely in accordance with the *à priori* reasoning suggested in my book, as referred to at the beginning of this article. Without friction, however (a condition impossible in nature), the semidiurnal tide would be reduced to one half its calculated value.

Should then observations on the fortnightly tide lead to the conclusion that it is reduced to somewhere about 0·7 of its calculated value, it would appear that such a result would on the canal theory agree perfectly well with the theory of a liquid substratum upon a rigid nucleus. And, further, under these circumstances no appreciable diminution of the semidiurnal ocean-tide could be expected.

The entire range of the fortnightly tide at Teneriffe, upon the supposition of a rigid earth, would be, according to Sir William Thomson, 4·5 inches*.

August 12, 1882.

XXV. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* By J. J. Thomson.

To the Editors of the Philosophical Magazine and Journal.

Trinity College, Cambridge,
August 21st, 1882.

Gentlemen,

I INFER from Prof. Clausius's letter in the last Number of the Philosophical Magazine that he has misunderstood my position with regard to the question of the dimensions of a magnetic pole in the electrostatic system of units. I did not attempt to show that Maxwell's value was in accordance with Ampère's theory as Prof. Clausius interprets it, but endeavoured to show that Maxwell's value was the necessary consequence of the principles laid down in his treatise; and to point out what modification of Ampère's theory these principles lead to. From what Prof. Clausius says about Ampère's theory being independent of electrodynamic considerations, it would seem that he understands the theory to state that every small magnet *is* an electric current, and not that the magnetic effects of every small magnet may be represented by those of

* Natural Philosophy, § 845, ed. 1867.

an electric current, since this statement is only intelligible on electrodynamic considerations. The theory in this form seems rather hypothetical for the foundation of a system of units. Prof. Clausius does not think it objectionable that his formula for the magnetic force between two poles is not in the electrostatic system of units of the dimensions of a force. But since the attraction between two poles is as much a force as the attraction of the sun on the earth, if the expression mm' / r^2 is not of the dimensions of force on the electrostatic system, it is clear that this formula no longer represents the force between two poles, and that another factor must be introduced to make the expression of the right dimensions. Now it is one of the great advantages of Maxwell's system that all his formulæ are true as they stand, and do not require the arbitrary introduction of a factor on passing from one system of units to another; these factors introduce themselves naturally through symbols representing some physical property of the body or medium. As Maxwell does not dwell on this point in his book, I may be pardoned if I quote a few illustrations of it.

Using the notation of Maxwell's treatise, the force between two electrified particles $= \frac{ee'}{\kappa r^2}$, where κ is the specific inductive capacity of the substance. Now in the electrostatic system the dimensions of e are $(M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1})$, and κ is of no dimensions in mass, space, and time. In the electromagnetic system the dimensions of e are $M^{\frac{1}{2}}L^{\frac{1}{2}}$, and κ is of dimensions $L^{-2}T^2$. Thus in both these systems the expression $ee' / \kappa r^2$ is of the dimensions of force; and the factor changing from the one system to the other makes it appearance in the κ.

The same thing is true for the force between two magnetic poles. The expression for the force is $mm' / \mu r^2$, where μ is the magnetic permeability of the substance. In the electrostatic system m is of dimension $[M^{\frac{1}{2}}L^{\frac{1}{2}}]$, and μ of dimensions $[L^{-2}T^2]$; in the electromagnetic system m is of dimension $[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1}]$, and μ of no dimensions; thus in both systems the expression $mm' / \mu r^2$ is of the dimensions of force. Other illustrations might be given; but the reader can easily verify the statement that in Maxwell's system every equation is true as it stands; and consequently, whenever we have a purely dynamic effect, the expression for it will be of the same dimensions in both systems of units.

I am, Gentlemen,

Your obedient servant,

J. J. Thomson.

XXVI. *A new Form of Magnetic Torsion-balance and Magnetometer.* By Frederick John Smith*.

In the torsion-balance, such as used by Coulomb to measure magnetic forces, two poles of the suspended magnet are acted on. The end the author has had in view is so to place one pole of the suspended magnet that it shall not be acted on by horizontal pull. A magnet of rectangular shape, N B S, fig. 1, having a brass counter-weight A B, is suspended by the filaments C D so that the south pole is in the axis of rotation of the whole mass; thus the magnet may be regarded as having only one pole that can be acted on by horizontal force. In addition to this, a little mirror is so placed on an axis, O M, and attached to S by a short lever P, that the ratio of the deflection of N to the rise of the whole mass is at once shown on the scale, R, by the usual reflecting method. In the magnetometer for determining the pole-strength of magnets the same kind of rectangular magnet is used, attached to a single horizontal wire, A B, fig. 2; the magnet is furnished with a mirror-scale and lamp.

Fig. 1.

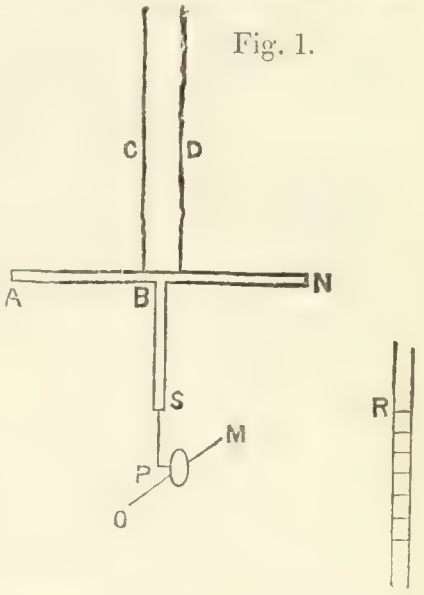
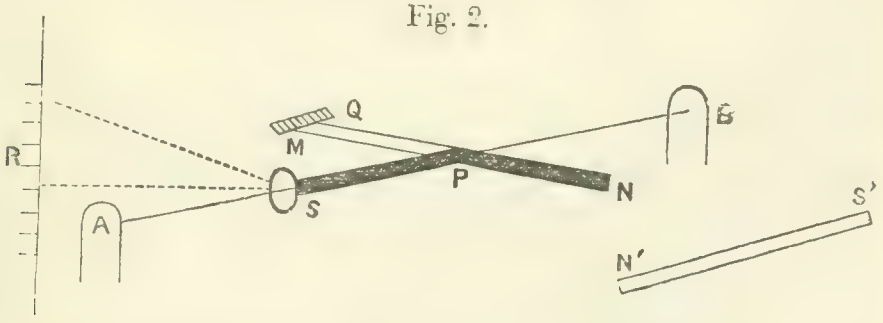


Fig. 2.



N P S, rectangular magnet; S, mirror; R, scale and lamp.

The instrument is used thus:—The weight at Q and the magnet N′ S′ being removed, the magnet N P S is set in a horizontal position by means of the torsion-wire; then a known weight, Q, is placed at M, M P = P N, and the deflection on the scale is recorded; then the weight is removed, and the

* Communicated by the Author.

same deflection produced by bringing up the N pole of the magnet N′ S′. The magnets are made of equal pole-strength and tested in the usual manner; the distance between them is taken in centimetres, the weight Q being in parts of the gramme. In the formula which shows the relationship between magnetic forces, $f = \frac{m \times m'}{d^2}$ (where f = force, $m\,m'$ pole-strengths, d distance between them), if $m = m'$ as in this instrument, then $f = \frac{m^2}{d^2}$; and if f be replaced by Qg (*i. e.* the numerical value of the weight × gravitation), the formula becomes $Qg = \frac{m^2}{d^2}$, from which m is known in absolute measure. With the pole-strength thus obtained the strength of other magnets and solenoids can be readily compared. In order to secure great strength, the frame which carries the torsion-wire is a gun-metal casting, having a rib at the back.

Taunton, July 27, 1882.

XXVII. *Notices respecting New Books.*

A Treatise on the Distillation of Coal-Tar and Ammoniacal Liquor. By George Lunge, *Ph.D., F.C.S., Professor of Technical Chemistry in the Federal Polytechnic School, Zurich.* London: Van Voorst, 1882.

Dr. Lunge is so well known, not only from his published investigations, but also from his 'Treatise on the Manufacture of Sulphuric Acid and Alkali,' that we were desirous of ascertaining how he would handle the subjects of coal-tar and ammoniacal liquor. A careful examination of his book enables us to pronounce most favourably upon it; and we regard it as a very valuable contribution to the literature of coal-tar and its derivatives.

Dr. Lunge commences his work by a preliminary chapter of 25 pages entitled "The Origin of Coal-tar," in which he discusses the differences between the tars derived from peat, browncoal, bituminous coal, and real coal, and also the effect of the temperature at which the coal is distilled upon the resulting products. We quite coincide with the author that the system well-nigh universally adopted, of squeezing the maximum quantity of gas out of the coal and letting the tar come out as it may, is a bad system. We may, perhaps, be hardly prepared to accept the dictum recently uttered at the annual meeting of gas-managers by a well known American, that, before long, gas will become the "residual" and coal-tar the principal object in the carbonization of coal: nevertheless it is, we think, indisputable that coal-tar is a necessity of the age, and will become rapidly of greater and greater value.

In Chapter II. a table is given of the constituents of coal-tar.

This table is the most complete that has yet been published. It must be remembered, however, that it includes the products of Boghead and other bituminous shales as well as those of true coals. After the table, is given a brief but sufficient account of the chemical and physical characters of the principal substances mentioned in it. The sketch of the history and properties of benzene is very complete, and includes a clear description of the views of its constitution propounded by Kekulé, Claus, and Ladenburg. The author is quite right in saying that it would be well-nigh impossible to find one's way through the interminable field of the aromatic compounds, if Kekulé's theory of the constitution of benzene had not brought light and order into it.

Chapter III. is devoted to "The Applications of Coal-tar without Distillation." It is well written and very interesting, and describes the various attempts that have been made to utilize it for fuel, varnishes, &c.; it is, however, becoming too valuable for these and similar methods of getting rid of what was once regarded as a nuisance.

The chapter on the "First Distillation of Coal-tar" is so minute that there is even a section on the "Carriage of Coal-tar." Even the best form of casks is discussed: and we are informed that "Long barrels are said to be pulled more easily than those more bellied." Does the author mean rolled instead of "pulled"? The sentence seems rather obscure. The distillation of tar by steam and fire is fully described; and the best forms of stills, condensers, and rectifying apparatus are illustrated by engravings.

In treating of Pitch, the manufacture of artificial asphalt, and of the so-called asphalt-pipes used for conveying water, acid, air-blasts, for covering underground telegraph-wires, &c. &c., is described, and instructions are given for the preparation of artificial fuel. The distillation of pitch for the production of anthracene oil is fully treated; and engravings are given of Fenner and Versmann's apparatus.

Chapter VI. is devoted to the working-up of the anthracene oil, which is effected by cooling to cause the solid hydrocarbons to deposit; the whole is then pressed, and the liquids are returned to the heavy oils, or employed as lubricants, or are redistilled. The solid portion constitutes rough anthracene, and may be sold as such, or be submitted to further purification. The preparation of anthracene has, however, been very fully treated of by Auerbach, whose excellent treatise has been translated into English, and is in the hands of every one interested in the subject. The chapter on Creosote oils is a valuable one; but we think the author is scarcely sufficiently alive to the comparatively small part played by phenol in the preservation of wood. Certain it is that long after the phenol has so far disappeared that it is almost impossible to detect it even by the most delicate tests, the wood continues to remain sound for many years. We are also of opinion that those modern specifications for creosote oils which insist that no naphthalene shall be deposited at 40° F. have been issued under a false impression of

what constitutes a good creosote oil, and would exclude oils of the highest preservative character. We have lately examined timber "pickled" 30 years ago with creosote oils from the London tar, and which oils when cooled to 40° F. yielded a very large amount of naphthalene; and yet the wood remains perfectly sound to this day.

The directions for the preparation and estimation of pure phenol (carbolic acid) are very minute, and include the results of the most recent workers on the subject. The author also describes the methods of preparing carbolic soap and disinfecting powders.

The chapter on Ammoniacal Liquor is well written, and contains a full account of all the substances contained in it; directions are also given for estimating its value, and for working it up. This latter part of the work is fully illustrated with engravings.

In conclusion, we congratulate the author upon having produced a work which is absolutely indispensable to all manufacturers of coal-tar products.

Worked Examination Questions in Plane Geometrical Drawing. *By* F. E. Hulme. Longmans, Green, and Co.: London.

This work consists of three hundred questions taken from old examination papers, two thirds of which have figures corresponding to them, said to be *solutions* of the problems. There is no attempt at classification; on the contrary, it has been purposely avoided by the author, and for a reason with which we do not hold. There are no demonstrations, and in most cases only scant directions; and even these are given where least, and omitted where most, needed. Many of the constructions are empirical, and incapable of being demonstrated: hence, from a mathematical point of view, they are not solutions at all. Scale Questions, those bugbears of Candidates for Military Examinations, ought to have been collected, and complete solutions of typical cases given. What good results from answering precisely similar questions over and over again? Notwithstanding its many defects, there is much in the book to recommend it. The questions are such as are certain to be encountered in Woolwich and Sandhurst papers; and the constructions are well drawn and conveniently placed for easy reference.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON THE DURATION OF THE PERCEPTION OF LIGHT IN DIRECT AND INDIRECT VISION. BY AUG. CHARPENTIER.

After various experimenters, I have sought to determine the time that elapses between the appearance of a light before the eye and the making of a signal by the subject of the experiment as soon as he perceives the light. There was interest in ascertaining if the duration of the perception was different for the centre and for the excentric portions of the retina, if exercise could modify that duration, and if the modification would or would not be limited to the part exercised.

For these experiments the eye, place at the centre of a Landolt's perimeter, looked into a large box lined with black, in the bottom of which a perforation had been made, about 1 square centim. in

section, usually closed by a plate lined with black, which plate, heavy and metallic, was retained in its position by the attraction of an electromagnet, but without coming into immediate contact with the latter, so that as soon as a person placed behind the box interrupted the current animating the electromagnet the stopping-plate instantly fell and disclosed the window placed before the eye which was under experiment. A current supplied by a laboratory Gramme machine, after passing through the electromagnet, put in action a small Deprez signal, the pen of which left its trace on a registering cylinder with a Foucault regulator. The signal immediately announced the interruption of the current, and consequently the precise moment of appearance of the light. Then the subject under experiment, directly after perceiving the light, restored the current in the signal through a derived path, by pressing on a spring the index finger of his right hand; precisely at this moment a new sign is traced upon the registering cylinder.

The interval which had elapsed between the interruption and reestablishment of the current, measured by comparison with the vibrations of a Marey electric chronograph, indicated directly the time which had been required for the subject to perceive and signal the light. For shortness, I shall call that time simply the duration of the luminous perception.

Here are the principal results which I have obtained in this investigation:—

(1) For one and the same person, under the same conditions, the duration of the perception varies from single to double without any apparent regularity. But if in one and the same experiment the mean of a sufficiently large number of successive determinations be taken (ten for example), a duration constant during the whole time of the experiment is found. I have found for myself, in direct vision, a mean duration of 0·13 second with daylight.

(2) The duration of the direct perception varies according to the individuals. I have seen it vary, according to the persons, from 0·09 to 0·15 second.

(3) The duration of the perception is sensibly the same for the right and for the left eye when they are sound.

(4) The duration of the luminous perception is notably increased by another cerebral occupation imposed on the subject during the experiment. Thus, when he speaks, when he listens attentively to a reading or a discourse, while at the same time applying himself to the experiment, he must have, for the reaction, 0·04 or 0·06 second more than before.

(5) The duration of the luminous perception is always more considerable in indirect than in direct vision; it is more considerable in proportion as the point of the retina struck by the light is more distant from the centre. This cannot be due to a difference of sensitivity, since, as I together with M. Landolt have shown, the retina is everywhere nearly equally sensitive to light.

(6) The difference between the duration of indirect and that of direct vision showed itself especially considerable at the beginning of our experiments. There was then between the duration of per-

ception for the centre and for a point situated 80° on the outer side in the visual field a difference of nearly 7 hundredths of a second. That difference was notably lessened by repetition of the same experiments during a month and a half; at the end of that time it was not more, for my left eye, than 2 hundredths of a second.

(7) If exercise attenuates the difference of duration of direct and indirect perception, it never annihilates it; so that the first constantly takes place more rapidly than the second. The influence of exercise asserts itself rapidly from the first sittings; afterwards it takes effect rather slowly, and then affects direct as well as indirect vision.

(8) Having established at the commencement that the duration of perception is the same for the left as for the right eye, I made, almost every day during a month and a half, fifty determinations on two well-defined points of my left eye only, excluding all other points of my two retinas. I thus exercised exclusively, a very great number of times, the centre of the *left* eye and the point of the *left* retina corresponding to 80° in the external part of the visual field (the internal part of the retina). At the end of that time I could estimate the influence of exercise by comparing the duration of the luminous perception on the same points in the *right* retina, and even on other points in both retinas. That duration was, for the centre of the left eye 0·129 second, for the centre of the right eye (not exercised) 0·143; at 80° outside for the left eye the duration of perception was 0·160 second; at 80° outside, for the eye not exercised, 0·210 second. Therefore exercise had notably shortened the duration of the reaction of the points experimented on.

(9) I wished to see if the abbreviating influence had extended over the *left* eye to points which had not been exercised. Now the duration of the reaction *was found to have been shortened in the same proportion for all the points of the inner half of the left retina* (the outer side of the visual field), *but not for the points of the outer half.* Consequently the exercise of an excentric point affects the different points of the same retinal hemisphere, but not those of the other hemisphere.

(10) The shortening influence *had extended to the outer hemisphere of the retina of the right eye*, while *the inner hemisphere reacted much more slowly than the same part, exercised, of the left eye.*

These facts can hardly be explained, except by admitting Wollaston's theory respecting the incomplete crossing of the fibres of the optic nerve in the chiasma, and supposing that the exercise of one part of the retina does not act merely on that part itself, but rather on the whole of the nervous centre, which receives both the fibres from the half of the retina containing the exercised point and the fibres from the half *on the same side* of the opposite retina.

Most of these experiments were simultaneously made by my assistant M. Bernardy, who aided me throughout, but, unfortunately, being able only to utilize the right eye for these researches, did not control points 8 and 10.—*Comptes Rendus de l'Académie des Sciences*, July 10, 1882, t. xcv. pp. 96–99.

AN AIR-THERMOMETER WHOSE INDICATIONS ARE INDEPENDENT OF THE BAROMETRIC PRESSURE. BY ALBERT A. MICHELSON.

The appearance of an abstract of a paper by Pettersson* on a new air-thermometer has led me to publish, sooner than I had contemplated, a notice of an instrument far simpler and more manageable than that which is there described, and which likewise retains the important advantage of giving indications which are independent of the external pressure.

The instrument consists of a glass bulb and stem, the former about 40 millim. and the latter about 2 millim. in interior diameter. The bulb contains dry air at a pressure of about 100 millim. of mercury; and this air is separated from the upper portion of the tube by a column of mercury about 100 millim. in length. The mercury remains above the air, notwithstanding the large diameter of the bore, owing to the resistance to deformation of the meniscus. The space above the mercury is a vacuum.

Thus the pressure of the air in the bulb is constant, and is equal to that of the column of mercury above it. If the bore of the stem is not of uniform section, the length of the column will change; but this length is easily read off, and gives at once the true pressure.

The pressure need not be limited to 100 millim.; but if it be much greater the instrument becomes inconveniently long.

The only precaution to be observed, beyond what is used in an ordinary mercurial thermometer, is that the stem must be kept vertical.—Silliman's *American Journal*, August 1882.

Case School of Applied Science,
Cleveland, O., July 5, 1882.

ON A PROPERTY OF THE ISENTROPIC CURVE FOR A PERFECT GAS AS DRAWN UPON THE THERMODYNAMIC SURFACE OF PRESSURE, VOLUME, AND TEMPERATURE. BY FRANCIS E. NIPHER†.

The equation of this thermodynamic surface is

$$pv = \mathrm{RT}; \quad \ldots\ldots\ldots\ldots \quad (1)$$

where p, v, T represent the pressure, volume, and absolute temperature, and where R is directly proportional to the volume of a unit mass (or inversely proportional to the density) of the gas at a standard temperature and pressure.

By differentiation, (1) becomes

$$dp = \frac{\mathrm{R}}{v}d\mathrm{T} - \frac{\mathrm{RT}}{v^2}dv. \quad \ldots\ldots\ldots\ldots \quad (2)$$

For convenience, putting

$$\frac{\mathrm{R}}{v} = \mathrm{A}, \quad \frac{\mathrm{RT}}{v^2} = \mathrm{B},$$

(2) becomes

$$dp = \mathrm{A}d\mathrm{T} - \mathrm{B}dv. \quad \ldots\ldots\ldots\ldots \quad (3)$$

1. To find the direction of maximum slope with respect to the v, T plane at any point on the surface. For this purpose pass a

* *Annalen der Physik und Chemie* (Beiblätter), No. 5, 1882.

† From Trans. of St. Louis Academy of Sciences, read April 3, 1882.

plane through any point in the surface, and at right angles to the v, T plane. Its trace upon the v, T plane is

$$T=\beta+av. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4)$$

p being indeterminate, where a is the tangent of the angle which the trace makes with the v axis, or

$$a=\frac{dT}{dv}. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5)$$

From (3) and (5) we have

$$dp=(Aa-B)dv. \quad \ldots\ldots\ldots\ldots\ldots \quad (6)$$

Calling S the slope of any element of the intersection of the plane and the surface, dz being the projection of the element on the v, T plane, we have

$$S=\frac{dp}{dz}=\frac{dp}{\sqrt{dv^2+dT^2}}, \quad \ldots\ldots\ldots \quad (7)$$

which by (5) becomes

$$S=\frac{dp}{dv}\cdot\frac{1}{\sqrt{1+a^2}};$$

and by (6) we have, further,

$$S=\frac{Aa-B}{\sqrt{1+a^2}}. \quad \ldots\ldots\ldots\ldots\ldots \quad (8)$$

In determining the direction of maximum slope at any point, it is evident that A and B will be constant, which gives as the required condition,

$$\frac{dS}{da}=\frac{A+Ba}{(1+a)^{\frac{3}{2}}}=0,$$

or

$$a=-\frac{A}{B}.$$

Substituting the values of A and B, we have

$$a=-\frac{v}{T}=-\frac{R}{p}=\tan i. \quad \ldots\ldots\ldots\ldots \quad (9)$$

For very low pressures, the direction of maximum slope $\frac{dp}{dz}$ becomes more and more nearly at right angles to the plane of p, v; while for high pressures this direction becomes more and more nearly parallel to the plane of p, v. The direction of maximum slope is constant along a line of constant pressure.

2. To find the direction of the isentropic line at any point on the surface, as related to the direction of maximum slope determined in (9).

Poisson's equation,

$$Tv^{k-1}=\text{const.}, \quad \ldots\ldots\ldots\ldots\ldots \quad (10)$$

is a projection of the isentropic line upon the plane of v, T, where k is the ratio of the specific heats $=1{\cdot}41$.

Calling a' the tangent of the angle which any element of this

projection makes with the v axis, we have

$$a' = \frac{d\mathrm{T}}{dv} = \tan i'.$$

This value of a' is obtained by differentiating (10), and is found to be

$$\frac{d\mathrm{T}}{dv} = -\frac{\mathrm{T}}{v}(k-1) = -\frac{\mathrm{P}}{\mathrm{R}}(k-1). \quad \ldots\ldots\ldots\ldots \quad (11)$$

Here also the condition of constant pressure gives a constant value for a'. Hence, at any point along any line of constant pressure, the production of an element of the isentropic line upon the v, T plane makes a constant angle with the projected line of greatest slope at the same point.

From equations (9) and (11) it follows that

$$\tan i' = \frac{k-1}{\tan i}; \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (12)$$

from which it will appear that for either very high or very low pressure the isentropic line runs at right angles to the direction of greatest slope. The condition that it shall coincide with the direction of greatest slope is

$$\tan i = \sqrt{k-1} = \frac{\mathrm{R}}{p},$$

or

$$p' = \frac{\mathrm{R}}{\sqrt{k-1}}. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (13)$$

For air this pressure is about 3·2 millimetres of mercury; and for other gases it is proportional to the volume of a unit mass at a standard temperature and pressure.

The thermodynamic surfaces of various gases will lie the one above the other, those having the largest value of R being uppermost. If we now substitute the value of p' of (13) in the original equation of the surface, we have

$$v = \sqrt{k-1}\,\mathrm{T}, \quad \ldots\ldots\ldots\ldots\ldots \quad (14)$$

which is independent of R. Hence, for all gases which follow the law represented in (1), the lines on their respective surfaces, where the isentropic lines coincide with the direction of maximum slope (13), will all lie in a common plane passing through the axis of P and at right angles to the plane of v, T, its trace upon the latter plane being represented by (14).

If the gases have a common temperature while in this condition, (14) shows that they will also have a common density, which, when T is 273°, will be 0·000058 gramme to the cubic centimetre.

It will be observed that for air the pressure indicated in (13) is practically the same as that at which Maxwell's law for viscosity begins to fail. This, however, is a mere coincidence. The two phenomena have nothing in common, as is evident both from theoretical considerations and from experimental results.—Silliman's *American Journal*, August 1882.

ON THE INFLUENCE OF THE QUANTITY OF GAS DISSOLVED IN A LIQUID UPON ITS SURFACE-TENSION. BY S. WROBLEWSKI.

It has long been known that the solution in a liquid of a gas which is superposed to it diminishes the tension of its surface. M. E. Desains observed, twenty-five years since, that the rise of the meniscus terminating water at its contact with air was a little less than that formed with hydrogen, and a little greater than that formed with carbonic acid—that is to say, that it was lowered in proportion as the gas was more soluble. I have found that, in all the liquids which I have studied, the surface-tension in contact with air is a little greater than in contact with carbonic acid. Lastly, M. Quincke has shown that, in the case of ammonia and hydrochloric acid (which are highly soluble in water), the diminution of the surface-tension increased with the quantity of gas dissolved. On the other hand, a number of cases can be cited in which the more or less complete absence of the faculty of absorbing gases is always accompanied in a liquid by relatively great surface-tension, and conversely. Thus liquids whose coefficient of absorption is considerable (ether, alcohol, the oils) have a feeble surface-tension. Saline solutions, which absorb much smaller quantities of gas than water, have a greater surface-tension than the latter: and their tension increases with the quantity of the salt dissolved, while their capability of absorbing gases diminishes. By reducing the surface-tension of water by the addition of alcohol, the solubility of gases in the mixture thus formed is increased. And mercury, which, of all liquids, has the greatest superficial tension, is almost incapable of absorbing gas.

The study of the correlation between these two classes of phenomena may be commenced, on the one hand, by determining the laws governing the solubility of a gas in a liquid, and, on the other, by measuring the surface-tension of a liquid in contact with a gas, the solubility of which in the surface-layer of the liquid can be regulated at pleasure by increasing or diminishing the pressure upon the gas. These experiments are so much the more easy to perform, as the saturation of the surface-layer of the liquid is effected instantaneously, and the tension, depending only on the condition of that layer, follows with the same velocity every change in the pressure which determines the value of that solubility.

Up to the present time no one has considered the question in this light. On the contrary, it has been attempted to establish a theory of liquids which, while based on the facts which militate in favour of the existence of that correlation, denies the correlation itself. That theory, attributing to pressure a direct influence upon the surface-tension, leads to consequences at variance with the facts. Thus Kundt, having observed that the height to which a liquid ascends in a capillary tube diminishes in the same proportion as the pressure upon the gas is increased, has drawn from this fact the following consequences:—

There is an influence of pressure upon surface-tension. The observed diminution ought to be regarded as a tendency of the liquid to pass into the gaseous state. If the compression could be

carried far enough, not only all liquids, but solids also, such as salts, would finally be reduced at ordinary temperature to the gaseous state, as takes place under the action of heat. Finally, at a little higher temperature (M. Kundt made his experiments at one temperature only), cohesion being diminished by the increase of temperature, the decrease of surface-tension under the influence of pressure would take place still more quickly.

Having recently determined the solubility of carbonic acid in water under pressures of from 1 to 30 atmospheres, I proposed to myself to make evident and establish the correlation of these two classes of phenomena. Reserving the description of the method employed and the numerical data for a special memoir, I will only enunciate here the results of my experiments.

Under pressures of 1-30 *atmospheres, there exists a remarkable relation between the laws of the solubility of carbonic acid in water and the surface-tension of that liquid.* That relation can be expressed thus :—

1. *The product obtained by multiplying the surface-tension* α *by the pressure* P *under which the carbonic acid is placed is proportional to the saturation-coefficient* S *corresponding to that pressure*—that is,

$$\alpha P = AS,$$

where A is a coefficient which depends on the temperature and increases with it.

According to the first law of the solubility, the temperature remaining constant, $\frac{S}{P}$ *decreases in proportion as the pressure increases**. *Experiment shows that the decrease of* α *is proportional to that of* $\frac{S}{P}$. With the aid of this relation of the phenomena of capillarity, those of the solubility of the gas can be calculated, and conversely.

2. *The pressure remaining constant and equal to* n *atmospheres* (n *being greater than* 1), *it follows from the laws of solubility that the quotient* $\frac{\left(\frac{S}{P}\right)_{P=n}}{\left(\frac{S}{P}\right)_{P=1}}$ *decreases with the lowering of the temperature. Experiment shows that in this case the ratio* $\frac{\alpha_{P=n}}{\alpha_{P=1}}$ *of the tensions corresponding to these pressures decreases also.*

This result is in evident contradiction of M. Kundt's theory, since the lowering of the temperature, instead of retarding the decrease of the surface-tension, accelerates it. The phenomena are therefore completely independent of the pressure, and depend only on the state of saturation of the surface of the liquid—that is to say, on the quantity of gas dissolved in the surface-layer.

The above relation does not end at the pressure of 30 atmospheres. The solubility increasing less quickly than the pressure, tends towards a certain limit, which at 0° seems to be reached at

* See my Note, *Comptes Rendus*, t. xciv. p. 1355.

the moment of the liquefaction of carbonic acid, since that liquid does not mix with water. Experiment shows that the decrease of the surface-tension, becoming slower with the increase of pressure, tends also towards a certain limit, which at 0° is reached under the pressure at which the liquefaction of carbonic acid takes place; at that instant the surface-tension of the water is reduced to about one half*.

Bisulphide of carbon, which also does not mix with liquefied carbonic acid, behaves in a similar manner in contact with that gas. The decrease of its surface-tension also takes place at 0° much more quickly than at a higher temperature. It becomes slower, and ceases under the pressure of liquefaction of the gas.

In my next Note I will show that the phenomena present themselves under a different form as soon as we have to do with a liquid that mixes in all proportions with liquefied carbonic acid.—*Comptes Rendus de l'Académie des Sciences*, Aug. 7. 1882, t. xcv. pp. 284–287.

ON THE STRUCTURE AND MOVEMENT OF GLACIERS.

BY M. F.-A. FOREL.

M. F.-A. Forel, of Morges, Switzerland, has recently published (*Bibl. Univ.* III., vii. p. 329) an important memoir upon glaciers, embodying the results of observations by himself and M. Ed. Hagenbach-Bischoff, with a discussion of these results and also of those obtained by other observers. His argument rests plainly upon the well-attested fact that glacial ice has a distinctly crystalline granular structure, the mass being composed of a confused agglomeration of individual crystals, each optically distinct—and, moreover, that the size of these crystalline grains increases from the upper margin of the glacier at the limit of the névé, where they have the size of a hazel-nut, down to the middle part, where the size is that of a walnut, and further down to the extremity, where they are as large as a hen's egg. For example, at the lower extremity of the Aletsch glacier, or that of the Rhone, the grains have a diameter of 7 or 8 centim. In regard to this gradual increase in size of the individual crystals, the author remarks that two suppositions are possible: either the growth of some grains must go on at the expense of others less favourably situated, one gaining what the next loses, and absorbing as much heat as is disengaged by the crystallization; or each grain increases in size by means of the water which reaches it from above from the surface of the glacier. Of these two hypotheses, the first is rejected, on the ground that, wherever observations have been made, they have shown the grains to be all of sensibly the same size in the same region, and not to be some small, others large, as this explanation would require.

Accepting provisionally the second hypothesis, the author remarks that for the increase in volume of the crystals there are needed water, cold, and favourable conditions. About the last point nothing is definitely known; but the others admit of further dis-

* The case in which one of these two liquids is superposed to the other does not come within the scope of this communication.

cussion. The water is believed to be afforded by the melting of the upper surface of the glacier under the influence of the heat of summer. This water runs over the surface of the ice, descends into the crevasses, and, if it be admitted that the ice contains capillary fissures (a point which is discussed later), much of it would be absorbed by the mass of the glacier and used in increasing the size of the crystalline grains; the rest of the water flows off in the subglacial torrent. The low temperature needed for the solidification of the absorbed water is believed to be due to the continued loss of heat during the winter, the glacier as a whole being a mass the temperature of which can never be above zero, but may fall considerably below. The question as to the mean temperature of the ice at different seasons of the year is discussed at length; and the author concludes, for a variety of reasons which cannot be quoted here, that the middle of the mass of the glacier has probably a temperature at the end of the winter several degrees below 0° C. This excess of cold would be partially expended in causing the solidification of the water which, as already stated, is absorbed into its mass and thus goes to increase its volume. The crystalline grains are therefore to be conceived as growing by accretion, successive layers being added to them at the expense of the water derived from surface-melting, and in the process of the warming of the glacier which goes on during the summer.

Assuming the correctness of the results of Hugi as to the increase in size of the crystalline grains—that is, in brief, that they increase from a diameter of 1 to one of 4 centim.,—taking 100 years for the time of their development, the author finds that the annual increase in volume is $4\frac{1}{2}$ per cent. Assuming, further, that the cold of winter is all employed in bringing about this increase, it is calculated that the hypothesis advanced is satisfied if the temperature of the glacier descends in winter to −6·8 C., or in round numbers −7° C. This temperature, the correctness of which is obviously dependent upon the accuracy of the assumed data as to the rate of increase of volume, is too low to be accepted, and leads to the inference that a part of the increase is accomplished by a process different from that which has been described. Thus at the end of the summer a considerable portion of the glacier must be at the temperature of melting ice, and in the capillary fissures between the crystalline grains there must be water; now, as the glacier cools down in the autumn, the first effect of the loss of heat would be the solidification of this water, and the consequent increase in size of the crystalline grains. Taking into account this last point, the author considers that the temperature that would have to be assumed for the glacier at the end of the winter would be quite within the range of possibility.

The hypothesis which has been advanced depends upon the assumption that the water can find its way into the interior of the glacial mass through the capillary fissures separating the individual grains. This point is one which is yet somewhat doubtful; and the author, after considering the various observations of Agassiz and others, which tell for and against the possibility of such a

penetration of the water, discusses the question from a more theoretical standpoint, and concludes that the assumption of the impermeability of the glacier is contrary to fact. He promises, further, to make this a special subject of observation at a later period.

In regard to the cause of the movement of glaciers, M. Forel places himself on the side of Hugi and Grad in supporting the theory of expansion, although modifying somewhat their hypothesis. On the old dilatation theory, it was the expansion of the water contained in the capillary fissures at the moment of their solidification to which the glacial movement was supposed to be due. According to the view of M. Forel, however, this special expansion plays a subordinate part; and it is rather the gradual increase in volume of the crystalline grain, due to the molecular affinity which causes a crystal to grow in the mother-liquor in which it is placed.

In discussing further the application of the hypothesis, a distinction is made as to the course of events during the youth and during the old age of the glacier. The glacier may be divided into three parts. The first is in the elevated region where the glacier has its commencement, that of the névé. Here the heat of summer is not sufficient to melt the whole volume of the snow which falls during the year; only a part of the snow is consequently transformed into water; and this penetrates into the layers below, and is solidified there: the temperature is much below the freezing-point. This is the region of the infancy of the glacier. Following this comes the line of separation, where the heat of summer is just sufficient to melt the winter's snow, and there is no excess of heat to attack the ice.

The second stage (that of the youth of the glacier) is found below this line of separation, where the summer's heat not only melts the snow but also partially melts the ice; the water so formed is absorbed and assimilated by the ice; and the temperature below the surface is, even at the end of summer, below zero. In this region the glacier is increasing in volume, and consequently moving downward. Then follows a second line of separation, where the water absorbed is all used in the increase of volume of the glacial grain. At this point the subglacial torrent has its origin; and at the summer's end the temperature is at 0°.

The third stage is that of the old age of the glacier, where the supply of water exceeds that needed to bring the temperature of the ice back to 0°; the excess of water flows off in the glacial streams. The temperature of the ice is at 0° during the summer; and the excess of the summer's heat goes to cause the melting and destruction of the glacier.

In concluding his interesting memoir, the author promises to test his hypothesis by further observations and experiments, bearing especially upon the questions as to the comparative size of the crystalline grains in the different parts of a glacier, and as to the possibility of the penetration of the surface-water into the mass of the ice. —Silliman's *American Journal*, August 1882.

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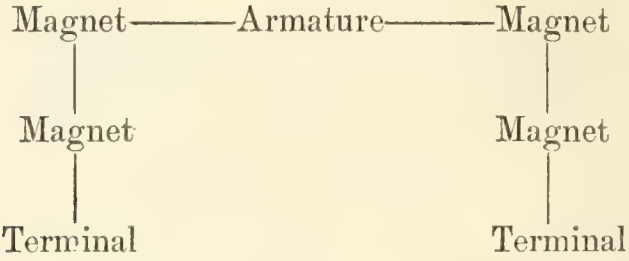
XXIX. *Notes on Practical Electricity.* *By* R. H. M. Bosanquet, *Fellow of St. John's College, Oxford.*

THE work I have had in hand for some time past has been the design and construction of clock-regulated uniform-motion machines. As to these, I will only say that the constructions formerly described by me have been entirely superseded by a new design. This is not yet in a state of sufficient forwardness for description, and I propose to reserve it for another occasion. The working of these machines is dependent on the application of a considerable amount of electrical power. After seeing the Paris exhibition I decided that in all probability a dynamo machine would be a better source for laboratory purposes than batteries, which are in my opinion a nuisance, and subject to most serious defects when used on the large scale. I accordingly set up an A Gramme machine. I also have made a set of accumulators. The practice of the employment of these instruments for ordinary purposes gives rise to numerous points of interest; and I propose to give a short account of my experience.

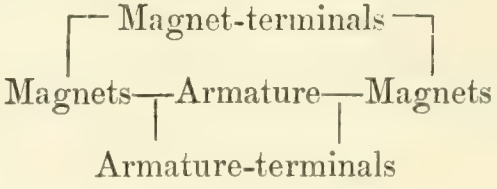
The laboratory steam-engine is able to supply sufficient power to give a powerful electric light from the machine. I take it that under these circumstances about $2\frac{1}{2}$ horse-power are absorbed altogether. The nominal horse-power of the engine is 2. It is scarcely sufficient to develop the full power of which the dynamo is capable; but for laboratory purposes it is quite sufficient.

The A Gramme machine has four electromagnets, two above and two below, whose axes are in one plane. In the same plane is the axis of the Gramme ring armature. The resistance of the machine is about 1·2 ohm.

The magnet-wires are directly connected with the armature-brushes; and the terminals receive the wires after each has passed through two magnets. Consequently all currents have to pass through both magnets and armature, thus:—



For purposes which will be presently described, it was found necessary to be able to separate the magnets from the armature. The wires were therefore cut, by which means the following combinations were obtained :—



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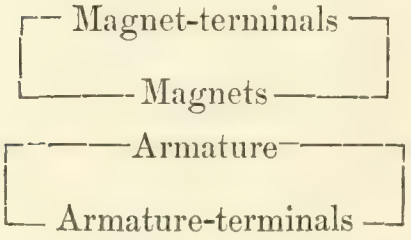


in which last arrangement the magnets are separately excited. There are other forms, which will be described presently.

The resistance of the magnet circuit is about ·75 ohm. That of the double course through the armature is therefore about ·45 ohm, the total resistance being taken at 1·2.

The terminals are marked + and − respectively. When the machine is acting in correspondence with these indications, I call it " straight," and the upper pole-piece attracts the unmarked end of a compass-needle. The residual magnetism is very strong. I think the whole of the solid part is probably made of cast iron.

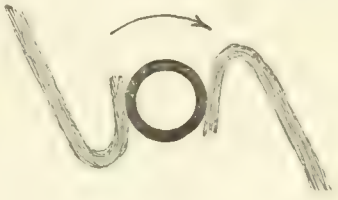
When the polarity gets reversed from any cause, I call it "reversed." In this case the upper pole-piece attracts the marked end of a compass-needle.

The directions of currents and rotation deserve some attention.

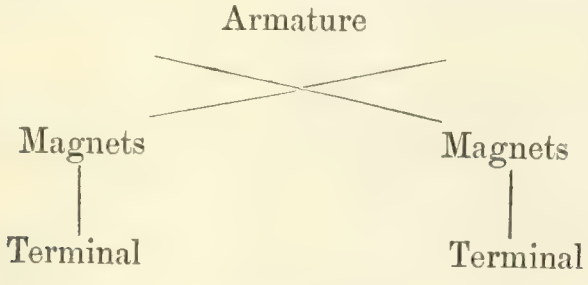
The machine is driven in the direction opposite to that of the hands of a watch, regarded from the pulley-end. It gives currents in the one or the other direction according as it is "straight" or "reversed." This is obvious; for with reversal of the current both field-magnets and armature are reversed, and the attractions which have to be overcome in doing the work remain the same.

When a current is sent through the machine from a battery, it always turns in the direction opposite to that in which it is driven by the engine; for whichever way the current goes the attractions are the same as in the former case, and as these are overcome by the engine in that case, it is clear that they tend to turn the machine in the opposite direction to the engine.

When the machine is driven by a battery, with the hands of a watch from pulley-end, it is necessary so to adjust the brushes that their ends may not catch in the commutator. I turn up the ends of a spare pair of brushes, and apply them to the commutator in this manner.



Otherwise the machine can be caused to turn in its normal direction when driven by a current, by reversing the connexions between armature and magnets; this is made possible by the cutting of the wire above described. The arrangement may be formulated thus:—



The machine is constructed for driving one arc light. Although this is not a matter with which I am directly concerned, the normal conditions may be of interest:—

Serrin lamp.
Revolutions about 900 per minute.
Current about 20 ampères.
Difference of tension between lamp-terminals about 40 volts.

These are the highest values obtained under favourable circumstances.

According to these numbers the resistance of the arc is about 2 ohms; that of the machine and connexions is about 1·5.

This corresponds to 800 volt-ampères, or rather more than 1 horse-power, in the light itself (1 horse-power = 746 volt-ampères).

We observe here that little more than half the power expended is developed in the lamp.

Circuits of High Resistance.

Suppose I take a single British incandescent lamp (150 ohms cold, and about 80 ohms hot), the machine only just raises it to a red glow, though a speed of as much as 2000 revolutions per minute be employed. The current developed is much less than 1 ampère.

Here the high resistance kills down the current, and prevents the proper excitation of the field-magnets. Hence the advantage in this case of machines in which the magnets form a shunt circuit of high resistance. It is easy to excite the Gramme machine by putting the magnets into a shunt circuit with a suitable resistance; but if a resistance-wire be employed, the power developed in heating it is wasted. I have therefore in many cases adopted the plan of putting useful work into the shunt-circuit of the magnets. This may sometimes with advantage consist of a number of incandescent lamps in parallel circuit. Substituting for this a certain number of accumulator-cells, we have the origin of a useful method which I call charging in balanced circuit, to which I shall return.

Galvanometers.

The galvanometers I employ were made in the laboratory. They are of a simple character, but quite sufficient for practical purposes. There are three of them.

Tension Galvanometer.—A circular wooden channel about ·11 m. radius, wound with wire of which 1 metre = roughly 10 ohms. Resistance = 2900 ohms. This had its constant determined by reading the current from 9 Daniell's cells, which are taken as representing 10 volts. Resistance of Daniell's cells determined and allowed for. For high ten-

sions a further resistance of 5000 ohms is interpolated in the galvanometer circuit. Brass dial 3 inches in diameter divided to degrees. Short steel needle with long pin-points. 50° on the galvanometer, with the 5000 ohms external resistance, corresponds very nearly to 50 volts. Other readings by the tangent-law.

Two Quantity Galvanometers.—These consist each of a ring of gun-metal, ·11 m. radius; the conductors leading to them are copper, and of tube-and-core form; brass dials, and steel needles with long pin-points as before.

When dealing with large currents I placed a steel magnet under one of these, so as to convert it into a high-quantity instrument. Its constant was ascertained by sending the same current through both instruments, the one without the magnet reading as a tangent galvanometer in absolute measure.

Measures.

Although numerous measures of dynamo machines have been published, the laws of any given machine cannot as yet be predicted; and a few measures are given, which are sufficient to illustrate the general course of the performance of this machine under different circumstances.

First, we will consider the cases where the governor was used. This may be taken to give 840 revolutions in all cases, except where, the resistance being 3 ohms or less, the leverage against the engine was such as to reduce the speed materially.

Resistance in parallel circuit with lamp. ohms.	Tension between terminals. volts.	Quantity. ampères.
7	24·5	3·8
6	31·4	5·5
5	37·9	7·6
4	41·0	10·5

With lower resistance the speed cannot be maintained without taking the governor off. In considering the total electromotive force developed, of course the whole resistance of the circuit must be considered. The resistance of machine and connexions may be taken at 1·5. Then, multiplying the current by the whole resistance, we should have the total electromotive force.

The work of Meyer and Auerbach leads for a given speed to equations of the form $\theta = a\,\mathrm{E} - b$, where θ is an angle whose tangent is the measure of the current, and E the total electromotive force. I used the expression at first as it stands;

but now I think it better to eliminate E as follows:—Putting $E=CR$, the above form gives $CR=\frac{\theta+b}{a}$ or $R=\frac{\theta+b}{aC}$, where R is the resistance of the circuit, C the current, and θ the angle of a tangent-galvanometer by which the current is measured.

To apply this to the above observations, we find the following values of the total resistance, current, and θ:—

R.	C.	θ.	$R=\frac{\theta-20.}{C}$
7·9	3·8	50°	7·9
7·0	5·5	60	7·3
6·2	7·6	$67\frac{1}{2}$	6·2
5·3	10·5	73	5·1

(When calculating the resultant resistance of the parallel circuit, we notice that it should be the same as the quotient of the observed tension by the current. It does not differ in any case by more than one or two tenths of an ohm. This is sufficient, considering that no special accuracy was aimed at. Further, it must be remembered that the wires of the resistance are nearly or quite red-hot under these circumstances, so that some discrepancy is to be expected.)

Calculating R from the formula

$$R=\frac{\theta-20}{C},$$

we obtain the numbers in the last column.

This result is tolerably satisfactory; but it is of little practical use where the motor employed is a steam-engine capable of considerable variations of speed. With a gas-engine, or other motor of very constant speed, the above method of finding the approximate resistance for a given current would probably be useful.

But with my engine, when the governor is not used, the speed adapts itself to the work to be performed; so that, within considerable limits, the only things to be considered are the amount of steam turned on and the work to be done. To illustrate this I will quote a series of experiments, in which the object was to maintain the tension necessary for a "British" incandescent lamp, which is about 80 volts, with different resistances in parallel circuit.

The least resistance with which this tension can be maintained is 5 ohms. It is easily seen that the resulting external resistance here is nearly four times that of machine and connexions; so that the total E.M.F. would require to be 100 volts, roughly, to get 80 between the terminals. For smaller

external resistances the total required would be larger; and this cannot be obtained. The load on the engine is heavy; full steam is required; and the speed is moderate.

Increase the 5 ohms to 6, leaving the steam as before. The engine runs faster, and the required difference of potential is kept up with about the same head of steam.

Increase to 7 ohms. Similar effect.

Increase to 10 ohms. The engine runs very fast; but the required difference of potential is still easily maintained. Of course here the total E.M.F. required is decidedly lessened.

As the resistance increases beyond this point, the speed required to maintain a difference of potential of 80 volts becomes too great for convenience, and by the time it reaches 20 or 30 ohms it becomes impossible.

In view of this power of accommodation, the question as to the precise resistance required to produce a given current at a given speed becomes of little practical importance.

The explanation of the power of accommodation is, roughly, as follows. The total E.M.F. is to be supposed constant. In the preceding experiments it is not quite so, being 100 volts, about, with the 5 ohms, and 90 or so with the 10. But the conditions of constancy can be realized; for with the 10 ohms I kept up the difference of potential of the terminals to 90 volts for some time, this corresponding to about 100 volts total E.M.F.; so that, in fact, this condition of constancy of total electromotive force can be attained through a certain range with a little attention.

Then the current developed follows Ohm's law throughout this limited range, with varying resistances; and the attraction between the magnets and armature depends on the current in each; so that a rough idea of the course of the values may be formed by assuming the attraction proportional to the square of the common current through both; and this is the reaction against the engine. Consequently as the resistance in circuit increases the current diminishes (Ohm's law), and the statical resistance to the engine diminishes, as we suppose for the moment, in the square of the ratio of increase of resistance in the circuit. Of course, the real values of the attraction depend on the magnetizations of magnets and armature; but the above accounts in a general way for the increase of speed produced by introducing additional resistances into the circuit. The increased speed is then utilized in obtaining the same electromotive force from the diminished current, or rather from the diminished magnetization.

It will be readily seen how importantly this power of accommodation may be utilized in the practical treatment of such

a question as the variation of the number of incandescent lamps on a circuit. Take the case of lamps of 70 ohms. Then 7 of these in parallel circuit constitute a resistance of 10 ohms, and 14 one of 5 ohms. Between these limits the tension might be kept nearly constant by the automatic variation of speed above mentioned. I do not say that this is the best way of attaining the result; but with means such as I possess it is a valuable auxiliary. I believe as many as 25 such lamps can be driven from the A machine; but this must require far greater power than is at my disposal.

Accumulators.

I have made 21 cells with lead plates coated with red lead, and 9 of another kind, which I will describe presently. In all cases the plates are wrapped in canvas. The lead I employed was 2 lb. to the square foot; each plate is about a square foot in size; and the red-lead plates have 1 lb. of red lead on each surface. The tags at the end were left rather narrow. I found lately that all the tags of the oxidized plates were so eaten away at the surface of the acid that they had to be replaced. I now use lead 3 lb. to the square foot for oxidized plates; the tags are left 3 or 4 inches broad, and they are additionally thickened at the junctions by burning on a piece of lead on each side; they are also varnished.

These cells were "formed" by charging in the same direction every day for a considerable time. The maximum efficiency was reached in about a month, after which no further improvement was perceived.

These cells do not pretend to the excellence which is said to be obtained by the cells of the Faure Company. If left charged for any time, the sulphuric acid is rapidly and completely absorbed, leaving the water quite sweet, and considerable quantities of sulphate of lead are formed. This is slowly decomposed by fresh charging; but in the mean time it appears to isolate large portions of the active material, and considerably impairs the efficiency. I am informed that this local action does not take place with the cells supplied by the Faure Company. The red lead on the hydrogen plates becomes reduced to metallic lead, and that of the oxidized plates changed into black peroxide *. It is not until this change is complete that the cells attain any considerable efficiency.

* I am indebted to Mr. Fisher, of the Oxford-University chemical laboratory, for the examination of samples of these substances. The metallic character of the reduced substance was proved by amalgamation with mercury. The peroxide contained very nearly the proper quantity of oxygen, but fell a little short—not more than would be explained by inevitable impurity.

The cost of the construction is very considerable. I have little doubt that they have cost me on the whole as much as if I had bought them at the very high price charged. But much of the expense could have been avoided. As it was, the original construction cost under £2 per cell. About half the outlay went to provide the wooden cases lined with lead. If stoneware jars could have been procured, I think one third of the original cost might have been saved. The "forming" of the cells with the small power at my disposal was expensive. I have not estimated the cost; no doubt it could be done very much more cheaply on the large scale.

The best return I have ever obtained amounted to about one half the power expended. This was obtained after a charge of short duration, the cells running one Swan lamp. The current and tension were measured at intervals. The return was about 70 per cent. of the electrical charge taken, and, as I judge, about half the total power expended. But this is by far the best return I have ever obtained. As a rule, the return is only a small fraction of the power expended.

The other 9 cells are made with amalgamated plates of lead, and "formed" by charging in opposite directions in the manner practised with Planté batteries. These have less capacity than the red-lead cells, and are troublesome to form; but on the whole I am inclined to prefer them for laboratory purposes to home-made red-lead cells. They are much less trouble to put together, and do not suffer from local action.

Having cells of different kinds in the system is a great inconvenience. When the red-lead cells have suffered from local action, they increase their resistance; and if systems of balanced charging are employed, in which two or more circuits are used, a progressive change takes place in the distribution of currents, which requires constant watching. It should be a first principle that all the cells should be as similar as possible.

The red-lead cells have 11 plates in each cell, the others 15.

With all their imperfections these cells are extremely useful for laboratory purposes; and as this was my object in constructing them, I am on the whole satisfied with their performance.

On charging Accumulators.

Suppose that the accumulators are entirely without charge, and the machine polarized rightly, having its residual magnetism such that the + terminal gives a positive current and the − one a negative current. Then in charging we join + (or oxygen) to +, and − (or hydrogen) to −, and all goes

rightly. Now suppose that, after the accumulator has acquired a sensible charge, we stop the dynamo machine without breaking the connexions. The power of the machine to drive back the battery-current was derived from its velocity, which is gone. Consequently the battery-current discharges itself through the machine in the opposite direction to the proper current of the machine, and reverses the magnetism of the machine. This change is recognized, as I have mentioned, by the effects of the pole-pieces on a pocket-compass. When the machine is "straight," the upper pole-piece attracts the unmarked end of the needle. When it is reversed, it attracts the marked end.

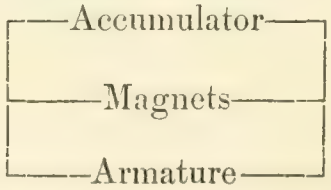
If the machine be then set in motion again, the effect is to strengthen the current in the magnets. But this is now the discharge current of the battery. Consequently the machine proceeds to pump the electricity out of the battery.

This reversal is prevented in ordinary practice in two different ways. In charging by Gramme machines, in general, two machines are employed, of which the one serves only to drive a current through the magnets of the other. This current is wholly independent of the charging current; and the arrangement is consequently not liable to reversal.

The power which is employed in this case to maintain the current in the magnets contributes nothing to the work; consequently this arrangement is not economical. But in a large establishment one small machine may be used to excite several large ones, and the waste is reduced to a minimum.

The best method for charging in general would appear to be the use of machines of the Siemens type, with the field-magnets in a shunt circuit of high resistance; for then the reaction of the battery-current seeking to pass backwards into the machine conspires with the forward current from the machine, so far as the supply of the field-magnets is concerned.

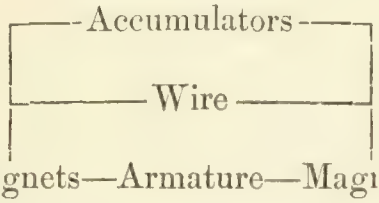
Diagram of Circuits in Shunt-Circuit Machine.



But as my machine is not thus arranged, I have had to devise other means of doing the work. I have not seen any account of these processes.

The simplest mode of effecting the charge is to charge in one series, starting by means of a resistance-wire in parallel circuit, thus :—A wire of 5 or 6 ohms resistance, arranged so that it can become red-hot without injury, is introduced between the terminals, and the machine driven at full speed. A difference of tension is thus produced between the ends of the wire, which must be greater than that of the accumulators to be charged. This difference amounts with my machine to 70 or 80 volts, or even more. Then, if we have 80 volts and 6 ohms of wire, we get a current of a little over 13 ampères, which is enough to excite the machine well.

The terminals of the battery are now joined up to the terminals between which the difference of tension is established, + to + if the machine is straight, + to − if reversed. The existing difference of tension drives back the battery-current, and a charging-current is set up. With the 30 cells in series I find that the current traversing the accumulators is about 10 ampères under these circumstances.



The machine is then traversed by the double current of more than 20 ampères from both battery and resistance-wire. Great statical resistance to the engine is consequently developed, and the magnets are well excited; but the current through the 6-ohms wire goes to waste, and in fact the wire is kept nearly red-hot. A large power is required to maintain this state of things. The last step is to remove the wire, leaving the battery with the current of about 10 ampères passing through it. The resistance at the machine is reduced to about one fourth by the halving of the current, and the engine quickens its speed; but, on the other hand, in order to maintain the difference of over 70 volts when excited only by 10 ampères, the machine requires a high speed.

Consequently there is a period of instability at the moment of removing the resistance, and the machine is often reversed before the high speed is established. The only means of obviating this are (1) to cram on every available pound of steam at the moment, (2) to execute the two operations of joining up the battery and removing the resistance in such quick succession that the speed has not time to fall. By the use of both these precautions I have generally succeeded in

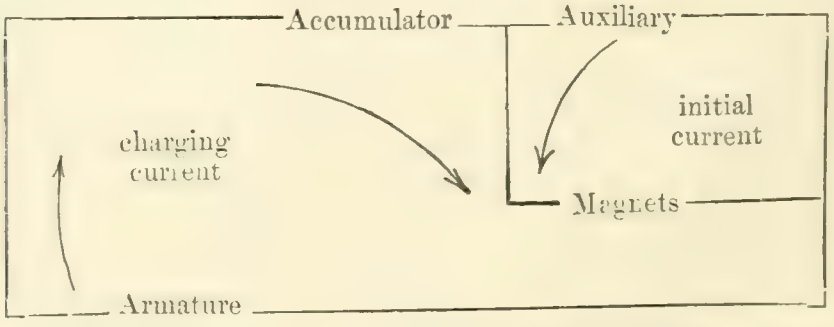
starting the charging in this form; but until I had acquired considerable experience and a thorough understanding of the conditions, I frequently failed.

When this arrangement is once started, and if sufficient power is available to keep it going, it is not only the most economical method of charging with this machine, but is more economical than any other method, excepting the case where the magnets form a shunt circuit of very high resistance indeed.

The objection to the arrangement in question is that there is a very small margin between the tension of the battery and that of the machine which has to overcome it; consequently at the slightest check to the steam, and frequently without any apparent cause, the battery-current will overpower the tension of the machine and reverse it. The motion of the machine tends to reinforce the current of the battery in the new condition, and the resistance of the machine is small; so that a tremendous current, probably more than 50 ampères, is poured through the machine, and the electromagnetic attraction resulting is powerful enough to stop the engine. This reversing is indicated by a hiss of the belt on the pulley, followed by the stoppage of the engine. The person in attendance has immediately to break the circuit, or the whole store of electricity would soon be poured uselessly away.

With the limited power at my disposal I have never been safe from reversals in charging in series in this manner when the number of cells to be charged amounted to twenty or more.

Another method of starting the charging in series is to throw a current through the magnets from an auxiliary battery or from a series of accumulators already charged, and then introduce those to be charged. The connexions are as follows:—



In this arrangement one terminal of the magnets must be disconnected from the armature, and the magnets made up into one circuit.

When the charging is started, the auxiliary battery is removed. An instability of the same sort as that above described takes place.

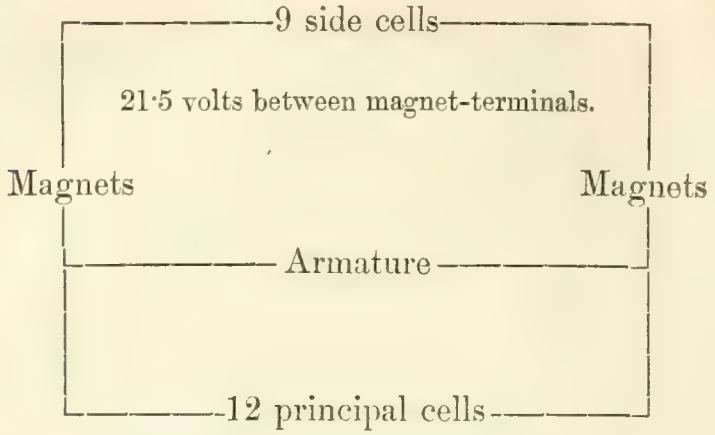
This method is not superior to the last, and is troublesome from requiring the auxiliary battery.

A very simple mode of charging, where the cells are all similar, is to make them up into two or more parallel circuits, according to the electromotive force available. When there are different kinds of cells this method is troublesome, as the two circuits have to be exactly balanced, and this must be done by trial. Further, the red-lead cells alter during charging as if their resistance diminished, which I take to be caused by the decomposition of the sulphate of lead with evolution of sulphuric acid. It is then necessary to keep watch at the galvanometers in the two parallel circuits and rearrange the circuits when necessary, which is very troublesome. Latterly, however, I have used this method more. With my machine it would be more suitable for charging about 40 cells. The charging is started with a resistance in the same way as in the first case.

The next method is an interesting one ; I found it useful when the cells were in good condition, with low resistance and with about 20 cells ; but I have used it also with 30 cells. When the resistance of the cells is high, it is better to use the last method with 30 cells.

I call the present method the method of balanced charging. It arose in this way :—I began by putting the magnets in a shunt circuit, with a resistance in the magnet circuit ; then accumulators could be charged in the main circuit (armature-terminals), and the current spent in heating the resistance in the magnet circuit went to waste. I therefore tried substituting cells for the resistance in the magnet circuit. I put the two similar quantity galvanometers in the two circuits, and varied the distribution of the cells until the deflections were equal; so that all the cells were getting uniformly charged. I found at once that under certain conditions a balance was established, so that the tension-difference on the two sides of the magnet circuits was small, and that, so long as the balance was maintained, there was no tendency to reversal.

The following scheme will explain one of these arrangements :—



In this case I had a balance when 9 ampères of current were passing in each circuit.

It is clear that the current through the magnets is due to the difference of tensions between the two sets of cells; consequently,

	volts.	
Tension of 12 cells	= 28·1	(measured)
,, 9 ,,	= 21·5	,,
	6·6	

and, dividing by the measured current of 9 ampères, we find ·73 ohm for the resistance of the magnets. This corresponds well with my direct measure of ·74, and less well with the resistance given by the makers, ·77.

The balance is a function of the current; and if the power in action be varied, the current in the principal cells changes faster than that in the magnet circuit (or side cells). It is to this circumstance that the stability of the arrangement is due.

In order to give foundation for the theory of this arrangement we have to represent the variation of the tension of the cells with varying current. We may represent the course of the changes sufficiently for this purpose, according to my measures, by assuming that the tension varies with the charging current, so that it is 1·9 volt per cell when at rest and 2·4 per cell when the current is 10 ampères, *i. e.* it varies $\frac{1}{20}$ volt per cell per ampère. The same rule expresses fairly well the diminution of tension when the freshly charged battery gives out a current. I shall return to the question of the rationale of this, and for the present assume general values, and that the change of tension is proportional to the current.

Let E be the tension of one cell at rest,
x the current in the principal cells,
y the current in the side cells,
ϵ the change of tension per cell per ampère,
m the number of the principal cells,
n the number of the side cells.
Then,

Tension of armature-terminals, or of principal cells
$$=m(\mathrm{E}+\epsilon x)\,;$$

Tension of magnet-terminals, or of side cells
$$=n(\mathrm{E}+\epsilon y).$$

Then, if R be the resistance of the magnets, since the difference of these tensions drives current y through R,

$$m(\mathrm{E}+\epsilon x)-n(\mathrm{E}+\epsilon y)=\mathrm{R}y;$$

or

$$(m-n)\mathrm{E}+m\epsilon x=(n\epsilon+\mathrm{R})y;$$

$$\frac{dx}{dy}=\frac{n\epsilon+\mathrm{R}}{m\epsilon}.$$

Put $n=m-d$, where d is the difference between the principal cells and side cells; then

$$\frac{dx}{dy}=1+\frac{\mathrm{R}-\epsilon d}{m\epsilon};$$

which is >1 if $\mathrm{R}>\epsilon d$.

In the case above given,

$$\epsilon=\cdot05,\quad \mathrm{R}=\cdot74,\quad d=3;$$

whence $\frac{dx}{dy}=2$ nearly, and the change of the current through the principal cells is twice that in the magnet circuit.

Of course it is the change in the magnet-current that alters the excitation, and gives rise to instability; so the more we throw the change off the magnet-current, the more stable the arrangement will be.

The behaviour of the balance as ascertained by experiment corresponds with this theory. The magnet-current alters more slowly than the other.

To deduce the magnitude of the balancing current in any particular case, put $y=x$ in the equation

$$m(\mathrm{E}+\epsilon x)-n(\mathrm{E}+\epsilon y)=\mathrm{R}y;$$

$$(m-n)\mathrm{E}+\{(m-n)\epsilon-\mathrm{R}\}x=0.$$

Let $m-n=d$ as before,

$$x=\frac{\mathrm{E}}{\frac{\mathrm{R}}{d}-\epsilon}.$$

If $\mathrm{R}=\cdot74$, $d=3$, $\epsilon=\cdot05$,

$$x=5\mathrm{E}.$$

If we assume $\mathrm{E}=1\cdot9$, $x=9\cdot5$, which is near enough to 9, the observed value, to show that we have a general representation of the facts. The constants are not determined with sufficient closeness for accuracy.

Again, suppose we wish to charge with a larger current. Put $d=4$ in the above; then we find for x the balancing current 14·6. So that we have only to increase the difference between the two sets of cells.

Again, supposing we wish to use a smaller current, put $d=2$. Then $x=6$, nearly; or the balancing current is about 6 ampères. Both of these conclusions have been verified experimentally.

With reference to the assumption just made as to the dependence of the potential on the current, and generally as to the constants of the accumulator, if we calculate the resistance of a cell of the accumulator from the data available, assuming that the plates are 1 centim. apart, and that the sulphuric acid has a strength even considerably less than 10 per cent., which is its original value, we find a result which is extremely small. In fact, specific resistance *

$$\begin{aligned} \cdot2 \text{ per cent.} &= 4\cdot47\times10^{10},\\ 8\cdot3 \quad ,, \quad &= 3\cdot32\times10^{9}. \end{aligned}$$

The intermediate value 10^{10} represents our case with sufficient approximation.

The edge of each plate is about 30 centim., and its surface not far from 1000 square centim.; so we may consider the conducting solid as consisting of one plate of unit thickness, and surface somewhat exceeding 10,000.

The resistance therefore would be $\frac{10^{10}}{10^4}=10^6$, or the thousandth of an ohm roughly. This prevents us from assuming that the change of potential caused by current is due to the spread of the tensions along a resistance according to Ohm's law. And it appears probable that any change of tension due

* These numbers are taken from Prof. Everett's 'Illustrations of the C.G.S. System.'

to such a source is negligible, so long as there is acid in the solution.

A key to the nature of the phenomenon may be found by considering what happens when the cells are being exhausted. If a current of 10 ampères, such as I have used for charging, be demanded from the accumulators, the tension falls off. If the demand continues, the cell becomes exhausted. If we let it stand, it will recover itself, just as is the case with a Leclanché cell, for instance*.

Now the simplest way of accounting for this, as well as for the rise of tension in charging, is to suppose that the chemical action, in which the storage consists, does not actually reside on the surface, but penetrates to a certain extent into the subjacent layers of the mass. It is easily conceivable that such an action should be only capable of transference through the mass at a certain rate; that when too large a current is demanded from the cell, the chemical change does not return from the interior layers to the surface with sufficient rapidity to maintain the current; and that when, on the other hand, a high-charging current is employed, it cannot get away fast enough into the substance from the surface, and is accumulated there and forced to a higher intensity. It is simplest to assume that this heaping-up is directly proportional to the current which causes it.

So far as the excitation of the magnets goes, the method of balanced charging is more economical than charging in series; but as a double current passes through the armature, there is some considerable waste in heating it.

The electromagnetic attraction of the machine is in this case roughly double that of charging in series (assuming for the moment that the magnetization is proportional to the current). This gives the engine a better hold of its work in the case considered without making the load so very heavy as when the double current passes through both magnets and armature.

The lead of the dynamo machine, or the angle at which the brushes have to be set forward on the commutator, is about double what it is under other circumstances. This is no doubt due to the exalted magnetism of the armature due to the double current taking longer than usual to be affected by the comparatively weak field-magnets through which only a single current is passing. This appears to point to the reduction of the

* So, if we try to drive an arc light, we can get for a short time a current of more than 20 ampères and a good light; but the battery soon begins to be exhausted, and a most curious effect sets in: the alternations of exhaustion and recovery succeed each other with great rapidity, and the carbons begin to chatter in a most extraordinary way.

lead by the employment of more powerful field-magnets. In all cases the lead must be ascertained by its being the position of least sparking, and the brushes set pretty closely to it; otherwise the brushes are burned away, and the sparking is unpleasant.

Having charged the accumulators, I divided them up into circuits as required, and employ them to work my electro-pneumatic and other appliances. I have driven every thing for a week after a charge of a couple of hours; but on account of the local action in the red-lead cells, this is not a very advantageous course; and latterly I have preferred to charge for half an hour in the morning of each day. If the charge is meant to last, of course the currents must be used economically.

There are considerations, however, connected with the inevitable variation in the power of the current when any description of cells is used as a source of electricity, which make me already doubtful whether this alternate procedure will answer, and whether it may not be necessary to use the cells with the machine running simply for the purpose of sluicing off small currents with fairly constant differences of tension at their sources from the main current of the machine. It is even possible that simple pairs of lead plates in acid without preparation may be sufficient for this purpose. But the discussion of this matter must wait until my uniform rotation-machines are in a more complete state.

I will close with an experiment showing, in a manner suitable for the lecture-table, the development of a counter electromotive force in the dynamo when driven by a current from the accumulators.

I place a Swan lamp in parallel circuit with the machine. Then, so long as the machine is at rest, the lamp does not burn. But when the machine is in motion, as the velocity increases the counter electromotive force is set up, and the passage of the current through the machine more or less completely barred. The lamp in the parallel circuit then burns up. If the machine is stopped by a brake or overloading, the lamp is extinguished again.

XXX. *On the Methods employed for determining the Ohm.* *By* G. Wiedemann*.

THE Congress of Electricians which met at Paris in the autumn of last year adopted the electromagnetic units based on the centimetre-gramme-second system for the con-

* Translated from the *Elektrotechnische Zeitschrift*, July 1882. From a separate impression, communicated by the Author.

stants of the galvanic current, as fundamental units, and expressed a wish that a special international commission should be intrusted, in the first place, with the construction of a standard ohm as unit of resistance. Since, then, further consultation is to take place before very long, it seems desirable to consider again the methods hitherto employed, with their sources of error, from the experimental standpoint; their mathematical theory has been sufficiently discussed.

I desire also to give an impulse to further discussion of the methods to be adopted in the new determination. This is to be done the more strictly and thoroughly, since the units, having been once determined, ought not to be altered again immediately in consequence of further investigations.

It is well known that W. Weber, to whom we owe the fundamental facts in this subject, has given four methods of obtaining a conductor of given resistance in electromagnetic units:—

I. A circle of wire of known dimensions is caused to revolve through a certain angle about an axis (vertical) inclined to the direction of the earth's magnetism; and the intensity of the current thus induced is measured by means of a galvanometer of known dimensions. The intensity of the current, under similar conditions, is inversely proportional to the resistance of the conductor.

II. Instead of measuring the dimensions of the galvanometer in the first method, the action of the unit current on the magnetic needle of a multiplier is determined by the damping of its oscillations when the circuit is closed.

III. A magnetic needle is allowed to oscillate within a closed multiplier of known dimensions, and the damping of its oscillations is determined.

IV. A circle of wire is put into uniform rotation about a horizontal or vertical diameter; and we observe the deflection which takes place in a magnetic needle swinging at the centre of the circle, in consequence of the current induced in the revolving circle by the earth's magnetism. Since in carrying out these methods each separate measurement is of necessity attended with error, that method appears at the outset the most reliable which involves the determination of the fewest constants, and in which these determinations can be made with most accuracy. Hence methods III. and IV. appear at the outset to offer special advantages.

We will consider the fourth method first, in order to discuss at once a number of sources of error which partly affect also the other methods. This method is in fact that employed

by the Committee of the British Association appointed for the purpose in 1863.

A wire coil is put into rotation about a vertical axis, in consequence of which currents are induced in it, whose intensity in unit time is, on the one hand, directly proportional to the horizontal component of the earth's magnetism and to the change of the projection of the plane of the coil on the vertical plane at right angles to the direction of that component, and, on the other hand, inversely proportional to the resistance of the coil.

But, then, the induced currents which traverse the coils act upon the neighbouring coils, and induce in them extra currents whose electromotive force is proportional to the change in the directly induced current, in the unit time; hence the inductive action in the spiral is diminished. The deflection of the needle is determined by the total action of all the induced currents. The following investigations are therefore necessary to determine the absolute resistance of the coil.

1. The measurement of the space enclosed by the windings of the coil, to which the induction is, *cæteris paribus*, proportional, as well as its form, upon which the extra currents induced in it depend, and the moment of rotation exerted on the magnetic needle at its centre.

These determinations offer very considerable difficulty.

If thick wire is employed for winding the coil, its diameter, together with the insulating covering, must be exactly an aliquot part of the interior width of the frame on which the wire is to be wound, or else the outer layers of wire will be squeezed more or less between the wires of the inner layers, and so cause displacement. Moreover, as W. Siemens* has shown, the wire is stretched in winding, the more the thinner the wire is; and this extension may be as much as 6 per cent. Again, the insulating covering of the wire becomes pressed together; this takes place less when the covering consists of solid gutta-percha or similar substance than when the wire is covered with silk or cotton. The thinner the wire is, the more important do these errors become, of displacement of the wire, of extension on winding, and of the squeezing together of more of the insulating covering in proportion to the diameter of the wire. It is therefore not correct to calculate the space enclosed by the coils from the length of the wire before winding or after unwinding, and from the dimensions of the coil.

On this account, as W. Siemens rightly remarks, the accurate measurement of the length of the wire of about 1·1 milli-

* Poggendorff's *Annalen*, 1866, vol. cxxvii. p. 327.

metre thickness to the tenth of a millimetre, as in the older experiments of the British Association, was useless.

It is not difficult to determine the internal diameter, in various directions, of the coils of wire (that is, the diameter of the frame on which they are wound), either by means of a kathetometer, or with an inextensible steel tape of constant temperature; but, on the other hand, the measurement of the external diameter or circumference is much more difficult, in consequence of the inequality of the covering of the wire and the unevenness of the surface.

If the error in the determination of the mean diameter only amounted to 0·5 millim., which, in view of the circumstances mentioned, is certainly not an extreme estimate, then in the case of the coil of 314 millim. diameter employed by the Committee of the British Association the estimate of the space enclosed by the coils would be wrong by $\frac{1}{314}=0{\cdot}32$ per cent. In order to reduce this error as much as possible, it is necessary, as both W. Weber * and Lord Rayleigh† recommended, to take the diameter of the coil as great as possible consistently with accuracy of rotation. Also the wire should be wound with a tension as uniform as possible ; and the exterior diameter should be controlled after each layer has been wound.

A much more important source of error lies in the uncertainty of determining the mutual position of the separate coils, depending upon the conditions explained above, upon which the intensity of the extra current induced in the coil when put into rotation depends. Since the inductive action of the coils upon each other takes place at very small distances, a very small error in measuring the distance apart is of great importance.

A further source of inaccuracy is introduced by making the coil of two parts parallel to each other, but with a space between in order to admit the thread by which the magnet is suspended; so that here also the parallelism and distance apart of the two portions must be very exactly determined. That the data may be greatly altered by the extra currents is seen from the fact that, in an experiment of a Committee of the British Association, the position of maximum induction of the coil in rapid rotation was displaced by not less than 20°; and the correction for this amounted to some 8 per cent.

If we seek to determine the self-induction by opposing the coil to another of known coefficient as a Wheatstone's bridge ‡,

* *Berichte der Königlich Sächsischen Gesellschaft der Wissenschaften*, 1880, p. 77.

† Proceedings of the Royal Society, 1881, vol. xxxii. p. 122.

‡ Compare Maxwell's 'Treatise,' vol. ii. p. 357; Brillouin, *Comptes Rendus*, vol. xciii. p. 1010 (1881); *Beiblätter*, vol. vi. p. 39.

then we have, besides the sources of error of the original apparatus, a number of other sources of error which need to be specially examined; so that it is possible the accuracy of the results might be seriously prejudiced. In any case the difficulty of accurately determining the self-induction is the most suspicious part of the method under consideration.

2. The temperature of the coil must be determined with the greatest accuracy, since the conductivity of the wire decreases about 0·3 per cent. for a rise of temperature of 1° C. The corresponding change of length, and consequently of surface embraced, amounts only to $\frac{17}{1000000}$, and may therefore be neglected.

3. We have further to inquire whether there may not be secondary currents induced in the supports of the apparatus, if these are of metal, by the currents circulating in the spiral, which may act upon the magnetic needle*.

This point may be determined by interrupting the continuity of the metallic supports by means of insulating material. According to experiments of this nature, made by Lord Rayleigh and Dr. Schuster†, this source of error was not important in the experiments of the British Association, the error amounting only to 0·16 per cent. It would be better, however, to construct the supports of insulating material.

4. The testing of the instrument to determine if the coils lie symmetrically with reference to the axis of rotation offers no particular difficulty, if we observe by means of a telescope corresponding points of the frame on both sides of the axis in different positions of the coils, making with each other an angle of 180°.

5. In the same way it is easy to ascertain by known optical methods whether the axis of rotation is really vertical, and remains so; deviation from the vertical position may exert a considerable effect.

If the angle of dip were about $\angle = 70°$, an inclination of the axis towards north or south of 0°·2 would cause an alteration in the inductive action of the earth in the proportion of $\cos 70° : \cos (70° \pm 0°{\cdot}2)$—that is, not less than 1 per cent. An inclination of this amount in the case of the British-Association coil would correspond to a displacement of 0·5 millim. in the ends of the axis. An exact determination is therefore very necessary. Small displacements of the axis towards east or west have only an insignificant influence.

6. The counting of the number of revolutions of the coil in

* Compare F. Kohlrausch, in Pogg. *Ann.* 1874, Ergänzungsband vi. p. 9.

† *Loc. cit.*

unit time by known methods offers no special difficulty; nor does the maintaining of a constant velocity of rotation by mechanical means*.

7. The adjustment of the magnet in the centre of the rotating coil should also not be difficult to effect. Moreover a small deviation from exact adjustment causes no important error.

8. The moment of the magnet may be determined by vibration-experiments after determining its moment of inertia and the horizonal component of the earth's magnetism; or it may be determined from experiments on deflection. If, in order to render the inductive action of the magnet on the rotating coil imperceptible, we employ magnets of very small moment, then these methods offer many sources of error, on account of the very perceptible influence of the friction of the air in the vibration-experiments, or of the small distance at which the deflecting magnet must be placed.

If the moment of the magnet is very small, it and the distribution of magnetism in the magnet (which is very difficult to determine) have both very small effect upon the results. If the poles of the magnet are at a distance from the central point in the median plane of the coils which is less than $\frac{1}{6}$ of their radius, and if the distance of the poles from the plane is not greater than 0·84 of the length of the magnet, then the force exerted upon it by a current in the coil is constant to within 0·0005, under conditions otherwise similar, up to a deflection of 56°.

9. The adjustment of the telescope and scale required for reading-off the position of the magnet, and the correction of the readings, may be made in the usual way; and the divisions of the scale must be compared with an accurate measure. The accurate measurement of the distance of the scale from the central point about which rotation of the magnet takes place, or from the reflecting surface of the mirror, offers a certain amount of difficulty.

10. The force of torsion of the suspension-thread of the magnet may easily be compared with the directive force of the magnet by turning the thread fastened to a torsion-circle through a certain angle, and observing the deviation of the magnet. Unavoidable changes of considerable magnitude may result from the variable moisture of the air, if weak magnets are employed.

11. We have, further, to inquire what influence currents of air and the vibration of the apparatus caused by the motion

* Compare the ingenious arrangement adopted by Lord Rayleigh, *loc. cit.*

exert upon the needle when the coil is put into rotation with circuit open. In the first experiments of the Committee of the British Association, in which attention had not yet been paid to the separate conditions of the experiment in the way which will be necessary for a final determination of the ohm, these last-mentioned sources of error made themselves in a high degree perceptible.

Thus F. Kohlrausch, in a discussion of these experiments, has justly pointed out that, in order to avoid inductive action on the coil of wire, the magnet (a magnetized steel ball of 8 millim. diameter), in spite of its great mass, had only a moment equal to that possessed by an extremely fine magnetic needle weighing 0·025 gramme. The magnet was attached by means of a wire 0·25 metre long to a mirror of 30 millim. diameter suspended by a simple silk fibre of 2 metres length. The currents of air produced by the rotating coil only 0·31 metre distant acting upon the relatively large surface of the mirror and magnet, as well as the variable torsion of the thread, become much too considerable in comparison with the directive force of the magnet.

Further, the vibration of the apparatus produced by the rotation may propagate itself to the case surrounding the mirror, and so put the air, and with it the mirror, into rotation. It might thus happen that the deflections obtained by rotating the coil in one or the other direction might vary by as much as 8·5 per cent. If the mean results obtained in different series of observations should differ amongst each other by only 2·3 per cent., yet even this is not a guarantee of greater accuracy, but can only be regarded as a proof *that the apparatus always acts in nearly the same way.*

Moreover the supplementary elimination of sources of error, *e. g.* by more exactly calculating the effect of self-induction, as attempted in the memoir of Lord Rayleigh and Dr. Schuster, can in no way free the results from the influence of sources of error shown to exist by the deviations cited above. Above all things, observations of this kind ought never to be arbitrarily corrected on the ground of probability only without having perfectly definite numerical data; or else all secure experimental ground is lost.

Hence we may consider it shown that the results of these experiments are not to be themselves taken as a final determination of the ohm, but rather as extremely valuable preliminary experiments by which we have become acquainted with the precautions to be observed.

By means of new experiments made by Lord Rayleigh and Dr. Schuster with the apparatus of the British Association,

altered in some points, the difficulties mentioned above under (1) remained unaltered; the magnet, however, was replaced by four magnetized needles, each 0·5 centim. long, fastened on the parallel horizontal edges of a cube of cork. The mirror was attached directly to the cork. But here also the directive force of the magnet is small in comparison with the somewhat powerful influence of currents of air acting on the large surface and small moment of inertia.

Further, the case protecting the suspended portions of the apparatus is attached to the glass tube which surrounds the suspension-thread. Perfect stability could hardly be obtained with such an arrangement; since with rapid rotation vibration of the mirror would certainly be experienced. These experiments also are to be regarded more as preparatory for later and final measurements, and as such have been excellently carried out. On this account no doubt the more exact data for vertical adjustment of the axis &c. have not yet been given throughout. Lord Rayleigh, as already mentioned, lays emphasis on the necessity for new coils for the final experiments, and raises the question whether these should not be arranged as in the Helmholtz-Gaugain galvanometer, so that the directive action of the current on the needle is independent of any small excentricity or deviation of the needle.

In the final experiments, whether they are made according to this method or according to one of the other methods, it is in any case necessary that as complete a statement should be given of each separate proceeding in arranging and using the apparatus. Also the experiments should not be made with an apparatus set up and adjusted once for all, since then the errors of the arrangement will repeat themselves with each new determination undertaken with the apparatus. On the contrary, the apparatus itself must be frequently altered in various ways. Only so can we obtain results independent of each other, which can be used for mutual control.

The sources of error which are so difficult to avoid in using method IV., more particularly in consequence of the extra currents, have induced W. Weber* himself, in conjunction with the late F. Zöllner, to again take up the first method (which had been employed by the first-mentioned so long ago as 1846 in preliminary experiments) with the most perfect experimental means. Of two equal coils, weighing some 207 kilog., most carefully and very regularly wound upon mahogany rings, as shown by measurement of different diameters, the

* W. Weber and F. Zöllner, *loc. cit.*

coils consisting of cotton-covered copper wire about $3\frac{1}{3}$ millim. in thickness, and having a resistance of about 5 ohms—one, the multiplier, surrounds a cylindrical magnetic needle about 100 millim. long and 10 millim. thick, fixed in the magnetic east and west plane. The diameter of the coils (interior about 960 millim., exterior 1040 millim., and breadth of coils about 254 millim.) is so large that the magnetic forces acting on the needle may be supposed, without perceptible error, to be concentrated at their centre. The other coil, connected with the first by conducting-wires, the inductor, is capable of rotation about a vertical axis, out of the east and west position through an angle of 180°. This displacement of the inductor takes place suddenly at such times that the currents induced in it and transmitted through the multiplier render the deflections of the needle constant according to the "method of multiplication" or the "method of recoil."

In carrying out the experiments, the displacement of the inductor by means of clockwork was not found to be practicable, and it was therefore effected by hand. The time required for the displacement (some 2 seconds) must only be a small fraction of the time of vibration of the magnet hanging in the multiplier. For this purpose the time of vibration of the needle is increased (up to 30 seconds) by placing the needle in a stirrup to which a tube of brass 272 millim. long was fastened in a horizontal position and at right angles to the needle, the ends of which carried plane mirrors. Telescopes with parallel scales are placed before both mirrors, each at a distance of 4 millim. By this double reading the difficult measurement of the scales from the mirrors is replaced by the easier measurement of the distance of the scales from each other, and of the distance of the mirrors apart. An examination of the point whether, and how far, the instants of displacement of the inducing coil may differ from those indicated by calculation, and how far these possible deviations may affect, might be easily made*. In any case, according to the experiments which have been made, this influence cannot be of importance, since the results obtained at different times by use of a magnet 100 millim. long agree with each other to within 0·06 per cent., and those obtained with a magnet 200 millim. long do not differ at all from the mean result. This shows at the same time that the instrument had not altered in any important respect in the interval. The inductor must be so far removed from the multiplier that the currents in the first cannot directly deflect the needle.

* Compare Chwolson, *Bulletin de St. Pétersbourg*, vol. ii. p. 403; *Beiblätter*, vol. v. p. 450.

It is a point of very great importance in the employment of this method that self-induction in the inductor is without influence, and that, further, the intensity of the earth's magnetism does not enter into the calculation, provided that it is of equal intensity in the positions occupied by inductor and multiplier. It may not be allowable to assume that this is the case in ordinary rooms, because of iron hooks let into the walls; but in a building constructed for the purpose this condition might easily be fulfilled. This point may also easily be controlled by oscillation-experiments; the ratio of the directive force of the earth may be determined at the two places.

It is true that we have to set against this advantage that we have two coils to measure, and are so liable to a double error. (This, however, is also the case in the experiments of the British Association described above, where the revolving coil consists of two separated halves.) This disadvantage may be considered small in comparison with the error resulting in the fourth method from the measurement of the self-induction of the coil. It is further reduced by the large dimensions of the coils. At the same time stability of the apparatus, and consequent freedom from disturbances resulting from vibration and from currents of air, is much more easily obtained than with the rapidly-rotating coil employed in the former method.

Previous communications on experiments with this apparatus are only preliminary, on which account we are not yet in possession of details of adjustment; for which, however, not only the name of W. Weber, but also that of Repsold, the instrument-maker intrusted with the work, give us full guarantee that such points as the verticality of the axis of rotation, the adjustment of the coil magnetic east and west, the arrangement for displacing the coil through exactly 180°, and so on, will be attended to. The preliminary method of measuring the circumference of the mahogany rings on which the coils are wound and of their exterior circumference by means of strips of paper, which were then compared with a divided wooden rule, will no doubt give place to more exact methods in the actual experiments.

A special advantage of this method is that the wires may be unwound from the rings and wound again without any great difficulty; and so repeated observations independent of each other can be obtained.

This method is therefore to be recommended for the final determinations.

If we employ Weber's second method, we have no need to determine the dimensions of the multiplier; but we determine

the damping of the oscillations of the needle, once with open circuit and again with closed circuit. Hence, by determining the deviation of the needle when the inductor is displaced, the resistance of the circuit in electromagnetic measure may be calculated. For this purpose the multiplier must enclose the needle more closely, so that the damping may be sufficient. We have to determine, besides the dimensions of the inductor, the period of oscillation and moment of inertia of the needle and the intensity of the horizontal component of the earth's magnetism. The sources of error of this method have been carefully examined by F. Kohlrausch*; the error in measurement of the inductor may, however, have been scarcely estimated high enough. (Compare also the following method.)

If proper experimental means and a suitable observatory are available† for accurate determination of the earth's magnetism, this method may very well be employed together with Weber's first method.

In Weber's third method, which is apparently so simple, we have, in order to determine the absolute resistance of a multiplier, to observe only the damping of the oscillations of a needle suspended in the coil when the ends of the coil are united, and again when the circuit is open. In order to be able to calculate the electromotive force due to the oscillations of the needle and its action upon the needle, which is directly proportional to that electromotive force, and inversely proportional to the resistance to be measured, we must know in general the dimensions of the multiplier and the position of the needle relative to it, as well as the distribution of the magnetism in the needle.

The former determinations are difficult to carry out exactly, since, in order that there may be sufficient damping, no very large dimensions can be given to the coil; the latter determinations can only be made inexactly by observation of the currents induced in a short coil at different points of the magnet, or by numerous oscillation- and deflection-experiments with a magnetic needle swinging at different distances from various points of the magnet. We have also to satisfy ourselves that the damping is not dependent upon the angle of deflection of the needle.

This method has been employed by Fr. Weber ‡, in Zürich,

* *Loc. cit.*

† Compare also the new methods by F. Kohlrausch, *Göttinger Nachrichten*, March 4, 1882.

‡ F. Weber, *Elektromagnetische und kalorimetrische Messungen* (Zürcher und Furrer, Zürich, 1878); *Beiblätter*, vol. ii. p. 499.

employing a magnet of 80 millim. length, 20·1 millim. breadth, and 21·1 millim. depth, suspended by a silk thread between two conical coils very regularly wound, of 144·43 millim. internal, 184·46 millim. external radius, and 51·64 millim. breadth.

The mean distance of the spirals from each other was either very small, or the distance of their median planes was made 164·4 millim., or very nearly equal to the mean radius of the coils. In this last case the distance of the poles from each other has no important influence, although it has in the first, where also the distribution of moments in the magnet must not be neglected. We must, however, always take into account inevitable differences in measurement in consequence of the mutual pressure of the coils and certain unavoidable irregularity in the winding, which are more important in consequence of the small dimensions of the coils, as in the fourth method which we have already considered. Nevertheless it was shown, by comparison with a standard resistance of Siemens's, with which the resistance of the coils was compared by means of a Wheatstone's bridge, in both the cases mentioned, and after rewinding of the coils, that the standard resistance expressed in absolute measure varied in three series of experiments only from 0·9532 to 0·9570, from 0·9528 to 0·9555, and from 0·9527 to 0·9551—that is, not more than 0·5 per cent. The mean is 0·95451.

In further experiments according to another method, Fr. Weber placed the coils mentioned above at a definite distance from each other, connected the one, "the inducing coil," with a very constant Daniell's cell and a simple ring of 168·7 millim. radius; the other, "the induced coil," with a multiplier, which was composed of a coil consisting of two equal conical halves placed close together, of 154·2 millim. inner radius and 172·2 external radius, between which was suspended a magnet 40 millim. long provided with a mirror. The simple ring of the inducing circuit lay between these coils. The deviation of the needle whilst a constant current passed through the inducing circle measured its intensity J, which was to be measured in absolute measure from the known dimensions of the ring; the deviation on breaking the inducing circuit after disconnecting the ring gave the intensity i of the induced current. Since the induced electromotive force $e=JP$ (where P is the mutual potential of the coils) may be calculated in absolute measure from the dimensions of the coil by putting the constant of induction equal to unity, we obtain the resistance of the induced circuit r from the formula $r=\frac{e}{i}$.

We have therefore for these determinations to ascertain the dimensions (1) of the wire ring, (2) of the inducing coil, (3) of the induced coil, (4) of the multiplier, (5) of the distance of the separate layers of the inducing and induced coils from each other, (6) the determination of the position of the magnetic poles, to which are to be added the manipulations required for the other methods, the adjustment of the needle in the centre, and of the coils of the multiplier and of the wire ring between them with reference to the meridian &c. Since the intensities of the primary and secondary currents are measured by the deflection of the same magnet, the horizontal component of the earth's magnetism does not enter into the calculation. There are in this method many more sources of error to be taken into account than in the other methods, by which its accuracy may be seriously impaired.

Notwithstanding, Weber finds, by determining the resistance of the inductive circuit by this method, and comparing it with the Siemens standard which he employed, that at two different distances of the inducing and induced coils, and with two different intensities of the inducing current, a mean result of 0·9554 (0·9589 to 0·9516)*.

This method has also been employed by Rowland†, only that he employed a tangent-galvanometer to determine the intensity of the inducing current, but a special galvanometer to determine the induced current; it was necessary also to determine the ratio of the intensities of horizontal force at the position of the two pieces of apparatus. His three induction-coils had a diameter of about 27·4 centim., and were wound on frames having a thick brass rim on each side. They could be combined in pairs, so that the one which served as inductor, through which a constant current flowed which could be interrupted, might be placed at four different distances from the others (6·5 up to 11·47 millim.). Very thin-covered copper wire (no. 22) was taken for the coils; so that the sources of error mentioned on p. 260, caused by displacement of the covering, the mutual compression of the coils, and expansion of the wire, become very prominent. Since the in-

* An indirect method employed by Fr. Weber to determine the resistance of a zigzag platinum wire placed in a calorimeter, from the heat produced by a current whose strength in absolute measure was determined by means of a tangent-galvanometer of known dimensions, can hardly be placed by the side of the more direct methods, on account of the difficulty of all calorimetric measurements. Nevertheless Fr. Weber finds by this method, by comparison with his Siemens standard, a resistance for the latter of 0·9560, which again differs only 0·1 per cent. from the other determinations.

† Rowland, 'Silliman's Journal,' 1878 (3), pp. 325, 430.

duction-coils are so close together, a small error in measuring their distance apart is of great influence—the more so since, from what has been said, it follows that the position of the separate coils could not be accurately controlled. That the distance of the coils apart at various points was measured to the $\frac{1}{20}$ millim., and that the mean distance apart is given to $\frac{1}{1000}$ millim., and the mean radius to the $\frac{1}{10000}$ millim., can hardly be regarded, in view of the above-mentioned disturbing causes, as a guarantee of the actual exactitude of the results.

Since the length of the needle of the tangent-galvanometer (2·7 centim.) amounted to only $\frac{1}{18}$ of the diameter of the coil (50 centim.), the deviation from the law of tangents is imperceptible ; but, on the other hand, there is an important source of error in the fact that the needle turned upon a point. Rowland asserts that the needle, which was provided with a pointer playing over a circle of 20 centim. diameter, always took up its position accurately to 1 or 2 minutes (1 minute corresponds to only 0·03 millim. upon the circle). Since this does not at all agree with previous experience, an exact explanation how so great sensitiveness was obtained is indispensably necessary.

The needle of the galvanometer was 1·25 centim. long ; the coils, which were brought up to it from the east and west, had an internal diameter of 3 centim. and an outer diameter of 5·6 centim. ; their inner end-surfaces were at a distance of 0·935565 centim. (thus given to $\frac{1}{100000}$ millim.) from the point on which the needle turned. Whether the law of tangents is altogether applicable here must remain uncertain.

Since the intensity of the inducing and induced currents is measured at different places, Rowland introduces a further complication by determining the ratio of the horizontal component of the earth's magnetism at the position of the tangent-circle and of the galvanometer by surrounding the galvanometer with a larger coil, sending the same current through it and the tangent-circle, and comparing the deflections. The radius of the new circle (some 4 centim.) is again given to $\frac{1}{10000}$ millim.

The values of the different dimensions given to so many decimal places are probably not regarded by the author himself as accurate in the same degree, the last decimals (obtained by interpolation) being added for the sake of completeness. It is, however, always advisable to give exact account how far actual observation goes, since the mean values can never be accurate beyond the limit of possible observation, or else with the roughest measures it would be possible by repeating measurements to obtain any required degree of minuteness.

Since, moreover, the sources of error mentioned above may undoubtedly cause considerable deviations, such a one-sided accuracy cannot be a guarantee for the exactitude of the final results *.

On account of the much larger number of determinations which this method requires, I consider it much less suitable for the final construction of a definite resistance than the fourth and first of Weber's methods, of which again I give the preference to the latter.

After a conductor A of known absolute resistance has been constructed, by the one method or the other, it has to be compared with the resistance B of a mercury column of known dimensions in order to determine the length of such a column of 1 square millimetre section which possesses 1 ohm resistance.

If we employ Weber's first method, we may introduce the mercury column B directly between the inductor and multiplier, and calculate the resistance of B from the decrease of intensity; otherwise the resistances A and B may be compared by means of the differential galvanometer, or more conveniently by means of the Wheatstone's bridge, according to the accurate method already employed by W. Siemens in constructing copies of his mercury unit. Since we already know the length of mercury corresponding to 1 ohm very nearly, we take care to arrange the resistance B so as to be as nearly as possible equal to the resistance A.

It is highly important, after calibration of the measuring-wire, to secure perfect contacts by using freshly-amalgamated thick copper connecting-pieces and cups full of pure mercury, the resistance of which is previously determined by including several in a circuit. Plug-contacts are not sufficiently accurate. Also the temperature must be carefully kept constant throughout at the right point, as was the case in the construction of Siemens's copies.

* If we employ in this and other methods a coil wound uniformly on a closed ring, a neutral solenoid, as inductor, but as induced coil a coil surrounding the former at one point, the conditions are so far simpler that the current due to opening or closing the current in the solenoid, only the linear dimensions of the solenoid, its interior and exterior diameters d and d_1, the number of coils of the induction-coil and of the solenoid, or in the case of a solenoid of rectangular section the height a, are concerned (compare Roiti, *Atti di Torino*, vol. xvii., 30th April 1882). Nevertheless the employment of such a solenoid is attended with the great difficulty of winding the wire exactly upon it, and of measuring its dimensions with sufficient accuracy. The latter is the more difficult, since the section of the solenoid can only be taken relatively small, and the formula contains the expressions $\sqrt{d} - \sqrt{d_1}$ for a solenoid with circular section, $a \log \frac{d_1}{d}$ for a solenoid with rectangular section.

A difficulty presents itself here, since induced currents are produced in A on opening and closing the current which flows through the combination of wires. We must therefore either employ a constant current of such feeble intensity that the heating of the circuit shall be imperceptible, or, according to the method of Kohlrausch*, who has also worked out the necessary calculations, we must employ an induction-apparatus which sends alternating currents through the circuit. A special source of error is found in making contact with the mercury column contained in an accurately-calibrated carefully cleaned glass tube. The ends of the tube terminate in glass vessels of diameter relatively large. The apparatus is best filled with mercury by pouring mercury into one of the vessels and inclining the apparatus towards the same side, then, after closing the openings, repeatedly exhausting the apparatus as completely as possible, gently warming the tube, then allowing pure air which has passed over phosphoric anhydride and wadding to enter, and finally, after exhausting again, allowing the mercury to flow into the tube by placing it in a horizontal position. If the electrodes are immersed in the vessels containing mercury, then the resistance of the mercury in the tube is increased by that of the mercury contained in the vessels up to the ends of the electrodes. This resistance may be calculated on the assumption that the vessels are infinitely large. It might perhaps be advantageous to determine it directly by compensating the resistance of a tube filled with mercury with two contact-vessels by a nearly equal resistance on Wheatstone's bridge, and then to cut the tube at one or more points and to introduce wider mercury vessels of suitable form, and then again determine the resistance. We might also introduce two tubes, of lengths m and n and of equal section, between the same contact-vessels, and compare their resistances, and from the data so obtained calculate the resistance of the vessels.

In order to avoid this separate comparison of the resistance of the coil determined in absolute measure with a mercury column, Carey Foster † and Lippmann ‡ have almost simultaneously suggested (L.) or carried out (C. F.) the ingenious plan of employing Weber's fourth method, as adopted by the British Association, and Poggendorff's compensation-method

* Poggendorff's *Annalen*, 1871, vol. cxlii. p. 418.

† Carey Foster, 'Electrician,' 1881, vol. vii. p. 266; *Beiblätter*, vol. vi. p. 133.

‡ Lippmann, *Comptes Rendus*, 1881, vol. xciii. p. 713; *Beiblätter*, vol. vi. p. 43.

for determining electromotive force without having previously determined the resistance of the rotating coil.

Let the resistance R to be investigated and a tangent compass be included in the circuit of a constant pile, *e. g.* a thermopile. Let the ends of the resistance be connected also by a second circuit, into which a delicate galvanometer and the coil rotating about a vertical axis are introduced during a particular phase of its motion by means of a commutator revolving with the coil. The velocity of rotation is so regulated, or the resistance in the branch containing the constant pile is so altered, while the velocity of rotation of the coil remains constant, that the galvanometer is not affected. Then, at the moment that the coil is introduced, the induced electromotive force in it is equal to the difference of potential at the ends of the conductor R caused by the thermopile. If the current-intensity in it, and also in the circuit of the thermopile and of the tangent-galvanometer, be equal to J, then $E = J R$. The factor of reduction of the tangent-galvanometer is to be determined from its dimensions and the horizontal component of the earth's magnetism at the place, and in the same way the electromotive force induced by the rotation of the inductor. In most cases we may assume the horizontal component of the earth's magnetism to be the same at both places, as in Weber's first method, which thus disappears from the calculation. In this method, therefore, we have the same determinations to make as in Weber's fourth method. The uncertainty of contact in the commutator is without influence, since when the galvanometer is not affected there is no current in the inductor-circuit. If we adjust the commutator so that it makes contact with the coil at the time of maximum induction, just at the moment when it varies least, then, if the ratios are in other respects properly chosen, the influence of self-induction in the rotating coil is reduced to a minimum. A certain amount of care, however, is required in order to avoid thermo-electric disturbances, resulting from the heating of the points of contact; and the determination of the exact phase during which the commutator makes contact offers a certain amount of difficulty.

The results so far obtained by the different methods are, some of them, tolerably far apart, although the investigations have been carried out with great care, if not in all points with the most perfect experimental means which a final determination of the ohm demands. Thus the resistance of a mercury column 1 metre long of 1 square millim. section at 0° C. was found by the first experiments of the British Association to

be 0·9830; according to F. Kohlrausch it is 0·9717; according to F. Weber 0·9550; and according to Lord Rayleigh and Mrs. Sidgwick 0·9413 $\left(10^9 \frac{\text{centim.}}{\text{sec.}}\right)$. The first ohm constructed by the British Association, according to F. Kohlrausch is 1·0196; according to Rowland 0·9910, and according to Rayleigh and Schuster 0·9893 of the true ohm $\left(10^9 \frac{\text{centim.}}{\text{sec.}}\right)$.

Hence at any rate it is indicated that the final determination of the ohm must not rest alone on experiments made only according to one method and carried out at one place. Further, the results of each separate method (as I have already mentioned) offer security against possible constant errors only if they are obtained from entirely independent series of experiments, made with apparatus varied in all possible ways. Since investigations are already in progress in different places, with excellent apparatus and according to different methods, we may shortly expect to be in a position to compare together the data which they yield, and so to attain as reliable a final result as possible.

In such important and permanent determinations as those of the electrical units, a delay of a few months is of no importance whatever in comparison with the reliability of the result to be obtained. Any introduction of the ohm as given by detached series of observations and distribution of copies for practical use would therefore be premature and without authority.

The Commission appointed for the determination of the electrical units does not complete its task by simply determining the ohm. It is further necessary to construct at least one of the two remaining units, *e. g.* the volt. Since it is at present impossible to construct this in a form capable of reproduction, it will be necessary first to compare electromotive forces with those of a constant element whose electromotive force is known in volts. We are therefore in the same case as if, wishing to measure a length in metres, we were obliged to employ a yard measure whose ratio to the metre was known. So much the more thoroughly are we obliged to investigate the electromotive forces of the galvanic cells to be employed as intermediate measures, and their dependence upon external conditions. The cell invented by Latimer Clark has indeed been shown to be capable of reconstruction of exactly the same electromotive force; at the same time it can only be employed for electrostatic measurements, since it becomes polarized when used to produce a current. Obser-

vations with reference to Daniell's and other cells do not appear to me to lead to any certain conclusion.

Similar indirect methods have to be employed for measuring the intensity of a current in absolute measure; the factor of reduction of the tangent-galvanometer employed, for example, has to be determined. This problem has been considerably facilitated by the recent investigations of F. Kohlrausch and Mascart on the electrochemical equivalent of silver, which, however, exhibit small deviations among themselves. There is here consequently a rich field for accurate investigation, for which we have already a series of valuable preliminary investigations. All these labours, however, can only give a final result sufficient for our present purpose when they are executed upon some common and well organized plan, and are carried out with the most perfect experimental means.

XXXI. *Crystallographic Notes.* *By* L. Fletcher, *M.A., of the Mineral Department, British Museum; late Fellow of University College, Oxford**.

[Plate VI.]

X. *On Twins of Copper Pyrites.*

THE memoir of Haidinger† on the Crystallisations of Copper Pyrites, published so long ago as the year 1822, was so exhaustive and withal so simple in its character that little seemed to be left to tempt the crystallographer to devote further study to this mineral; and in fact, with the exception of the papers of Sadebeck, whose early death all interested in the progress of mineralogy must so deeply deplore, and the confirmatory data in Groth's Catalogue of the Collection of the University of Strassburg, we have still no other information at our disposal. A study of these memoirs and of the various text-books of mineralogy leaves upon one's mind such a feeling of doubt as to the true statement of one of the laws of twin-growth, and that (as will be explained later) a law almost, if not quite, unique in the domain of crystallography, that, at the suggestion of Professor Maskelyne, the collection of copper pyrites in this Museum has been examined with a view to a possible settlement of the difficulty.

To get a clear idea of the present position, it is necessary to trace the history of this particular law from its first statement down to the present time.

In the above memoir of 1822, the twin-growths of copper

* Read before the Crystallological Society, June 3, 1882.

† Memoirs of the Wernerian Society, vol. iv. p. 1. 1822.

pyrites were assigned by Haidinger to three distinct laws. In the first kind, the twin-plane, or plane of rotation, is a face of the octahedron {1 1 1}, and the composition-plane, or plane of junction, is generally parallel, but sometimes perpendicular, to the twin-plane: these growths correspond to the blende twins of the Cubic system. In the second, the twin-plane is a face of the octahedron {1 0 1}, and the composition-plane is *perpendicular* to the twin-plane. In the third, the twin-plane is a face of the prism {1 1 0}, and the individuals are interpenetrant. The second law is that with respect to which uncertainty has arisen; and it is with this law that we have now to deal.

In the original memoir of 1822 the growth is so clearly described, and the law so distinctly expressed, that it is impossible to read the memoir and to mistake its meaning. Three years later, however, Haidinger published in the 'Edinburgh Journal of Science' a series of papers "On the Regular Composition of Crystallised Bodies," copiously illustrated with figures which have since found their way into almost every manual of the science; and in its natural place in the tetragonal (or pyramidal) system he describes once more, though briefly, the particular growth of copper pyrites which we are about to consider. There is little doubt that this secondary description has been the cause of serious misunderstanding.

On page 68 of vol. iii. (1825) we read as follows:—"Regular composition often also takes place in this species parallel to a plane of P−1{1 0 1}, or perpendicular to the terminal edges of P {1 1 1}. There are particularly two varieties of *this case* which in the present place deserve our attention. The individuals are either joined in pairs, or one central individual is surrounded by four others, added in the direction of all the edges of P. The product of the first, in the fundamental pyramid, would be fig. 30 [similar to fig. 6]. This has not yet been observed; but it will serve for explaining fig. 31, a variety of the form P−∞{0 0 1}, P−2{1 1 2}, P{1 1 1}, $\frac{3}{2\sqrt{2}}$P{3 0 2}, and $\frac{3}{2\sqrt{2}}$P+1{3 3 2}; from the mines of the district of Siegen in Prussia. This and several other interesting varieties of forms from the same locality I have described on another occasion (*Mem. Wern. Soc.* vol. iv. part 1, p. 1, 1822), from specimens in the possession of Mr. Sack, of Bonn."

Taken by itself, this explanation might at first sight suggest the interpretation which seems to have been placed upon it by some crystallographers—namely, that there are twin-growths of copper pyrites in which the *plane of composition* is parallel to a plane of the octahedron {1 0 1}.

This interpretation can never have been intended by Haidinger. At the beginning of the series he had remarked that, for the precise definition of a twin-growth, two planes must be given:—first, the twin-plane, or plane of rotation, to indicate the relative *directions* of corresponding faces of the two individuals; and, secondly, the composition-plane, or plane of junction, to indicate their relative *positions*. Haidinger's initial sentence, "regular composition takes place parallel to a plane of {1 0 1}, or perpendicular to the terminal edges of {1 1 1}," may therefore refer either to the plane of twinning or to that of composition, and to this extent is indefinite. But seeing that no plane of {1 0 1} is perpendicular to a terminal edge of {1 1 1}, the above sentence must indicate two distinct cases if the reference be to a composition-plane, while if the reference be to a twin-plane, only a single case is indicated; for, as will be shown later, a rotation through two right angles in a plane of the octahedron {1 0 1} is crystallographically identical in its results with a rotation through two right angles perpendicular to (or round) a terminal edge of the octahedron {1 1 1}. That Haidinger only recognizes a single case is evident from the italics in the next line of the above quotation. As a matter of fact, the figure of the twinned octahedron, repeated from his first paper, shows the plane of composition as perpendicular to the twin plane, though this is only evident after careful inspection. His final reference to the original memoir without calling attention to any deviation therefrom, shows that he was still in accord with the explanation there given.

From this we conclude that either a careful study of this later paper of Haidinger on the regular composition of crystallised bodies, or a simple reference to his original memoir on the crystallisations of copper pyrites, would have made clear the fact that Haidinger regarded the composition-plane as perpendicular to the twin-plane (1 0 1), though we grant that at first glance his second paper might suggest that the composition-plane is, in some cases at least, parallel to the plane of twinning (1 0 1).

In 1830, Naumann's well-known work* did in fact state the law differently from Haidinger, and assumed the plane of composition to be *parallel* to that of rotation. In his preface Naumann makes special mention of the help he had derived from Haidinger's series of papers on regular composition, and in his description of these particular growths adopts some of the figures there given. It seems quite impossible for this deviation from Haidinger to have been introduced wittingly;

* *Lehrbuch der reinen und angewandten Krystallographie*, 1830.

for attention is not directed to the difference in the explanations, and, further, it is unlikely that Naumann had been able, so soon after the publication of Haidinger's work, to undertake a special and minute study of this particular law.

We feel that the above reasoning shows clearly enough that Naumann's statement is a simple misinterpretation of that of Haidinger, and that this can only have resulted from the fact that Naumann referred to the secondary, and not to the original, explanation given by the author of the law.

As Naumann's 'Crystallography' became the recognized and universal text-book on the subject, this statement, though a mistaken one, has been extensively circulated, and appears probably in every text-book of the present day, though, as might have been expected, the explanation of 1822 is repeated in Haidinger's own manual of 1845.

In 1868* Sadebeck published the results of his study of specimens of copper pyrites (belonging chiefly to the Berlin collection), and in the explanation of the twins assumed the correctness of Naumann's statement of the law. In a second paper, published in the following year†, he gives an explanation of his position, so very brief and so clearly illustrative of the present difficulties that a translation is given here:—

"In my memoir on copper pyrites I have wrongly stated the law of twinning; for I have supposed the twin-plane to be also the composition-plane. According to this, one pair of tetrahedron-faces should meet in a salient angle of 1° 24′, and the opposite pair in a reentrant angle of the same magnitude. After I had published the memoir, Haidinger informed me by letter that this was not the explanation he himself had given, as may be seen from his statement in the 'Edinburgh Journal of Science,' which runs thus:—'Composition takes place perpendicular to the terminal edges of P.' In consequence of this friendly private communication from so famed a Nestor of the science, I subjected the crystals again to a careful study. The result was that I found it impossible to say whether the tetrahedron-faces actually coincided, or formed an angle of 1° 24′. This led me to retain my old view, since that law seemed to me a simpler one which regarded a plane of the form {1 0 1} as at once twin-plane and composition-plane. But if I now apply the general law for tetrahedral twins to this case, it follows that, as faces of the positive tetrahedron of the one individual are adjacent to faces of the

* "Ueber die Krystallformen des Kupferkieses," *Zeit. d. deutsch. geolog. Gesellsch.* p. 605, vol. xx. 1868.

† "Allgemeines Gesetz für tetraëdrische Zwillingsbildung," *Zeit. d. deutsch. geolog. Gesellsch.* p. 642, vol. xxi. 1869.

positive tetrahedron of the other, the composition-plane is perpendicular to the twin-plane. The tetrahedron-faces therefore must really fall into a plane; and I hope that crystals may yet be found which will leave the matter beyond doubt."

From this we conclude:—first, that Sadebeck had followed Naumann, assuming his explanation to be that of Haidinger; secondly, that, owing to the practical difficulty of distinguishing between growths according to the two laws, he could come to no decision from simple examination of the specimens; and, thirdly, that he declared in favour of Haidinger's view merely that the law might not be at variance with a second law, which was true in certain cases, but of which the general application had not been proved.

Apparently before 1876 there was another change of view on the part of Sadebeck; for on page 82 of Rose and Sadebeck's 'Crystallography'*, where this law is briefly referred to, we read that "the individuals have a face of the form {1 0 1} for composition-plane;" and a footnote gives a reference to the first paper of Sadebeck, without stating whether or not he had since obtained that evidence of the incorrectness of Haidinger's explanation which was confessedly wanting so late as the time of publication of the second. The omission of any reference to the difficulty may have arisen from unwillingness to perplex the student of an elementary text-book.

The next mention of the law is made in Groth's Catalogue of the Strassburg Collection (1878). We there find that, "as for the regular growths of copper pyrites, the results of Sadebeck are quite confirmed by the specimens in the Strassburg collection;" and further on we read that these particular growths are symmetric twins about a plane of the form {1 0 1}. In other words, Sadebeck's *first* explanation, or that of Naumann, is accepted. There is no reference to the difficulty in which Sadebeck had found himself placed; indeed it is quite possible that the later explanation of Sadebeck, agreeing with that of Haidinger, had escaped notice owing to its having appeared in a paper dealing with a more general subject and having no reference to copper pyrites in its title: in any case no measurements are recorded which render it possible to distinguish between the two statements of the law.

In the latter part of the same year, according to a paper on haplohedral hemihedry†, either Sadebeck was in a state of doubt as to which is the correct explanation, or else he considered both correct; for we read as follows:—"If, on the

* Rose and Sadebeck's *Elemente der Krystallographie*, 1876.

† "Ueber geneigtflächige Hemiëdrie," *Zeit. d. deutsch. geolog. Gesellsch.* p. 601, vol. xxx. 1878.

one hand, a face of {1 0 1} be both twin-plane and composition-plane, the adjacent tetrahedron-faces form small salient or re-entrant angles; if, on the other hand, the composition-plane be perpendicular to the twin-plane, the tetrahedron-faces of the one are coincident with the adjacent faces of the other." The probability is that both are mentioned, not because Sadebeck believed them to be both true, but merely to show that in either case the position then being contended for was a tenable one; and in fact the position of the composition-plane has no further bearing on the argument of that paper.

It would at first sight appear that a difference of a right angle in the position of the plane of composition would manifest itself by angular differences in the twin sufficient to render any difficulty of distinction impossible; we shall therefore attempt to make quite clear what differences would be observed in growths characterized by such different laws.

Fig. 2 represents an octahedron {1 1 1} of copper pyrites in equipoise, $a\ b\ c\ d$ being one set of similar alternate faces, and $\alpha\ \beta\ \gamma\ \delta$ the other set respectively parallel to the first. This figure approaches very nearly to the regular octahedron of geometry and of the Cubic system, of which the faces are all equilateral triangles and the sections through the edges all squares: the difference therefrom was first made known by Haidinger in the memoir of 1822. Though $A\,B\,\bar{A}\,\bar{B}$, the basal section of an octahedron of copper pyrites, is a square, the sections $C\,A\,\bar{C}\,\bar{A}$, $C\,B\,\bar{C}\,\bar{B}$ through the terminal edges are merely rhombs, the angles in the vertical axis being 90° 51′, and in the horizontal axes 89° 9′; the triangular faces are only isosceles, the vertical angle of each being 60° 29′ and the basal angles 59° 45½′.

In fig. 1, $a\,b\,c\,d$ and $\alpha\,\beta\,\gamma\,\delta$ are the stereographic projections of the points in which lines drawn through the centre parallel to the normals of the faces of the octahedron of fig. 2 would meet the sphere.

The faces of the form {1 0 1} truncate the terminal edges of the octahedron {1 1 1}; $T\,\bar{T}\,Q\,\bar{Q}$, four of the poles of this form, are shown in fig. 1.

According to Haidinger's measurement, $2\,Ca$ is 108° 40′, whence $\tan QC = \tan Ca \cos Q\,Ca = \tan Ca \cos 45°$.

Thus

$$QC = 44°\ 34\tfrac{1}{2}',\ Q\,T = 90°\ 51',\ \text{and}\ Q\,a = 35°\ 3\tfrac{3}{4}';$$

also

$$\cos Ta = \cos Q\,a \cos Q\,T,$$

and

$$Ta = T\beta = 90°\ 41\tfrac{3}{4}',$$

while

$$T\alpha = Tb = 180° - 90°\ 41\tfrac{3}{4}' = 89°\ 18\tfrac{1}{4}'.$$

Fig. 3 represents a second crystal, with its faces $a_1 b_1 c_1 d_1 \alpha_1 \beta_1 \gamma_1 \delta_1$ parallel respectively to the faces $a b c d \alpha \beta \gamma \delta$ of fig. 2.

Both versions of the law assume that the plane of rotation or the twin-plane is a face of the form $\{1\,0\,1\}$. Let us take for the particular plane of rotation the plane $(1\,0\,\bar{1})$ which truncates the edge $\delta_1 c_1$, or the edge δc, and is represented in the stereographic projection by its pole T.

On rotating the crystal represented in fig. 3 through two right angles round the normal $T\bar{T}$ to the plane $(1\,0\,\bar{1})$, its faces will take up positions represented in fig. 4; and the poles of the faces in this new arrangement are introduced in fig. 1 as $a_1 b_1 c_1 d_1 \alpha_1 \beta_1 \gamma_1 \delta_1$. In the first place we may remark that as each of the pairs of faces $\delta_1 c_1$ and $\gamma_1 d_1$ is diagonally symmetrical to the line T T about which the rotation takes place, c_1 will after the rotation have a direction parallel to that belonging previously to the face δ_1, and still belonging to the face δ; and similarly the face d_1 will after the rotation have a direction parallel to that belonging previously to the face γ_1 and still belonging to γ. In other words, the faces $c_1 d_1 \gamma_1 \delta_1$ of the rotated octahedron shown in fig. 4 are respectively parallel to the faces $\delta \gamma d c$ of the octahedron of fig. 2. If the octahedron had been the regular one of geometry, not only these faces, but all the remaining faces and all the edges of the octahedron of fig. 4 would have been parallel to faces and edges of the octahedron shown in fig. 2. As the line $T\bar{T}$ bisecting $C\bar{C}$ is perpendicular to the edge $\bar{C}A$, and therefore not parallel to the edge CA, the point T will not be midway between A and $\bar{C}$; and thus, although the edge $A\bar{C}$ would be unchanged in direction by a rotation through two right angles about $T\bar{T}$, while B would be rotated to $\bar{B}$ and $\bar{B}$ to B, yet $\bar{C}$ would not be rotated exactly to A nor A exactly to $\bar{C}$; and the edge CA of fig. 2 will thus not be parallel to $A_1\bar{C}_1$ of fig. 4, nor the edge BA to the edge $\bar{B}_1\bar{C}_1$. In fact, while the edge $A_1\bar{C}_1$ is parallel to the edge CA, the angle $A_1\bar{C}_1\bar{A}_1$ is, as stated above, 90° 51′, and the angle CAC is 89° 9′; whence the edges $\bar{C}_1\bar{A}_1$ CA must have a mutual inclination of 1° 42′. Similarly, although the edge $A_1\bar{C}_1$ is parallel to the edge CA, and the plane $\bar{C}AB$ to the plane $A_1\bar{C}_1\bar{B}_1$, the angle $A_1\bar{C}_1\bar{B}_1$, as stated above, is 60° 29′, and the angle $\bar{C}AB$ is 59° 45½′, whence the edges BA $\bar{B}_1\bar{C}_1$ are mutually inclined at an angle of 43½′.

The same results will follow, perhaps more simply, from

a study of the stereographic projection of fig. 1; for as $T c = T \delta$, c_1 will be rotated into the position of δ, and δ_1 into the position of c, while γ_1 will be rotated to d, and d_1 to γ. With the other poles it will be different: thus α will rotate into the position α_1, where $T\alpha = T\alpha_1 = 89° 18\frac{1}{4}'$, and $a\alpha_1 = Ta - T\alpha_1 = 90° 41\frac{3}{4}' - 89° 18\frac{1}{4}' = 1° 23\frac{1}{2}' = \beta b_1 = \alpha a_1 = b\beta_1$. Also $T\,\bar{Q}_1 = T\,\bar{Q} = 89° 9'$, whence $Q\,\bar{Q}_1 = 90° 51' - 89° 9' = 1° 42'$; and $T\,C = T\,\bar{C}_1 = 44° 34\frac{1}{2}'$, whence $C\,\bar{C}_1 = 90° 51'$.

Next, let M be a point bisecting the arc $Q\,\bar{Q}_1$; the poles of the two octahedra will be symmetrically disposed to the twin-plane, represented in the projection by the line $\bar{M}\,B\,M$; for $T\,M = T\,B = 90°$, and the arcs Tb_1 $T\alpha_1$ $T\alpha$ Tb $\bar{T}a$ $\bar{T}\beta$ Ta_1 $\bar{T}\beta_1$ are all equal. From this it will follow that $Ma = M\beta = M\alpha_1 = Mb_1$; also that $Ma_1 = M\beta_1 = Mb = M\alpha$, and that $M\gamma$ $M\gamma_1$ Md Md_1 Mc Mc_1 $M\delta$ $M\delta_1$, being all right angles, are equal to each other. The poles of the two individuals are thus not only symmetrical to the twin-plane $M\,B\,\bar{M}$, but also to the plane $T\,B\,\bar{T}$ at right angles with it, represented by an irrational symbol approximating to (100 0 99), and thus not a crystalloid plane. The same arrangement of poles might therefore be obtained by rotating the original octahedron about the line $M\,\bar{M}$ parallel to the tangent of the circle at T, and therefore to a "terminal edge" of the octahedron {1 1 1}. It may be remarked that, although the same arrangement of poles will be obtained by rotation about the lines $T\,\bar{T}$ $M\,\bar{M}$, the lettering will not be identical in the two cases; but as in both cases the planes which are quite or nearly coincident in direction are always represented in the one individual by italic letters and in the other by greek, there will be no crystallographic difference in the results of these two methods of derivation. This proves that, as has been mentioned above, Haidinger's statement, "regular composition often takes place parallel to a plane of {1 0 1} or perpendicular to the terminal edges of {1 1 1}," indicates only a single case if the reference be to the plane of rotation.

Haidinger's law might therefore be equally well expressed in the two following ways:—

I. Twin-plane a face of the form {1 0 1}; composition-plane perpendicular to the twin-plane.

II. Twin-axis a terminal edge of {1 1 1}; composition-plane parallel to the twin-plane.

We are now in a position to discuss the difference of growth which will be produced by a variation in the position of the plane of composition. As the line $T\,\bar{T}$ is perpendicular to the

edges $A\,\bar{C}$ $C\,\bar{A}$ and passes through the centre of the crystal, the rhombs $T\,B\,\bar{T}\,\bar{B}$ $T\,B_1\,\bar{T}\,\bar{B}_1$ of figs. 2 and 3 will be perpendicular to the twin-plane, and will each represent the composition-plane of Haidinger. If, now, by simple translation without rotation the half $T\,B\,\bar{T}\,\bar{B}\,A\,C$ above the rhomb of fig. 2 be associated with the half $T\,\bar{B}_1\,T\,B_1\,A_1C_1$ below the rhomb of fig. 4 in such a way that the two rhombs coincide, we get the composition represented in fig. 5. This figure will therefore be that of a growth according to the law of Haidinger. For convenience of direct comparison with the results following from the law as given by Naumann and Sadebeck, the same growth as fig. 5 is shown in fig. 6 as it would be seen either after a rotation of the whole figure through two right angles about the vertical axis, or by an eye placed at the back of the paper.

Again, let the rhombs $M\,B\,\bar{M}\,\bar{B}$ $M\,B_1\,\bar{M}\,\bar{B}_1$ of figs. 2 and 3 be the traces on the octahedron-faces of a plane parallel to the twin-plane, and therefore the composition-plane of Naumann. If now, just as before, the half $M\,B\,\bar{M}\,\bar{B}\,C\,\bar{A}$ above the rhomb of fig. 2 be associated with the half $M\,\bar{B}_1\,M\,B_1\,A_1\,\bar{C}_1$ below the rhomb of fig. 4 in such a way that the rhombs coincide, we get the composition represented in fig. 7. This figure will therefore be that of a growth according to the law of Naumann.

With the help of the stereographic projection of fig. 1 we can now investigate the differences of the growths represented respectively in figs. 6 and 7.

Fig. 6.	Fig. 7.
Haidinger's law.	*Naumann's law.*
$d\gamma_1 = \gamma d_1 = 0,$ $c\delta_1 = \delta c_1 = 0.$	$a\,\alpha_1 = \beta\,b_1 =$ salient $1^\circ\ 23\frac{1}{2}'$, $b\,\beta_1 = \alpha\,a_1 =$ reentrant $1^\circ\ 23\frac{1}{2}'$.
Edges $C\,\bar{T}$ $\bar{T}\,C_1$ coincident,	Angle $C\,M\,.\,M\,\bar{C}_1 =$ salient $1^\circ\ 42'$,
„ $A\,T$ $T\,A_1$ „	„ $\bar{A}\,\bar{M}\,.\,\bar{M}\,A_1 =$ reentrant $1^\circ\ 42'$.
Angle between the truncating planes of C and C_1 $= 89^\circ\ 9'$.	Angle between the truncating planes of C and $\bar{C}_1$ $= 90^\circ\ 51'$.
The faces $a\,\beta\,a_1\,\beta_1$ are not in a zone.	The faces $d\,\gamma\,c_1\,\delta_1$ are in a zone.

We have seen that in the projection the plane of composition is a plane of symmetry to the poles of the two individuals; and we further perceive that in each case the plane of composition is a plane of symmetry to the faces actually shown by

the twin-growths; and the angles of the upper half of fig. 6 are exactly equal to the similarly disposed angles of the upper half of fig. 7, and the angles of the lower half of fig. 6 to the similarly disposed angles of the lower half of fig. 7, the only difference in the growths being the relation of the upper to the lower half.

Of the specimens of copper pyrites in this collection, one figured by Haidinger himself in the 'Catalogue of the Allan-Greg Collection' (now in the possession of the British Museum), though probably not one of the original specimens of the memoir of 1822, offered itself as the most likely to afford a satisfactory solution of the difficulty. In this specimen, which comes from Freiberg, the twin-growths are disposed parallel to each other on galena, and are associated with quartz, chalybite, and calcite. The length of a side of the triangular faces is about 1·5 millim. Haidinger's drawing to illustrate this specimen is virtually the same as fig. 9 (copied from his paper of 1825), the only difference being that the faces of $e\{1\,0\,1\}$ being almost linear are not shown in the Catalogue. This figure represents the rotation as taking place about each of the normals to the four upper faces of $\{1\,0\,1\}$ of the central individual, of which both the upper and the lower halves are present. We may remark that a complex growth of this perfect kind would be explained by the law of Naumann equally with that of Haidinger, seeing that in the lower half of the regular composition the various planes of junction are represented as parallel to the corresponding twin-planes, and in the upper half as perpendicular to them. As none of the growths have an all-round development, the figure represents the growth in theoretical perfection rather than as actually existent; in fact the actual habit is more nearly shown in fig. 10, which at the same time will serve to give an idea of the striation to be observed on the faces.

A crystal from this specimen appeared to show that the triad of octahedron-faces $o\,o_1\,o_2$ did not quite coincide, as according to Haidinger's explanation should be the case; but still no satisfactory measurement of the angle could be obtained. The reentrant angle lying in the zone $c\,c_1$ at the junction of two individuals could, however, be determined with very fair precision, although the faces are finely striated parallel to their edge of intersection with each other. These planes were supposed by Haidinger to belong to the common form $z\{2\,0\,1\}$; and as the angle made by a plane of this octahedron with the adjacent face of the form $\{1\,0\,1\}$ is 18° 31′, the reentrant angle according to his theory should be 37° 2′. Actual measurement,

however, gave 20° 9′; and closer examination rendered it clear that on the crystal measured the faces belonged not to the form $z\{2\,0\,1\}$, but to another less common form $h\{3\,0\,2\}$, whilst smaller almost linear faces of $z\{2\,0\,1\}$ were to be seen lower down in the reentrant angle. The angle between (3 0 2) and (1 0 1) being 11° 20½′, the reentrant angle for this form should be, according to Haidinger's law, 22° 41′; on the other hand, according to Naumann's law, it should be 20° 59′ —that is, the difference of the inclinations of (3 0 2) and its parallel $(\bar{3}\,0\,\bar{2})$ to the plane $(1\,0\,\bar{1})$. The measured angle was thus 50′ less than the angle calculated from Naumann's law; and the latter angle was itself 1° 42′ less than the one calculated according to that of Haidinger.

The difference between the calculated and measured angles is large, and in fact is so considerable that it can only be attributed either to the growth being not strictly a regular twin, or to a deviation from the fundamental angle as determined by Haidinger. As the extreme accuracy of Haidinger's measurement of the fundamental angle of copper pyrites had been confirmed both in the memoir of Sadebeck and in the Catalogue of the Strassburg collection, and also by Kokscharow*, and as, further, this particular crystal did not lend itself to a precise determination of any other angle than the reentrant one above mentioned, the result seemed very unsatisfactory, and for some time the examination was discontinued. Two years later it was resumed; and, happily, another crystal from the same specimen was found to give reflections so good that a precise measurement of the angle between two octahedron-planes belonging to the same individual and on opposite sides of c could be obtained. This was found to be 108° 17½′, a deviation of 22½′ from the angle as determined from the specimens previously measured. A less precise determination of the angle of a terminal edge gave as mean 69° 59¼′, the limiting values being 69° 54¾′ and 70° 0½′. From the same crystal the angle $o\,o_1$ was found by help of the δ eyepiece of a Fuess's goniometer (as improved by Websky) to be 2° 3′, instead of zero according to Haidinger, and 1° 23½′ according to Naumann and Sadebeck.

The whole difficulty had, however, now disappeared, as is shown by the following table of calculated and observed angles:—

* *Bull. Soc. St. Pét.* 1874, xix. p. 562.

If 001.101 =	44° 34½′	44° 22½′	44° 22′	44° 21½′	Observed.
	Calculated.				
$\omega_2 \omega'_2$	108° 40′	108° 17¼′	108° 16¼′	108° 15¼′	108° 17½′
$o\,\omega$	70 7½	69 56	69 55½	69 55	69° 54¾′–70° 0½′, mean 69° 59¼′
$o\,o_1$ Naum....	1 23½	2 3	2 4½	2 6	2° 3′
$o\,o_1$ Haid. ...	0 0	0 0	0 0	0 0	
$h\,h_1$ Naum....	20 59	20 12¾	20 10¾	20 8¾	20° 9′
$h\,h_1$ Haid. ...	22 41	22 42¾	22 42¾	22 42¾	

From a third crystal not quite so perfect, but still giving very good images, the angle $\omega_2 \omega'_2$ was determined to be 108° 18¾′.

There can thus be no doubt that:—

1st. The growth is strictly regular.

2nd. The parametral angle differs from that determined from other specimens by previous observers, and is very nearly 44° 22′.

3rd. The twin-plane is a face of the form {1 0 1}. And

4th. The composition-plane is *parallel* (and not perpendicular) to the twin-plane.

At the suggestion of Prof. Maskelyne, a careful analysis of this specimen was made in the departmental laboratory, with the view of ascertaining to what extent this variation in the fundamental angle is attended by a difference from the chemical composition of ordinary copper pyrites. The following results were obtained by Dr. Walter Flight:—

	Observed.		Calculated,
	I.	II.	$CuFeS_2$.
Copper . .	25·78	30·66	34·45
Iron . . .	35·16	34·11	30·57
Sulphur . .	37·52	[35·23 by diff.]	34·98
Arsenic . .	traces		
Quartz . .	0·28		
	98·74	100·00	100·00

No other metals were discovered, although carefully searched for. The specimen is thus found to contain a considerable and variable excess of FeS_2 over that of typical copper pyrites—the first analysis corresponding very nearly to $CuFeS_2 + \frac{1}{2}FeS_2$, and the second to $CuFeS_2 + \frac{1}{4}FeS_2$. As close examination of the specimen reveals the presence of included minute crystals of iron pyrites (and mispickel), it is possible that even in the second fragment, which was specially

chosen for its approximate homogeneity, this excess of FeS_2 may be almost wholly due to mere admixture of iron pyrites.

Since the above determinations were made, the collection has been enriched by the acquisition of a superb Freiberg specimen of very much the same character as the one just described, but on a relatively colossal scale; for the length of side of the triangular face $o\,o_1\,o_2$ (and this is the smallest) is here not 1·5 but 24 millim. Owing to the absence of reentrant angles, the growth very much resembles a regular octahedron truncated by the faces of a cube, one of the octahedron-faces, however, showing in a very marked way the threefold composition of that part of the crystal.

A further examination of the collection has resulted in the finding of a specimen (from Pool mines near Redruth) which confirms the parallelism of the planes of composition and of twinning in the most satisfactory way. The crystals of copper pyrites have been here deposited on quartz crystals which they partly enclose: they thus have not an all-round development; in fact, there is a practical difficulty in determining more of the growth than is shown in fig. 11; but this, so far as it goes, falls little short of perfection (the dotted lines indicate the twin in theoretical completeness). The images from $\omega\,\omega_2$ were so well defined that the largest magnifier, α, of Fuess's instrument could be used, three different measurements at nearly normal incidence giving respectively for the value of this angle 1° 23′, 1° 23½′, and 1° 23′, while a measurement at almost grazing incidence, when the images were broader, gave 1° 23½′. The faces $o\,o_2$ were not so perfect; but still, with the δ eyepiece, good images were seen, from which two consecutive measurements of the angle gave 1° 23′ and 1° 23½′. Two measurements of $o\,o'$ gave respectively 108° 39′ 30″ and 108° 39′ 45″. The following table renders more evident the close correspondence of the observed and calculated angles.

	Observed.		Calculated from 001.101 = 44° 34½′.
	Angles of one individual.	Symmetrically disposed angles of the other.	
$o\,\omega$.....	70° 7½′	$o_2\,\omega_2$..... 70° 7½′	70° 7′ 32″
$\omega\,e$.....	35° 3½′	$\omega_2\,e_2$..... 35° 4′	35° 3′ 46″
$\omega\,o'$	70° 4¾′	$\omega_2\,o'_2$ .... 70° 12′, 70° 4½′	70° 7′ 32″
		$o'_2\,e'_2$ .... 35° 4′	35° 3′ 46″
$o\,o'$	108° 39⅝′	$o_2\,o'_2$.....108° 37′	108° 40′ 4″
Also	$\omega\,\omega_2$.....	1° 23′, 1° 23½′, 1° 23′, 1° 23½′	1° 23′ 30″
	$o\,o_2$	1° 23′, 1° 23½′	1° 23′ 30″

The linear faces $e\{1\,0\,1\}$ and the minute faces of another

octahedron $g\{2\,0\,3\}$, although present on the crystal, are not shown in the figure.

A chemical examination made by Dr. Flight shows that this specimen has a composition very nearly represented by the typical formula $CuFeS_2$. The following results were obtained:—

	Observed.	Calculated, $CuFeS_2$.
Copper	34·37	34·45
Iron	30·03	30·57
Sulphur.....	31·92	34·98
Quartz	4·19	
	100·51	100·00

Up to this point, for the sake of simplicity, a very important property of copper pyrites, its hemihedral structure, has been left as much as possible out of sight. It is found, however, that the faces of the octahedron $\{1\,1\,1\}$ of this mineral are not all similar, but must be regarded as belonging to two distinct tetrahedra: in the case of the first tetrahedron, for convenience of distinction termed the positive or *o* tetrahedron, the faces are rough or striated, and are sometimes coated with oxide of iron; on the other hand, the faces of the second or negative or ω tetrahedron are smooth, bright, free from this coating, and in general smaller than the former. From this it follows that the set of faces denoted above by the italic letters *a b c d*, though similar to each other, are distinct in physical character from those denoted by the greek letters $\alpha\,\beta\,\gamma\,\delta$; whence we infer that in such a growth as would be represented by fig. 7, where there has been a simple rotation of one individual through two right angles from a position of identical orientation with the other, and adjacent faces of the two individuals thus bear respectively italic and greek letters, the composition-plane will be a plane of geometrical, but not of physical symmetry. As, however, the correlative tetrahedra and also the correlative hemiscalenohedra are independent of each other, not only in surface-characteristics, but also in their presence on the crystal, even this geometrical symmetry could scarcely be expected in the actual twin-growth.

Now Sadebeck states that in the actual twin-growth the composition-plane is really a plane of symmetry not only to the geometrical, but to the physical peculiarities—the regular composition thus belonging to the class called by Groth "symmetric twins;" that instead of the faces of the octahedron which are parallel, or nearly so, in the two individuals belonging in one to the positive, and in the other to the negative, they really belong either both to the positive or both to the negative tetrahedron. To pass, therefore, to the actual

twin from parallel orientation of the individuals, there must be, in addition to the rotation through two right angles round a normal to $(1\,0\,\bar{1})$, a *further rotation* of one of the two crystals either through two right angles about a normal to one of the faces of the prism $\{1\,1\,0\}$, or through a single right angle about the vertical axis parallel to the edges of this prism. Though this double rotation may be compounded into a single rotation round the normal to a face of the octahedron $\{1\,1\,1\}$, the angle of this single rotation will not be 180°, as is the case in other twins, but 119° 31′.

If this statement of Sadebeck be accepted as having a satisfactory foundation, the growth must be regarded as up to the present unique in character; for no other regular composition appears to have yet been discovered in which, starting from a parallel orientation, a double rotation is *absolutely* necessary for the representation of the relative disposition of the two individuals. There exist twin-growths of *tetartohedral* crystals, it is true, such as those of sodium chlorate and certain regular compositions of quartz, described by Prof. Groth, which are somewhat analogous in character; but they are capable of a more or less satisfactory representation by a simple rotation of one of the individuals through two right angles from a position where corresponding crystallographic lines of the right and left individuals are identical in direction; they are moreover intimately related to the directions of the crystallographic axes. As, however, it had been impossible for Sadebeck to convince himself, from simple examination of the specimens, that certain faces assumed by Haidinger to be parallel might not be inclined to each other at an angle of 1° 23½′, it was possible to entertain a doubt as to the specimens being sufficiently well crystallised to allow of an absolute certainty in the distinction of the two tetrahedra; and as the law is so curious from its extreme rarity and simplicity, and so important in its bearing on the general question of twin-growth, about which there has lately been much discussion, it seemed desirable to place the law, if possible, beyond all suspicion.

The accuracy of Sadebeck's inference as to the disposition of the two tetrahedra in this twin-growth is confirmed in the most satisfactory manner by the specimens in this collection.

The Freiberg specimen of fig. 10 shows not only that the individuals are symmetrical to the plane of composition, but also that the differences of the two tetrahedra of each individual are too marked to allow of this symmetry of physical peculiarities being an accident of the growth.

The specimen from Pool mines (fig. 11) is even more satisfactory still; for the faces $\omega\,\omega_2$ which give such excellent

images are perfectly smooth and bright, and remarkably different in aspect from the two dull and striated faces $o\ o_2$.

A further example is presented by a specimen (probably from the Trevannance mine, St. Agnes) shown in fig. 12, which the symmetry to the combination-plane and the extreme difference between the smooth and the deeply-striated tetrahedra render most convincing. The angle between these striations is so very definite that it can be measured with fair accuracy by means of a microscope; it was determined to be $120\frac{3}{4}°$, the angle calculated according to Naumann's law being 120° 28′, and according to Haidinger's law 119° 31′.

Finally, we may refer to fig. 8, representing a Cornwall specimen (now in the Museum) figured in 1825 by Haidinger himself in his memoir on the Regular Composition of Crystallised Bodies. Here the predominant form of each individual is a hemiscalenohedron; and this in each pair is symmetrically disposed to the plane of composition. Although this specimen is symmetrical in its habit, the planes *s* are so striated and rounded that it was found impossible to assign to them a definite symbol; they lie, however, in the zone defined by the symbol $[1\,1\,\bar{2}]$, and approximate to $\{3\,1\,2\}$.

We conclude, therefore, that there is no doubt of the actual existence of a kind of twin-growth which it is not possible to represent by a single rotation through two right angles from a position of parallel orientation of one of the individuals to the other—that for the representation of this growth an additional rotation is requisite, but that the simplest mode of representation is the one which regards the two individuals as symmetrical to a plane.

EXPLANATION OF PLATE VI.

(To accentuate the differences in twin-growths according to the laws of Haidinger and Naumann, figs. 1–7 are drawn for a parametral angle $42\frac{1}{2}°$ instead of 44° 34½′.)

Fig. 1. Stereographic projection of the poles of $\{1\,1\,1\}$, and of the same twinned about T T, the normal to $(1\,0\,1)$.

Fig. 2. The octahedron $a\,b\,c\,d\,\alpha\,\beta\,\gamma\,\delta\ \{1\,1\,1\}$.

Fig. 3. The octahedron $a_1\,b_1\,c_1\,d_1\,\alpha_1\,\beta_1\,\gamma_1\,\delta_1\ \{1\,1\,1\}$, parallel to the last.

Fig. 4. The same turned through two right angles round T $\bar{\text{T}}$ the normal to $(1\,0\,\bar{1})$.

Fig. 5. Twin-growth of $\{1\,1\,1\}$, according to Haidinger's law.

Fig. 6. The same, viewed from the opposite side.

Fig. 7. Twin-growth of $\{1\,1\,1\}$, according to Naumann's law.

Fig. 8. Twin-growth, with faces *s* of a hemiscalenohedron or disphenoid (Haidinger, Edin. J. of Sc. 1825).

Fig. 9. Twin-growth of $\{1\,1\,1\}$ $\{0\,0\,1\}$ $\{1\,0\,1\}$ $\{2\,0\,1\}$ (Haidinger, Edin. J. of Sc. 1825).

Fig. 10. A similar twin-growth.

Fig. 11. A twin-crystal from Pool mines, near Redruth.

Fig. 12. A twin-crystal, probably from Trevannance mine, St. Agnes.

XXXII. *The Tails of Comets.* *By* E. Vansittart Neale.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I VENTURE to offer an explanation of the remarkable phenomena presented by the tails of comets, so simple that, considering the number of eminent astronomers who have turned their attention to this subject, and to whom it has remained a mystery, I am almost afraid to suggest my explanation, lest it should prove a sort of scientific mare's nest. Still, since I cannot see where it fails, while the subject is one of considerable interest, I have determined to run the risk of some eye more penetrating than my own discovering my error, if there is one.

My explanation rests on the interaction of three forces, of which two are known to exist, while the existence of the third may, I think, be reasonably assumed. These forces are:—(1) the force which urges the comet towards the sun; (2) the expansive force of the heat of the sun; (3) the resistance of an atmosphere surrounding the sun.

That there is such an atmosphere extending many hundred thousand miles from the sun's centre we know, because it can be seen. How much further it may extend in a condition in which it is invisible we do not know; but, considering the distance to which the atmosphere surrounding the earth is known to extend by its action on meteoric bodies, we may, I think, reasonably assume the existence of an invisible solar atmosphere, extending far beyond the limits of the visible atmosphere; and if this is assumed, we obtain an easy explanation of the phenomena of comets' tails.

The changes in a comet which give rise to its tail begin, according to the account given by Mr. Higgins in his excellent article on Comets in the current number of the 'Nineteenth Century,' by jets of a gaseous nature ejected towards the sun, *i. e.* in the direction determined by the expansive force of the sun's heat, acting on the comet, combined with the moving force of the comet. But soon, he says, these jets bend round, as if they were carried back by a strong wind, and form an envelope round the body of the comet and a cone behind it. Just so. It is what would happen to a man dressed in a loose robe and running rapidly through the air. His dress, though moving with him, would stream behind him, because it experienced more resistance than his body from the air. The jets

of gas which escape from the body of the comet and expand when they leave it are more resisted by the sun's atmosphere than its more solid head, which therefore gradually overtakes them ; so that they seem to fall back, till they constitute an envelope round it, and then spread in a conical form behind the head, through the joint operation of their own lateral movements, of the increasing expansive force of the sun's heat as the comet approaches the sun, of the increasing resistance of the sun's atmosphere (whence the head must continually gain more and more on the parts of the tail at first thrown off), and of the pressure of fresh envelopes continually forming round the nucleus as it advances. The body of the comet is constantly moving through the gases or vapours which it throws off, and thus produces the appearance of a tail, by leaving each successive part thrown off more and more behind it.

The central line of these successive envelopes would obviously tend to be a straight line from the sun's centre through the head of the comet; though the continual change of direction in this line, as the comet approaches its perihelion, must be liable to produce an apparent curvature in the tail, because the parts first emitted, and therefore most distant from the body, if they retain luminosity enough to be visible, must fall more and more behind the advance of this central line in its sweep round the sun.

The phenomena of divided tails, of bright streaks, &c., find a ready explanation in the accidental variety of pressures to be expected among jets of gas or vapour emitted under such circumstances, and the effect of perspective, according as we happen to look through the edges or across the more central parts of the envelopes forming the comet's tail—possibly combined with actual variations of pressure in the sun's invisible atmosphere, arising out of the enormous changes which can be observed in its visible atmosphere. But what as to the change in the direction of the tail when a comet has passed its perihelion? Why do comets then carry their tails before instead of behind their heads? Because the direction of the pressures which produce the tail has changed. Given an invisible solar atmosphere, a comet moving towards the sun will be perpetually passing from a rarer into a denser medium, while a comet moving from the sun will be perpetually passing from a denser into a rarer medium. At the same time the jets of gases or vapours which it will continue to emit from the expansive force of the sun's heat will then consist of particles moving from the sun. Thus the two tendencies—the movement of these particles due to the action of gravity, and the tendency of the expansive force to exert itself in the line of

least resistance—will combine to carry the luminous particles emitted from the comet in advance of the mass.

That the change in the direction of a comet's tail should take place with the rapidity and to the extent observed in the case of the enormous appendages of some of these bodies may still appear surprising. But it must be remembered that, in these cases, we can speak only of what we see. The conical mass of gases or vapours extending behind the nucleus of a comet may attain, in the case of the largest of these bodies, to an expansion much greater than the part visible, which may consist only of the parts that receive the strongest impulses from the centre of force; so that when the tail seems to have swung round through an enormous arc in the sky, what has really happened may be only that the line along which the substances forming it become visible may have shifted, in consequence of the direction of the impulses proceeding from the head having altered.

As the time when the most rapid alteration in the direction of the tail of a comet takes place necessarily coincides with that when the expansive action of the sun on the substances emitted from the comet is at its maximum, there must be the less difficulty in admitting the last hypothesis as an explanation of this phenomenon. It is the only one, so far as I see, that offers any difficulty in the way of the theory now proposed respecting the tails of comets, which may be summed up in the proposition that, as the incandescence of meteoric bodies proves to us the existence of a widely diffused atmosphere surrounding the earth, so the development of the tails of Comets proves to us the existence of a much more widely diffused atmosphere surrounding the sun—both sets of phenomena being due to the same cause, namely the resistance of these atmospheres to bodies rapidly passing through them.

I am yours &c.,

E. Vansittart Neale.

15 Portsmouth Street, Manchester,
August 21, 1882.

XXXIII. *Simple Method for Calibrating Thermometers.* *By* Silas W. Holman*.

THE calibration of a thermometer by most of the methods in ordinary use is a tedious and somewhat difficult operation, and hence often neglected even in important work. For the purpose of supplying a method simple both in observation and computation, and at the same time accurate, the following

* From Silliman's American Journal, No. 136, p. 278.

process is described, which, although involving little that is novel, has not to my knowledge been used before.

First, however, it is necessary to recall to the attention of observers the fact that, without calibration-correction, the readings of a thermometer having a scale of equal linear parts cannot be relied upon within one or more divisions of this scale, and that thermometer-makers, knowing this, almost universally space the graduation upon the tube to correspond more or less closely with the shape of the bore, as determined by previous calibration, or by comparison with a standard (!) instrument. This practice is much more general than is ordinarily supposed, and has an important bearing upon the accuracy of the work done with such instruments. For the scale thus made is merely approximate, the dividing-engine or other tool being usually changed only at such intervals as to make the average error less than some specified amount. An inspection of these conditions will show that the calibration of such a tube and scale can be only approximate, except with corrections for the inequalities of the spacing, involving an amount of labour disproportionate to the result obtained. The best makers, such as Fastré, Baudin, and others, have produced satisfactory thermometers graduated to equal volumes; but even these are not as reliable as instruments of less cost with a scale of equal linear parts, say of millimetres, supplemented by a calibration by the observer. The best form of tube for almost all work is one backed with white enamel, with an inverted pear-shaped bulb at the upper end of the capillary (a very important feature), and with a scale of equal arbitrary linear parts (0·7 to 1 millim. is a suitable length for estimation of tenths) or of approximate degrees, for convenience, etched or engraved upon it.

Without reviewing here the methods proposed by various writers, it may be said that it has been the general plan to select beforehand upon the scale two points between which to make the calibration, this space being the "calibration unit," the errors of these points being, of course, zero. This plan has led to unnecessary complexity. Such an assumption is no more requisite in calibration after a scale has been put upon the tube, than in calibrating by the dividing-engine or micrometer before making the scale. It is obvious that the selection of these points is wholly arbitrary, and, if used at all, one or both of them may, if desirable, be chosen after the observations with the calibrating-thread have been made. The choice should be made with a view to facilitating the work. Hence the use of the observed freezing- and boiling-points, upon which some methods are based, is most undesirable.

In the method which will now be given, either one or both of these points may be left to be selected, according to the combined conditions of length of thread employed, shape of the tube, and numerical convenience, after the observations with the thread have been made.

Let it be desired to find the calibration-corrections for a given tube. Determinations which will give the errors of every 3 centim. of length will ordinarily be sufficient; but this must depend on the result sought. Separate a thread of mercury of about that length. The actual length of the thread within two or three millimetres is of no consequence whatever; and hence a suitable thread can be obtained in a very short time.

Set the thread with its lower end at or near the beginning of the graduation: call the reading* of the lower end of the thread l_1, and that of the upper end u_1. Move the thread less than 1 millim. and read again, finding thus l_2 and u_2. Move the thread about 1 centim., and read l_3 and u_3. Move the thread less than 1 millim., and read l_4 and u_4. So continue throughout the whole length of graduation, increasing the number of settings or repeating the whole series in reverse order and several times, if the highest attainable precision is desired. This alternation between 1 millim. and 1 centim. in setting tends towards the better elimination of errors in estimation. It is not, however, essential, nor even always as well as an equal number of distributed readings. This must depend upon the skill of the observer. Avoid, as far as convenient, taking readings with an end of the thread apparently just at the line of the scale, as the width of the line, even in the best scales, is a source of considerable error†. If any point (*e. g.* the zero-point of the graduation) has for any reason been selected as the first of which the error should be assumed zero, the settings may to advantage, though not necessarily, be made to extend each way from this.

Then u_1-l_1, u_2-l_2, &c. will give a series of lengths of the calibrating-thread in all parts of the tube. Before reuniting this thread to the rest of the mercury, plot points with abscissas l_1, l_2, &c., and ordinates u_1-l_1, u_2-l_2, &c., the corresponding lengths of thread, and draw a smooth curve through the points thus obtained. This line will give a general idea of the form of the capillary bore; and should any parts of it show considerable irregularities, the corresponding portions of the tube should at once be reexplored with the thread.

If not already done, the point A upon the scale, to be used

* Tenths of a division are supposed to be read by estimation.

† Some of the advantages of Neumann's method are offset by this error.

as the starting- or reference-point of the computation, should now be selected. In general the extreme ends of the tube are to be avoided, as more likely to have been rendered irregular or rapidly tapering in the process of making or joining on the bulbs. If the zero of the numbering is placed one or two centimetres from the bottom of the tube, it forms a desirable starting-point.

Find upon the curve the ordinate u' corresponding to the abscissa A; then with abscissa $A+u'$ find the corresponding ordinate u'', with abscissa $A+u'+u''$ find the corresponding ordinate u''', continuing to the upper limit of the graduation. If A is at a sufficient distance from the lower end of the graduation, find a similar series below the point A. These points, A, $A+u'$, $A+u'+u''$, &c., upon the graduation are separated by equal volumes of the capillary. Select any one of these as the second point of which the error is to be arbitrarily assumed as zero, and call this B. Then

$$A+u'+u''+\ldots+u\,\text{nth}=B.$$

There are thus n spaces of equal volume between A and B; and these correspond each to $\frac{1}{n}$th of the interval B−A. Hence the true reading (which, however, it is not necessary to compute numerically) at the point

$$\begin{aligned}
&A && \text{is } A,\\
&A+u' && \text{is } A+\frac{1}{n}(B-A),\\
&A+u'+u'' && \text{is } A+\frac{2}{n}(B-A),\\
&\quad\ldots\ldots\ldots && \\
&B && \text{is } B.
\end{aligned}$$

And the error obtained by subtracting the true readings, as given in the right-hand column, from the corresponding actual readings, given in the left-hand column, at

$$\begin{aligned}
&A && \text{is } 0,\\
&A+u' && \text{is } A+u'\left\{A+\frac{1}{n}(B-A)\right\}=u'-\frac{1}{n}(B-A),\\
&A+u'+u'' && \text{is } u'u''-+\frac{2}{n}(B-A),\\
&\quad\ldots\ldots\ldots && \\
&B && \text{is } 0.
\end{aligned}$$

In selecting B it might have been assumed equal to $A+u'$, thus making $n=1$. This would somewhat simplify the calcu-

lation, and would be of equal accuracy, but is objectionable from the fact that, in general, this volume would differ considerably from the average volume obtained when n has a greater value (always an integer), and the resulting series of errors would assume larger numerical values.

The errors or corrections are, for purposes of interpolation, most conveniently represented graphically by a smooth curve through points with abscissas proportional to the direct readings

$$A,\ A+u',\ A+u'+u'',\ \&c.,$$

and ordinates to the corresponding corrections.

Should it be necessary to increase the accuracy by a second calibration with a thread of different length, it is only necessary to take one of approximately an integral part of (B−A), and when the final curve of error is drawn make the error at B equal to zero, distributing the difference at that point proportionally to the scale-readings among the errors at the intermediate points—in other words, to shift the axis of the second curve of error so that it shall make the error at B zero.

This method requires for each calibration the use of but a single thread. The computation is simple, and involves a minimum of approximation. Errors of observation are largely eliminated by the number of settings made in all parts of the tube, and by the inspection of the curve of lengths; both of which operations tend in an unusual degree to detect mistakes or any minor irregularities of the capillary. It avoids the common requirements of setting the thread exactly at certain definite points in the tube, or any approximate correction for slight errors in such setting—two sources of considerable error and inconvenience, especially when the thread must be set near or under a line of the graduation. And, lastly, the total time of calibration for a result of given accuracy is reduced to one half or one third of that required by Neumann's method, the quickest and most satisfactory with which I am acquainted except that given by Pickering. The latter, described with some slight inaccuracies, at the reference noted below, is a neat application of the graphical method; and the curve of lengths of thread adopted in the method which I have described is identical with the corresponding one given by Professor Pickering, while the whole process is fully one third shorter and somewhat more accurate. From a series of calibrations executed upon the same thermometer (one with a millimetre-scale, by Baudin, of Paris), using a variety of methods, I have obtained slightly more concordant results with the proposed method than with Neumann's or Pickering's

(all those possessing, however, nearly the same degree of precision), and decidedly better results with these than with any of the other existing simple methods.

Considerable aid in eliminating errors of parallax in such work is sometimes found by looking down upon the horizontal thermometer through a vertical tube having a small hole at each end. One of the cheap French microscopes with its lenses removed, and inverted in its stand, answers this purpose well.

With such a device two calibrations of the above-described thermometer with threads of 3 and 5 centim. respectively, each with only one series of observations, and requiring not more than one hour and a half each for completion, gave results whose average difference from each other at nine points was 0·04 millim., and the arithmetical sum of the extreme differences was 0·12 millim., a result of sufficient accuracy for any class of work of which such an instrument is capable.

For brief descriptions of methods of separating threads of mercury for calibration, reference may be made to the paper by Russell, and the text-book by Pickering, noted below. These processes are in general use, and are safe and convenient.

References upon Calibration of Closed Thermometer-Tubes.

Bessel, Pogg. *Ann.* vi. p. 287 (1826).
Rudberg, Pogg. *Ann.* ix. pp. 353, 566; xxxvii. p. 376 (1836); xl. pp. 39, 562 (1837).
Kohlrausch, 'Physical Measurements,' p. 59 (English translation).
Pickering, 'Physical Manipulation,' ii. p. 75 (1876).
Thiesen (Neumann's Meth.), Carl's *Rep.* xv. p. 285 (1879).
Russell (Neumann's Meth., transl. from Thiesen), Amer. Journ. Sci. xxi. p. 373 (1881).
Marek, Carl's *Repertorium*, xv. p. 300 (1879). (Solution by least squares.)
von Oettingen, *Inaug. Diss.*, Dorpat, 1865. (This I have been unable to obtain.—S. W. H.)

XXXIV. *On* Boltzmann's *Theorem on the average Distribution of Energy in a System of Material Points**.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Gräz, April 6, 1882.

POSSIBLY you may feel disposed to have the accompanying notice of a paper of Maxwell's translated for your valuable 'Philosophical Magazine.' So far as I know, this excellent paper of Maxwell's has not been reprinted in your Magazine; it may not, therefore, be without interest to your readers that some notice of it should appear, if only as

* Translated from Wiedemann's *Beiblätter*.

an abstract; and, further, my notice contains one on two new things, amongst which a remark on Watson's excellent book may be interesting to English physicists.

With highest esteem,

Yours &c.,

BOLTZMANN.

MAXWELL (Camb. Phil. Trans. vol. xii. part 3, pp. 547–570, 1879) shows that this theorem may be easily proved by means of Hamilton's principle. The theorem is also extended, since it is shown to hold good for any systems determined by generalized coordinates, if only they satisfy the principle of conservation of energy. There is a difference in method between Maxwell and Boltzmann, inasmuch as Boltzmann measures the probability of a condition by the time during which the system possesses this condition on the average, whereas Maxwell considers innumerable similarly constituted systems with all possible initial conditions. The ratio of the number of systems which are in that condition to the total number of systems determines the probability in question. In conclusion, Maxwell finds, further, that also for any unstable system of very many atoms in rotation under the action of no external forces the mean energy of internal motion is the same for each atom, and that a mixture of gases in a rotating tube behaves exactly as if each gas were present by itself.

Maxwell's proof mentioned above is as follows:—Let there be given any system S obeying the principle of energy. Let its configuration be determined by n generalized coordinates $q_1 \dots q_n$; let the corresponding momenta be $p_1 \dots p_n$. (For the sake of clearness I will take occasionally the simplest example, a system of material points acted on by any forces. $q_1 \dots q_n$ will then denote rectangular coordinates, $p_1 \dots p_n$ the products of the component velocity into the corresponding masses.)

Let the law of the forces acting in the first system be such that the potential energy V is a given function of the coordinates. Then the motion of the system is completely determined when we know the values $q'_1 \dots p'_n$ of the coordinates and momenta at the commencement of motion and the time τ which has elapsed. (In the example this means that the coordinates and component velocities at the commencement of motion must be known.) It is then most natural to take the $2n+1$ quantities $q'_1 \dots p'_n, \tau$ as so-called independent variables. Since the law of action of the forces is given, all other quantities relating to the motion (*e. g.* the values of the coordinates and momenta after the lapse of the time τ, which Maxwell denotes by

$q_1 \ldots p^n$ without index) may be calculated as functions of these $2n+1$ independent variables. If T be the kinetic energy at the time τ, then $V+T=E$ is the whole energy of the system. These quantities may, of course, also be expressed as functions of the $2n+1$ independent variables*.

If we imagine each of the $2n+1$ quantities $q_1 \ldots p_n E$ actually expressed as a function of the $2n+1$ independent variables, we obtain $2n+1$ equations between $4n+2$ variables. Hamilton's method consists in introducing in place of the independents hitherto chosen, which we may call the "old independents," other independents (the Hamiltonian independents). We may, in fact, from the $2n+1$ equations express any $2n+1$ variables out of the $4n+2$ variables occurring as functions of the remaining $2n+1$. Hamilton supposes the variables $p_1 \ldots p_n, p'_1 \ldots p'_n, \tau$ expressed as functions of $q_1 \ldots q_n$, $q'_1 \ldots q'_n E$; so that the last-named variables play the part of independents. Each of the first-named variables is therefore now to be regarded as a known function of these $2n+1$ independent variables. Starting from these Hamiltonian independents, we easily find

$$\frac{dp'_r}{dq_s} = -\frac{dp_s}{dq'_r}, \quad \frac{dp'_r}{dE} = -\frac{d\tau}{dq'_r}, \quad \frac{d\tau}{dq_r} = \frac{dp_r}{dE}\dagger,$$

where r and s are any equal or unequal numbers.

Just as the product of the differentials of three rectangular coordinates dx, dy, dz may be expressed by the product of the differentials of polar coordinates and then becomes equal to $r^2 \sin\theta \, dr \, d\theta \, d\phi$, so if any m variables v_1, $v_2 \ldots v_m$ are functions of m other u_1, $u_2 \ldots u_m$, the product of the differentials of the first variables may be expressed by the product of the differentials of the latter, by means of the well-known functional determinant

$$dv_1 \, dv_2 \ldots dv_m = du_1 \, du_2 \ldots du_m \, \Sigma \pm \frac{dv_1}{du_1} \frac{dv_2}{du_2} \quad \frac{dv_m}{du_m}. \quad . \quad (1)$$

* E will not contain τ, and will therefore simply be a function of $q'_1 \ldots p'_n$, since it remains constant during the whole motion.

† This follows thus:—If the magnitude $\Lambda = 2\int_0^\tau T \, dt$ be expressed as a function of the Hamiltonian independents, then Hamilton shows (Thomson and Tait's 'Natural Philosophy,' new ed. § 330, equation 18) that

$$p'_r = -\frac{d\Lambda}{dq'_r}, \quad p_s = \frac{d\Lambda}{dq_s}, \quad \tau = \frac{d\Lambda}{dE};$$

whence it follows at once that

$$\frac{dp'_r}{dq_s} = -\frac{dp_s}{dq'_r} = \frac{d^2\Lambda}{dq_r \, dq'_s},$$

and so on.

We have a special case if some of the v's are identical with some of the u's—if, for example, we retain the z-coordinates and transform only x and y into polar coordinates. Suppose that $v_1 = u_1$, $v_2 = u_2, \ldots v_k = u_k$, but that $v_{k+1} \ldots v_m$ are given functions of $u_1, u_2, \ldots u_m$; then the functional determinant is simplified to

$$dv_1\, dv_2 \ldots dv_m = du_1\, du_2 \ldots du_m \Sigma \pm \frac{dv_{k+1}}{du_{k+1}} \ldots \frac{dv_m}{du_m}. \quad . \quad (2)$$

We may now apply this general formula to the former one. Instead of $u_1 \ldots u_m$ let us put the $2n+1$ Hamiltonian independents $q_1 \ldots q_n, q'_1 \ldots q'_n \mathrm{E}$; for $v_1 \ldots v_k$ let us put $q_1 \ldots q_n$; but for $v_{k+1} \ldots v_m$ let us put $p_1 \ldots p_n \tau$. Then equation (2) becomes

$$dq_1 \ldots dq_n\, dp_1 \ldots dp_n\, d\tau =$$

$$dq_1 \ldots dq_n\, dq'_1 \ldots dq'_n\, d\mathrm{E} \Sigma \pm \frac{dp_1}{dq'_1} \ldots \frac{dp_n}{dq'_n} \frac{d\tau}{d\mathrm{E}}. \quad . \quad . \quad (3)$$

Let us now in the generally-applicable formula (2) introduce other special values, viz. for $u_1 \ldots u_m$ the Hamiltonian independents again, but substitute $q'_1 \ldots q'_n$ for $v_1 \ldots v_k$, whilst for $v_{k+1} \ldots v_m$ we substitute the variables $p'_1 \ldots p'_n \tau$; which indeed, according to Hamilton's method, are also functions of the independents introduced by him; consequently equation (2) is applicable to this case just as much as to the former. Equation (2) becomes by this substitution,

$$dq'_1 \ldots dq'_n\, dp'_1 \ldots dp'_n\, d\tau =$$

$$dq'_1 \ldots dq'_n\, dq_1 \ldots dq_n\, d\mathrm{E} \Sigma \pm \frac{dp'_1}{dq_1} \ldots \frac{dp'_n}{dq_n} \frac{d\tau}{d\mathrm{E}}. \quad . \quad . \quad (4)$$

The reader is advised to write down the functional determinants of equations (3) and (4) at length, and then for each member of the functional determinant of equation (4) to substitute the value which it would have according to equation (1). We shall then have, except for sign and for an exchange of horizontal and vertical lines, exactly the functional determinant of equation (3). The two functional determinants have therefore the same numerical value; and since we are here concerned simply with this, and in the equations (3) and (4) the products of the differentials of the right sides are identical, it follows from these equations that

$$dq_1 \ldots dq_n\, dp_1 \ldots dp_n d\tau = dq'_1 \ldots d'q_n\, dp'_1 \ldots dp'_n d\tau.$$

Dividing each side by $d\tau$, we obtain

$$dq_1 \ldots dq_n\, dp_1 \ldots dp_n = dq'_1 \ldots dq'_n\, dp'_1 \ldots dp'_n. \quad . \quad (5)$$

which equation expresses Boltzmann's theorem in its fullest generality*.

In this equation the old independents $q'_1 \dots q'_n\ p'_1 \dots p'_n \tau$ appear again. Since we divided by $d\tau$, and consequently $d\tau$ appears no more in the equation, this is equivalent to saying that the time of the whole motion is to be regarded as a constant. On the other hand, all the coordinates and momenta holding good for the instant of commencement of motion (*i. e.* all the quantities $q'_1 \dots p'_n$) are to be increased by infinitely small amounts. The values $q_1 \dots p_n$ of the coordinates and momenta at the time τ will therefore also undergo infinitely small increase; and, according to equation (5), the product of the first must be put equal to the product of the latter increments; consequently, if we choose the old independents, we must have

$$\Sigma \pm \frac{dq_1}{dq'_1} \dots \frac{dp_n}{dp'_n} = 1.$$

For the sake of a clear view of the meaning of equation (5), let us imagine, instead of one system S, an infinitely large number of exactly similar systems S. Let the law of action of the forces be precisely the same for all the systems (of course without any two systems having any action upon each other). Let the duration of motion τ be exactly the same for all the systems—but the conditions of the systems at the instant of commencement of motion not the same for all the systems, but having at the instant of commencement of motion the values of coordinates and momenta between the limits q'_1 and $q'_1 + dq'_1 \dots p'_n$ and $p'_n + dp'_n$ for all the systems. Then also at the instant at which the motion ends the conditions of all the systems will not be the same, and coordinates and momenta may lie between the limits q_1 and $q_1 + dq_1 \dots p_n$ and

* In Watson's excellent book, 'A Treatise on the Kinetic Theory of Gases' (Clarendon Press, 1876), p. 13, there is an error, or at least an inaccuracy of expression, in the derivation of this equation. In the partial differential quotients of the functional determinant, at the head of that page, besides p and P, the time τ of the motion is to be regarded as an independent variable; but the equation following from this,

$$\frac{dq_r}{d\mathrm{P}_s} = -\frac{d^2\mathrm{A}}{dp_r\, d\mathrm{P}_s} = -\frac{dQ_s}{dp_r},$$

only holds good when E is variable independently of p and P. Consequently, in forming the partial differential quotients of this equation, E is to be regarded as constant: in forming those of the functional determinant, τ is to be regarded as constant: and the applicability of an equation holding good between the first partial differential quotients to the latter requires still to be proved.

$p_n + dp_n$. Then equation (5) holds good between the products of differentials.

V is a function of the coordinates determined by the constitution of the system; so also T is a function of the momenta or of the momenta and coordinates determined by the constitution of the system. Therefore also $E = V + T$ is a function of coordinates and momenta $F(q_1 \dots p^n)$ given by the constitution of the system. If we imagine the variables replaced by their values at the time τ, then E appears also as a function of these values, which we have also denoted by $q_1 \dots q_n$. E may therefore be introduced in the product $dq_1 \dots dp_n$ of equation (5) in the place of one of the variables, e. g. p_1; so that we obtain

$$dq_1 \dots dp_n = dq_1 \dots dq_n\, dp_2 \dots dp_n\, dE \div \frac{dF(q_1 \dots p_n)}{dp_1}. \quad (6)$$

This magnitude E, the total energy of the system, is obtained also by substituting in the function F for $q_1 \dots p_n$ their values $q'_1 \dots p'_n$ at the commencement of the time. Then E appears expressed as a function of $q'_1 \dots p'_n$, and may be introduced in the product $dq'_1 \dots dp'_n$ of equation (5) instead of p'_1, which gives us

$$dq'_1 \dots dp'_n = dq'_1 \dots dq'_n\, dp'_2 \dots dp'_n\, dE \div \frac{dF(q'_1 \dots p'_n)}{dp'_1}. \quad (6a)$$

If q_1 be expressed as a function of the old independents $q'_1 \dots q'_n \tau$, then $\frac{dq_1}{d\tau}$ is the differential quotient of q_1 in the usual sense, by allowing the time to increase without altering otherwise the initial conditions $(q' \dots p'_n)$. Maxwell denotes it by $\dot{q}_1$. It is of course also a function of $q'_1 \dots p'_n \tau$. Let its value when $\tau = 0$ be $\dot{q}'_1$; then, according to Hamilton,

$$\dot{q}_1 = \frac{dE}{dp_1} = \frac{dF(q_1 \dots p_n)}{dp_1}, \quad \dot{q}'_1 = \frac{dF(q'_1 \dots p'_n)}{dp'_1}.*$$

Substituting the values (6) and (6*a*) in equation (5) and dividing by dE, we obtain

$$\frac{dq_1 \dots dq_n\, dp_2 \dots dp_n}{\dot{q}_1} = \frac{dq'_1 \dots dq'_n\, dp'_2 \dots dp'_n}{\dot{q}'_1} \quad \dots (7)$$

(dE does not occur here). The equation admits of the following interpretation. Let there be given an infinite number of similarly constituted systems S. Let the time of the entire motion have for all exactly the same value τ, and the total energy exactly the same value E. Let the values of the

* Compare Thomson and Tait, new edition, § 318, equation (30).

variables $q_1 \dots q_n, p_2 \dots p_n$ at the beginning of the times be between the limits

$$\left.\begin{aligned} &q'_1 \text{ and } q'_1+dq'_1 \dots q'_n \text{ and } q'_n+dq'_n, \\ &p'_2 \text{ and } p'_2+dp'_2 \dots p'_n \text{ and } p'_n+dp'_n, \end{aligned}\right\} \quad . \quad (8)$$

for all the systems, while p_1 is determined by the equation of energy. If, further, we denote the limits between which coordinates and momenta lie at the moment at which motion ends by

$$\left.\begin{aligned} &q_1 \text{ and } q_1+dq_1 \dots q_n \text{ and } q_n+dq_n, \\ &p_2 \text{ and } p_2+dp_2 \dots p_n \text{ and } p_n+dp_n, \end{aligned}\right\} \quad . \quad . \quad . \quad (9)$$

then again equation (7) must hold between the products of differentials.

Maxwell employs now a method which he calls the statistical. He assumes we have a large number N of systems such as S given, having all exactly the same energy E, but whose coordinates and momenta at the commencement of motion have all possible values. He proposes to himself the problem to investigate, not how coordinates and momenta change for each of these systems with the time, but how many systems at a given time "have the phase (pq)"—*i. e.* for how many the coordinates and momenta lie between the limits (9).

p_1 is always determined by the equation of energy. The number of systems which at the time τ "have the phase (pq)" Maxwell denotes in general by

$$Nf(q_1 \dots q_n, p_2 \dots p_n \tau)\, dq_1 \dots dq_n\, dp_2 \dots dp_n. \quad . \quad (10)$$

The number of systems which, at time 0, have the phase $(p'q')$, *i. e.* for which the variables at this time lie between the limits (8), will consequently be denoted by

$$Nf(q'_1 \dots q'_n, p'_2 \dots p'_n 0)\, dq'_1 \dots dq'_n\, dp'_2 \dots dp'_n. \quad . \quad (11)$$

But, in accordance with the signification already given to $q_1 \dots p_n$ and $q'_1 \dots p'_n$, exactly the same systems have the phase (pq) at the time τ which had the phase $(p'q')$ at the time 0. The expressions (10) and (11) are therefore equal; whence, referring to equation (7), we have

$$\dot{q}_1 f(q_1 \dots q_n, p_2 \dots p_n \tau) = \dot{q}'_1 f(q'_1 \dots q'_n, p'_2 \dots p_n 0). \quad . \quad (12)$$

Maxwell calls the distribution of the system stationary when the number of systems having any given phase, *e. g.* $(p'q')$, does not change with the time—when, therefore, for any $q'_1 \dots q'_n, p'_2 \dots p'_n$,

$$f(q'_1 \dots q'_n, p'_2 \dots p_n \tau) = f(q'_1 \dots q'_n, p'_2 \dots p'_n 0) \quad . \quad (13)$$

Since in equation (12) $q'_1 \dots p'_n$ are also any initial values

of the variables whatever, equations (12) and (13) may be at once combined with each other, and give

$$\dot{q}_1 f(q_1 \dots q_n, p_2 \dots p_n \tau) = \dot{q}'_1 f(q'_1 \dots q'_n, p'_2 \dots p'_n \tau)^*.$$

Since f no longer contains the time τ, it is better to omit τ from under the functional sign and to write

$$\dot{q}_1 f(q_1 \dots q_n, p_2 \dots p_n) = \dot{q}'_1 f(q'_1 \dots q'_n, p'_2 \dots p'_n). \quad . \quad (14)$$

Here $q'_1 \dots q'_n, p'_2 \dots p'_n$ are any initial values whatever; $q_1 \dots q_n, p_2 \dots p_n$ are the values of coordinates and momenta which a system starting from these initial values attains after a time τ, in other respects unfixed.

Let us therefore imagine a system starting from any initial values of coordinates and momenta; then in course of the motion it will assume continually new and new values of coordinates and momenta. The coordinates and momenta are therefore functions of the initial values and of the time. But there will be in general certain functions of coordinates and momenta which have constant values during the whole motion, as in a free system the component velocities of the centre of gravity, or the sums of angular momenta, are invariable. Let us therefore imagine in the expression $\dot{q}_1 f(q_1 \dots q_n, p_2 \dots p_n)$ first of all those optional initial values from which each system started, then continuously the values in order which coordinates and momenta assume for that system as the time increases; then for the existence of stationary distribution it is necessary and sufficient that the value $\dot{q}_1 f$ shall remain unaltered, or, in other words, $\dot{q}_1 f$ must contain only such functions of $q_1 \dots p_n$ as remain constant during the whole motion of a system from any initial values whatever, and consequently are dependent on the initial values, but not on the time which has elapsed. If the system is so constituted that its coordinates and momenta, starting from given initial values, assume in the course of a sufficiently long time all possible values consistent with the equation of energy, then $\dot{q}_1 f$ must in general have the same value for all coordinates and momenta consistent with the equation of energy—must therefore be a constant.

I will now mention some other terms employed by Maxwell. If one of the systems S starting from a given initial condition moves, all the conditions through which it passes in consequence of its motion as time increases, constitute the

* This or the identical equation (14) is necessary that the distribution may be stationary. It is also sufficient: for from it and equation (12) equation (13) follows at once for any $q'_1 \dots p'_n$ whatever, which is exactly the mathematical expression for a stationary distribution.

path of the system, each separate condition of motion a *phase* of this path. All the functions of coordinates and momenta which remain constant during the whole path he calls the parameter characteristic of the nature of the path, whilst all other functions of coordinates and momenta depend also on the phase. In order that the distribution of the systems shall be a stationary one, it is necessary and sufficient that f shall be equal to $\frac{1}{q_1}$, multiplied by an arbitrary function of the parameters characteristic of the nature of the paths.

Maxwell considers the simplest case when this function is a constant, and therefore $f = \frac{C}{q_1}$; then

$$\frac{NC dq_1 \dots dq_n dp_2 \dots dp_n}{q_1} \quad . \quad . \quad . \quad . \quad (15)$$

is always the number of systems for which coordinates and momenta lie between the limits (9), whilst p_1 is determined by the equation of energy.

This is, then, the simplest possible stationary distribution. If the q's denote the rectangular coordinates of material points, then the products of the component velocities into the corresponding masses $m_1 u_1$, $m_1 v_1 \dots$ are the corresponding momenta; then the kinetic energy

$$T = \tfrac{1}{2}(m_1 u_1^2 + m_1 v_1^2 + \dots) = \frac{p_1^2}{2m_1} + \frac{p_2^2}{2m_1} + \dots,$$

where evidently $\frac{m_1 u_1^2}{2}$ is the kinetic energy resulting from the motion of the first atom in the direction of the axis of x, and so on. In like manner, generalized coordinates can always be so transformed that

$$T = \frac{\gamma_1 p_1^2}{2} + \dots \frac{\gamma_n p_n^2}{2},$$

where the γ's contain simply the coordinates. Maxwell calls $\frac{\gamma_r p_r^2}{2}$ the "kinetic energy resulting from the rth momentum," or simply the "kinetic energy of the rth momentum" The mean kinetic energy of any one of the momenta, say of the rth momentum, is therefore expressed by

$$\frac{\int\int \dots \gamma_r p_r^2 dq_1 \dots dp_n}{2q_1} \div \frac{\int\int \dots dq_1 \dots dp_n}{q_1}.$$

Here the integration is effected with reference to all the other p's before that with reference to p_r.

I will here show only how the integration with reference to p_n is to be effected when r is not equal to n. For q_1 is to be substituted its value

$$\frac{d\mathrm{T}}{dp_1} = \gamma_1 p_1 = \sqrt{2\gamma_1}\sqrt{\mathrm{E} - \mathrm{V} - \frac{\gamma_2 p_2^2}{2} \ldots - \frac{\gamma_n p_n^2}{2}}. \quad . \quad (16)$$

If we consider that, in the integration with reference to p_n, the quantities $q_1 \ldots q_n, p_2 \ldots p_{n-1}$, and therefore also $\gamma_1 \ldots \gamma_n$, V, and p_r are to be considered as constant, we may put

$$\mathrm{E} - \mathrm{V} - \frac{\gamma_2 p_2^2}{2} \ldots - \frac{\gamma_{n-1} p_{n-1}^2}{2} = a, \text{ and } \frac{\gamma_n p_n^2}{2} = x;$$

then, in integrating with reference to p_n, all up to q_1 comes before the sign of the integral, and $\int \frac{dp_n}{q_1}$ reduces to

$$\frac{1}{\sqrt{\gamma_1 \gamma_n}} \int_{-\sqrt{a}}^{+\sqrt{a}} \frac{dx}{\sqrt{x}\sqrt{a-x}},$$

which, as is well known, can be easily calculated. The integration can be equally easily performed with reference to the remaining p's, and lastly to p_r. Since V is a given function of the coordinates, the mean kinetic energy may be found simply by repeated integration.

The symmetry of the formula (16) shows at once that it has the same value for all momenta, consequently also for all atoms in the case of material points. The number Z_1 of the systems for which the values of coordinates lie between q_1 and $q_1 + dq_1 \ldots q_n$ and $q_n + dq_n$, and the kinetic energy of the momentum p_r between k and $k + dk$, whilst all the other momenta have all possible values, is found by integrating the expression (15) with reference to those other momenta, but putting $\sqrt{\frac{2k}{\gamma_r}}$ and $\frac{dh}{\sqrt{2k\gamma}}$ for p_r and dp_r; the number Z_2 of the systems for which, whilst keeping the conditions for the coordinates, the last momentum may be any we like, by integrating also with reference to p_r or k over all possible values.

The integration, after using the substitution (16), offers no difficulty, and gives

$$Z_2 = \frac{NC[\Gamma(\frac{1}{2})]^n \sqrt{\gamma_1 \gamma_2 \cdots \gamma_n}(2E-2V)^{\frac{n-2}{2}} dq_1 \ldots dq_n}{\Gamma\left(\frac{n}{2}\right)} *,$$

$$\frac{Z_1}{Z_2} = \frac{(E-V-k)^{\frac{n-3}{2}} dk \Gamma\left(\frac{n}{2}\right)}{(E-V)^{\frac{n-2}{2}} \sqrt{k} \Gamma(\frac{1}{2}) \Gamma\left(\frac{n-1}{2}\right)}.$$

This number, and consequently the law of distribution of kinetic energy, has the same value for all momenta. For large values of n,

$$\frac{Z_1}{Z_2} = \frac{e^{-\frac{k}{2K}} dk}{k\sqrt{2\pi}} \text{ nearly,}$$

where $K = \frac{E-V}{n}$ is the mean value of the kinetic energy of a momentum corresponding to these values of coordinates, and the same for all momenta.

In order to apply these equations to the theory of Heat, Maxwell imagines amongst the systems S precisely similarly constituted warm bodies enclosed in absolutely rigid envelopes impermeable to heat, which are completely independent of each other, and all possess the same energy E. The systems S therefore now represent to us very many similarly constituted real bodies of equal temperature and under equal external conditions. The condition of motion of each of these bodies is to be determined by the coordinates and momenta $q_1 \ldots p_n$ formerly employed. The different bodies are to have started from very different initial conditions; and the number of systems for which, at the commencement of the time, coordinates and momenta lay between the limits (9), is to be given by the formula (15). We know that then the distribution is a stationary one. The systems which had the phase (pq) at the commencement of the time, it is true, soon pass out of this phase; but exactly as many systems enter on this phase to replace them, and thus it continues for all times. The equations obtained above hold, therefore, for all bodies. The mean kinetic energy must have the same value for each of the momenta, viz. the value calculated above. The case might, of course, occur that the equations should not hold good for each

* When V is small with reference to E, and n is large, $(E-V)^{\frac{n-2}{2}}$ approaches to the limit $E^{\frac{n-2}{2}} e^{-\frac{nV}{2E}}$, and the hydrostatic differential equation for polyatomic gases follows from the equation in the text.

single body—that, for example, the mean kinetic energy of a momentum should be greater in one body than that calculated above, in which case it must of course be smaller again in other bodies, so that we may have the true mean value for all bodies.

But it is to be remembered that all our bodies are found similarly constituted, of equal temperature, and under similar external conditions. In the case just spoken of, therefore, the behaviour of bodies of that kind would be different according to the initial condition from which they started. But this is not confirmed by experience. As often as one and the same body is left to itself with the same energy of motion and under the same external conditions, it assumes with time the same thermal condition, the stationary condition corresponding to that temperature and those external conditions. We are therefore justified in maintaining that our equations hold not simply for the above-defined conceptions of bodies, but also for the stationary final condition of each single warm body. That the condition of equality of temperature between warm bodies has a very simple mechanical meaning independent of their initial conditions, follows also from the fact that it is not influenced by the compression, turning, or displacement of particular parts.

If we substitute for the system S two different gases separated by a solid division-wall permeable to heat, then there follows the equality of the mean kinetic energy of progressive motion of the molecules of both gases, or Avogadro's law; the proof of which, hitherto resting on the equality of this mean kinetic energy in mixtures of gases, is unreliable, since we are not able to show that the mean kinetic energy of progressive motion is the same in mixtures as in separate gases at the same temperature.

The second case discussed by Maxwell is very interesting, but cannot be here reproduced in full. In this $q_1 \dots q_n$ are the rectangular coordinates $x_1 \dots z_n$, therefore $p_1 \dots p_u$ the velocity-components multiplied by the masses $m_1 u_1 \dots m_n w_n$ of a free system of atoms S′ with any internal forces but without external forces. Maxwell introduces into equation (5), instead of $du_1\, dv_1\, dw_1\, du_2\, dv_2 dw_2\, du_3$, the product $d\mathrm{U}\, d\mathrm{V}\, d\mathrm{W}\, d\mathrm{F}\, d\mathrm{G}\, d\mathrm{H}\, d\mathrm{E}$; where U, V, and W are the velocity-components of the centre of gravity, F, G, and H the constant sums of angular momenta of the elements of motion of the system S′. Equation (5), therefore, after dividing by $\dfrac{d\mathrm{U}\, d\mathrm{V}\, d\mathrm{W}\, d\mathrm{F}\, d\mathrm{G}\, d\mathrm{H}\, d\mathrm{E}}{m_1^3 m_2^3 m_3}$, assumes the form

$$\frac{dx'_1 \dots dz'_n\, dv'_3 \dots dw'_n}{a' r' \dot{r}'} = \frac{dx_1 \dots dz_n\, dv_3 \dots dw_n}{a r \dot{r}},$$

where r is the distance of the atoms m_1 and m_2, $\dot{r}=\frac{dr}{dt}$; α is the double of the projection of the triangle $m_1 m_2 m_3$ on the yz-plane. If we have again an infinitely great number of similarly constituted systems S′ given for which the magnitudes E, U, V, W, F, G, H have equal values throughout, we find, exactly as before, that the distribution of these systems is stationary when the number of those for which $x_1 \ldots z_n, r_3 \ldots w_n$ lie between the limits x_1 and $x_1+dx_1 \ldots w_n$ and w_n+dw_n, is equal to $\frac{C dx_1 \ldots dw^n}{\alpha r \dot{r}}$. Maxwell calculates, exactly as in the former case, the number of systems for which, with any velocities, the coordinates of the atoms lie between infinitely close limits, and, further, the number of those for which the velocity-components of an atom also lie between infinitely close limits, the mean kinetic energy of an atom, &c. I quote two only of the results.

1. If ξ, η, ζ be the velocity-components of an atom of mass m referred to new axes of coordinates, of which, at the instant in question, the z-axis passes through the atom, but the two others are axes of the section of the momental ellipsoid by their plane, whose origin at each instant is the centre of gravity of the system, and which revolve with the angular velocities which the system acquired in suddenly becoming solid through the operation of internal forces, then the mean values of the magnitudes $m\zeta^2$, $\frac{m\xi^2}{1-a\gamma z^2}$, $\frac{m\eta^2}{1-b\gamma z^2}$ are the same for all atoms. In particular, the law according to which these magnitudes are distributed amongst the atoms is the same as that according to which mu^2, mv^2, mw^2 were distributed in the former case. At the same time $\gamma=\frac{Mm}{M-m}$; if M is the total mass of the system, z is the distance of the centre of gravity of the system from a straight line passing through the atoms whose direction-cosine, with reference to the new axes of coordinates, are proportional to ξ, η, and ζ. Lastly, $a=\frac{BC-L^2}{D}$, $b=\frac{AC-M^2}{D}$ if A, B, C are the moments of inertia of the system with reference to the new axes of coordinates, L, M, N the sums Σmyz, Σmxz, Σmxy with reference to the system of coordinates, and

$$D=\begin{vmatrix} A, & -N, & -M \\ -N, & B, & -L \\ -M, & -L, & C \end{vmatrix}$$

If the number of atoms is very large, then still $\frac{m(\xi^2+\eta^2+\zeta^2)}{}$,

consequently the mean kinetic energy of internal motion (*i. e.* of that relative to the new axes of coordinates), is the same for the atoms.

2. A gaseous mixture distributes itself in a horizontal tube rotating about a vertical axis, exactly as if each of its constituents were present alone in equilibrium under the action of gravity and of centrifugal force. A tube 1 metre (l) long, with one end in the axis of rotation, must make about ten (n) revolutions per second in order that a mixture of hydrogen and carbon dioxide shall contain at one end 1 per cent. (p) carbon dioxide more than at the other. The rotation must last about two hours in order that the previous deviations from a stationary distribution shall become about one hundred times smaller; p is proportional to the square of the velocity of the moving end of the tube, and therefore to l^2n^2.

XXXV. *Notes on Physiological Optics.*

By W. Le Conte Stevens*.

IN the 'Philosophical Magazine' for May 1882 the present writer discussed certain phenomena of vision under variable physiological conditions. Among these was stereoscopy, attained from a pair of perfectly similar diagrams, with parallelism or slight divergence of visual lines, the binocular resultant image being caused to appear concave, convex, or plane at will, by properly adjusting the cards in position so that the two retinal images from them could be made either slightly dissimilar or alike. A geometric discussion of this was given in connexion with the record of other experiments that illustrated the important effect of muscular action in modifying our unconscious interpretation of retinal sensations. This discussion was preceded by a consideration of the current theory of corresponding retinal points, which was accepted only in a modified sense, and not mathematically. It was assumed that, in examining the binocular resultant, freedom of motion is allowed the eyes—a condition that has usually been found necessary when stereoscopy by this method is performed for the first time by any one who is not skilled in binocular experiments. Even at that date the writer was convinced that play of the eyes was not indispensable, however effective it might be in confirming the visual judgment. The geometric discussion, though correct so far as it extends, was not deemed capable of covering all the facts; but to test the extent to

* An Abstract from two Papers read before the American Association for the Advancement of Science at the Montreal meeting in August 1882. Communicated by the Author.

which it was possible to attain such results without motion of the eyes, it was important to employ the electric spark as a means of illumination. The opportunity of doing so was then wanting, but has since been secured.

Vision by the Light of the Electric Spark.

The apparatus employed for the production of momentary illumination was a large induction-coil belonging to the Physical Laboratory of Columbia College (New York), and loaned for the purpose by Professor O. N. Rood. The stereoscope used was the reflecting instrument described in a former paper*, which had been so constructed as to give for registration the angle, positive or negative, between the observer's visual lines, the distance of each card from the eye that receives its image, and the angle which the plane of this card makes with the visual line, assuming the latter to be horizontal and the axis of rotation of the card to be vertical. The writer was fortunate in securing the cooperation of Mr. W. W. Share, Assistant in Physics in Columbia College, who soon acquired more than usual skill in the control of his eyes for binocular experiments.

In the dark room the stereoscope was first so arranged that parallelism between the two visual lines was necessitated, in obtaining binocular vision of the pair of pictures at the moment these were equally illuminated by the passing of a spark The plane of each card being perpendicularly across the supporting arm of the reflector, the binocular resultant presented the appearance of a series of concentric circles on a flat surface. By rotating each card through a known angle on its vertical axis, the binocular resultant could be made to assume at will the form of a convex or concave elliptic shield. The observer was seated with closed eyes in front of the stereoscope while the manipulator of the apparatus arranged the cards. The observer, not knowing whether this arrangement would produce planeness, convexity, or concavity, was then invited to open his eyes and interpret the binocular retinal sensation attained by the illumination of the cards with a single spark. It was found possible, nearly always, to make a correct interpretation at the first trial. Mr. Share and the writer acted each alternately as observer and manipulator; and the result attained was confirmed by the experience of Professor Rood, who tried the same experiments independently.

The distance and diameter of the circle on each card being known, and also the angle of rotation on its vertical axis, it becomes possible to calculate the maximum retinal displace-

* Philosophical Magazine, December 1881.

ment of images which would have corresponded retinally if the angle of rotation, ϕ, were zero. The attention being specially given to the centre of the binocular concave or convex resultant, the illusion of binocular unity and depth in the picture remained possible when the retinal displacement corresponding to marginal portions of the combined image was as great as ·39 millim., or more than 80 times the diameter corresponding to what has been estimated to be the *minimum visibile*. By giving attention, through indirect vision, to the marginal portions, the illusion of binocular unity was easily destroyed, and double images at once became detectable. The result was confusion and loss of the third dimension in space at these marginal portions, while the perception remained clear for central portions where no duplication could be perceived. These effects were noticed by both Mr. Share and the writer.

The pictures found best in these experiments were concentric circles consisting of broad black bands on a white ground, or of white bands on a black ground. Various other stereographs were employed, many of them constructed for the production of stereoscopic relief, which could be reversed or totally suppressed by appropriate arrangement of the cards on the arms of the stereoscope. The peculiar nature of the relief, whether direct or reversed, was what the observer was requested to ascertain, and with satisfactory results, usually without delay. The most difficult case was that in which one picture consisted of a red diagram on a green ground, the other a green diagram of the same size on a red ground.

A series of experiments, continued through many days, was tried under illumination with the electric spark, by Mr. Share and the writer jointly, to test still further the effect of muscular strain in modifying the unconscious interpretation of the binocular retinal image as discussed in a former paper†. The optic angle was varied from 3° of divergence to 50° of convergence of visual lines, while the stereograph of the moon was again employed, being kept at a fixed distance on the arms of the stereoscope while the observer, under the abnormal conditions imposed, was requested to form an estimate of apparent distance and diameter. Each acted as manipulator and recorder for the other, the observer being kept ignorant of his own record until the whole series of experiments was completed. The result was in each case quite similar to that formerly obtained with vision by continuous light; but the limit of error was much wider, showing that under such unusual conditions no single visual judgment is worthy of any confidence. The general effect on each, however, was that

* Philosophical Magazine, December 1881.

strain of the internal rectus and ciliary muscles produces the illusion that the object perceived is smaller and nearer.

The above is a mere statement of facts. Any discussion they may suggest is reserved for a future paragraph.

The Binocular Union of Spectral Images.

If a sharply defined object be momentarily illuminated by the intense light of the electric spark, a positive after-image is perceived and quickly followed by a negative image of short duration. If the gaze be very steadily fixed upon one point of an object that is strongly illuminated by the direct rays of the sun, the eyes being at the same time protected from the glare proceeding from surrounding objects, a negative after-image is obtained that lasts several minutes. Since its existence is due to fatigue of the retina in certain parts while others remain unfatigued, such an image appears always in the direction of the visual line, changing in apparent position with every motion of the eye.

The late Professor W. B. Rogers, of Boston, published in 1860* some experiments on the binocular union of after-images from illuminated lines so arranged as to produce the appearance of relief. Perspective after-images were likewise obtained by Wheatstone and by Wundt; but an objection to conclusions drawn from such perceptions as these consists in the fact that the observer knows what effects *would* result in direct vision under the conditions imposed; indeed he simply retains a subjective perception of what he has just seen binocularly. It is difficult to determine how far the perception may be due to imagination rather than to immediate retinal sensation. Professor Rogers succeeded in attaining perspective after-images even when the luminous lines were regarded successively instead of together; but thus far no one else seems to have confirmed this result; and the experiment is still liable to the objection that the visual judgment is warped by anticipation and association. Experiments therefore have lately been made with a view to testing these results, and at the same time to ascertain whether any modification would be imposed by varying the muscular conditions under which the spectral images are seen.

1. Across the median plane of vision was held a card with the upper edge more remote than the centre, so that a white band from top to bottom on a dark background was inclined about 40°. This was fixedly regarded with each eye sepa-

* Proceedings of the American Association for the Advancement of Science, 1860, p. 187 *et seq.*

rately in succession, while held in direct sunlight, until both retinas were fatigued. On going then into a slightly darkened room, the inclined spectral image was easily perceived, apparently in mid-air. On making the visual lines parallel, it became projected on the wall, but without losing its obliquity. On strongly contracting the internal rectus muscles, it appeared still directly in front, but much smaller and nearer. The experiment was repeated many times, and varied, but with uniform results.

2. On separate cards a pair of diagrams were constructed in such a manner as to produce an image in relief when binocularly viewed, in the stereoscope or otherwise. These were separately and successively regarded in sunlight, each with the appropriate eye. In the dark room the resultant after-image appeared in mid-air in clear relief. On shutting one eye, the component image that remained visible to the other was at once projected upon the wall as a flat picture. Strongly contracting the ciliary muscle of the eye remaining open, without sensibly contracting the rectus muscles, the picture was made to approach and grow apparently smaller, in almost as marked a degree as by the previous experiment.

3. A series of concentric black and white circular bands was constructed on a card, which was held in a vertical plane obliquely crossing the horizontal visual line of the left eye. After the retina had become fatigued, the same card was held across that of the right eye, but with opposite obliquity, so that the distortions of the elliptic images on the two retinas should be opposite in sense. Each eye was closed while the other was receiving light from the card. The resultant spectral image was concave instead of plane, and presented the same variations with change of muscular conditions as in previous experiments.

4. To ascertain whether these perspective stereoscopic effects were due to imagination and association, or whether they were the immediate outcome of retinal sensation, from the existence of dissimilar images remaining through fatigue in the two eyes, it was necessary to test some one whose eyes were normal, but who was ignorant regarding the nature of the visual effects to be produced, and who therefore could not be influenced by anticipation. It was found possible to enlist the interest of a youth of good general intelligence, who was entirely unacquainted with even the elementary principles of binocular vision. He submitted to be trained until he could secure monocular after-images successfully with either eye at will. Without granting him the slightest clue by which results could be anticipated, the writer employed a pair of cards on

which were diagrams so arranged that the binocular resultant could be made either a raised cone, a flat picture, or a hollow cone, according to the mode of combination selected. These cards were viewed in sunlight, never binocularly, but always separately and in succession, the relation between the pictures being varied in successive experiments. As soon as the retinas were fatigued, the observer was led into a perfectly dark room, and requested to describe the resultant spectral images perceived. Without allowing him ever to know whether his visual judgments were right or wrong, these experiments were repeated day after day, until the youth's own conclusions were definitely formed by repeated interpretation of his retinal sensations. His judgments were in the majority of cases correct, during the latter part of the time invariably so; and by spectral images alone he learned what should be the proper arrangement of pictures to produce a binocular resultant that was concave or convex at will. The cards with concentric circular bands were then substituted; and in like manner he soon learned what kind of obliquity should be given the plane of each card in order to produce a concave or convex spectral binocular image immediately afterwards. His eyes were not sufficiently trained to enable him to test the effect of varying the tension in either ciliary or rectus muscles, nor was he able to perceive duplication in any part of any binocular spectral image.

5. A pair of diagrams were constructed in such manner as to show very plainly the binocular duplication of central parts in the background when the foreground was regarded and the gaze was monocularly directed to the centre of each in succession, with the usual precautions. The spectral image presented the appearance of relief. By an effort of special attention the duplication of the background became perceptible; but at the same moment the appearance of relief was lost.

Results from the Experiments just described.

These experiments, in conjunction with those made by the light of the electric spark, show that in the new mode of stereoscopy play of the eyes is by no means necessary, although it constitutes an important aid in all cases where a clear visual judgment is not attainable at the first glance. They show also very conclusively that the conscious perception of double images in the binocular field of view, on which so much stress was laid by Sir David Brewster*, far from being conducive to clearness of binocular perception, tends rather to interfere

* Brewster, 'The Stereoscope,' p. 76 *et seq.*

with it. If it be said that we unconsciously perceive them and intuitively distinguish between the two kinds, homonymous and heteronymous, this conclusion cannot be confirmed or disproven, except so far as experiments like those just detailed may be accepted as having some bearing upon the subject. The writer's disposition is to discard intuition entirely, and, with Helmholtz*, to regard the degree of attention bestowed upon objects pictured at the same moment on different parts of the two retinas as an element of more importance than either play of the eyes or the perception of double images. The point in the field of view to which most attention is habitually given is that pictured upon corresponding retinal parts; but the attention is at the same moment divided, being given in less degree to many other parts of the field of view as simultaneously perceived with each eye. The mental suggestion due to the impression of non-corresponding parts is that of the third dimension in space. If this be called the perception of double images, their effect seems to be dependent upon their *not* emerging into consciousness. Add to this the fact that the gradation between single and double vision is wholly imperceptible, and hence that for infinitesimal departures from single vision there can be no demonstrable distinction between the two kinds of double images. In the interpretation of our sensations we are certain that experience is our habitual guide, though by no means always a reliable one. Whether intuition can be accepted as an additional guide at all, it is not easy to pronounce. The debate between the advocates of the empiristic and nativistic theories is doubtless like the well-known quarrel about a certain shield, and may be continued indefinitely. The domain of intuition is certainly far more limited than was thought a few generations ago; whether it can be reduced to zero may perhaps be decided a few generations hence. In all ordinary cases of binocular vision the effect is cumulative. The judgment quickly reached is a product not only of difference in the degree of attention given at the same moment to objects seen by direct and by indirect vision respectively, but also of variation in attention to different points directly viewed in succession, of the muscular sense while free play is given to the eyes, and of all the elements available in monocular vision, which have been grouped together under the name of physical in contrast with physiological perspective.

* Helmholtz, *Optique Physiologique*, p. 1009.

XXXVI. *Notices respecting New Books.*

A Treatise on the Transit Instrument as applied to the Determination of Time. For the use of Country Gentlemen. By Latimer Clark, *M.I.C.E. &c.* (Published by the author, 6 Westminster Chambers, London.) 72 pages text, with 29 pages Transit Tables.

THE object of this little work is to popularize the use of the portable Transit for finding correct time among amateurs of small means. About half the work, describing the instrument and its use, is written in a thoroughly popular style, as free as is possible from technical terms. Simple modes of adjusting in position are given; and the instrument is supposed to be used only in correct position, so that no "corrections" have to be computed. This is an admirable mode of use for a beginner: some preliminary personal instruction would, however, be required; the detail given is not enough for a person quite unused to instruments. The second part is intended for more advanced amateurs. In this some astronomical terms and usages are explained, and the mode of computing the "corrections" to transits observed with an instrument not in perfect adjustment are fully entered into.

The Tables (72 pages) contain the data for transits of the sun and certain stars for six months in a simple form very suitable for beginners; they form an appendix to the text, and are to be published anew yearly, thus saving the need of mastering the Nautical Almanac (itself rather a formidable work).

There is a misprint of $8^{s}\ 46'\ 28''$ for $8^{h}\ 46^{m}\ 28^{s}$ on p. 42, which may confuse a beginner. The accounts of the Polestar on p. 26 and p. 48 do not agree: on p. 26 it is said that it "is very close to the North Pole, and revolves round it daily at a distance of about $1\frac{1}{2}$ degree;" whilst on p. 48 it is said, "let us imagine ourselves at the North Pole. We should see the Polestar directly overhead remaining motionless." On the whole, however, the work may be said to fulfil well the purpose for which it was written, and will help to supply an amateur's wants. To meet the case of amateurs of small means, it is stated (in an advertisement) that an excellent portable transit with 14″ telescope and $1\frac{1}{2}$″ aperture can now be had for £8. Allan Cunningham, *Major R.E.*

Geology of Wisconsin. Survey of 1873–79. Vol. III. Large 8vo, 763 pages. With numerous Plates and other Illustrations, and an Atlas of Maps. Published under the Direction of the Chief Geologist [T. C. Chamberlin] by the Commissioners of Public Printing, in accordance with Legislative Enactment. [Madison.] 1880.

Volume II. of this excellent Survey, published in 1877, and noticed in this Journal for April 1880, p. 302, treated of the geology of the eastern, central, and south-western portions of the State of Wisconsin. In the volume before us the extreme north-western and north-eastern portions of the State are described. The

former area is bordered by Lake Superior, between Minnesota and Michigan; the latter, with a part of the iron district of Michigan annexed, is traversed by the Menominee River and some of its affluents.

Part I. of this volume consists of valuable observations by Professor Roland D. Irving on the "General Geology of the Lake-Superior Region," which is estimated at 70,000 square miles in area, with strata, probably more than 100,000 feet in thickness, including four great unconformable systems: and the whole is coated with enormous deposits of glacial drift, besides lake-alluviums. I. The Laurentian gneiss forms the crystalline nucleus of the region, and is continuous with that of Canada. With some associated and often gneissoid granite, these altered strata are greatly folded, and have a general southerly dip, with an E.–W. strike, and an enormous thickness. II. The Huronian rocks are 12,800 feet thick, and consist of (from below upwards):—1. crystalline tremolitic limestone and a partial quartzite, 130 feet: 2. quartz-schist, mica-schist, and some novaculite, 410 feet: 3. tremolitic magnetite-schists and irony quartzites (Penokee Iron Range), 780 feet; 4. black mica-slates, with diorite and schistose quartzites, &c., 3495 feet: 5. dark-grey mica-schists, with intrusive granite, 7985 feet. The dip is northward, and the strike is oblique to that of underlying Laurentians. III. The Keweenawan or Copper-bearing series succeeds, seven miles in thickness, and consists of distinctly stratified igneous rocks like great flows and ash-beds. The lower rocks, from 10,000 to 30,000 feet, are almost wholly augite-plagioclase—namely, diabase, melaphyr, and gabbro (Rosenbusch). Shales, sandstones, and conglomerates then come in, and, becoming more and more frequent, nearly exclude the igneous rocks for the uppermost 15,000 feet of the series. IV. Lying on the eroded surface of the Keweenawan strata is a great horizontal set of sandstones, defined as the "Lake-Superior Sandstone," and regarded as equivalent to the Potsdam Sandstone of the Mississippi valley.

In Part II. Professor Raphael Pumpelly gives the "Lithology of the Keweenawan System," specimens of which were sent to him, and represented Diabase, Melaphyr, Gabbro, Uralitic Gabbro, Uralitic Diabase, Augite-diorite, and Felsitic Porphyries.

Part III., by Prof. R. D. Irving, describes the "Geology of the Eastern Lake-Superior District" of Wisconsin, premising its Topography, with Altitudes, Drainage-system, Vegetation, and Soils. The lithology, stratification, and economics of each of the great systems are given in detail according to locality; also an account of the Glacial Drift and Lacustrine Clays (Champlain Series).

Part IV., by Mr. C. E. Wright, treats of the "Huronian Series West of Penokee Gap." The Penokee Iron Range is here especially described, with the details of method of examination. The magnetic bands being covered by Drift, their breadth and extent were defined by the use of the solar dial-compass and the dipping needle, both of which are succinctly described. The iron-ores are, it seems, nearly all poor or "lean," the good ore being probably high up in the series.

Part V., by Mr. E. T. Sweet, gives the topography, natural-history, geology, lithology, and economics of the "Western Lake-Superior District" of the State. Among the Quaternary deposits occur the Moraines and Pot-holes of the Kettle Range, similar to that of Eastern Wisconsin, described in the previous volume. In the Glacial Drift occur not unfrequently nuggets and boulders of native copper. Some of the granitic and gneissic boulders must have crossed Lake Superior and travelled at least 200 miles.

Part VI. consists of an account of the "Geology of the Upper Saint-Croix District," based on the Notes of the late Mr. Moses Strong, edited by Mr. C. T. Chamberlin. It treats of the Surface-features, the Quaternary formations, and the Older formations, both generally and in detail. This area is inland and south of the "Western Lake-Superior District."

The Menominee Region, including parts of both Wisconsin and Michigan, is described geologically and lithologically in Parts VII. and VIII. by Major Thomas Benton Brooks and Mr. E. T. Street. This being an important iron-district, overrunning the boundary of the two contiguous States, and Wisconsin not supplying any money for the Survey beyond its own border, Major Brooks completed the work at his own expense, and suffered serious illness also from his labours. Besides the Superficial Deposits of Drift &c., the country has:—1. The Calciferous sand-rock and limestone and the Saint-Mary's (Potsdam) sandstone, of the Lower Silurian; 2. None of the Copper-series: 3. The Upper-Huronian granite, gneiss, schists (hornblende, actinolite, mica, chlorite, and quartz), iron-ores, clay-slate, carbonaceous slate or graphitic shale, quartzite, and conglomerate. 4. Middle-Huronian clay-slate and quartzite. 5. Lower-Huronian dolomite, iron-ore, and quartzite. 6. Laurentian granite, gneiss, and crystalline schists. Three elaborate Tables of the rocks and their component minerals in the Menominee and Marquette Regions present a summary of the lithological characters of the several systems and series of rocks and of their relative abundance and stratigraphical order. The descriptive lithology of the Menominee rocks and of the Huronian rocks south of Lake Superior form two interesting chapters (Chapters 3 and 4) of Part VII.; and are followed by Dr. Arthur Wichmann's microscopical investigations in the Huronian rocks, prefaced with a technical account of the minerals composing the said rocks. Besides this eminent lithologist of Leipsic, others have aided in the microscopical lithology of Wisconsin as treated in this volume, namely E. Törnebohm, F. Zirkel, Herr Wapler, S. Allport, Frank Rutley, G. J. Brush, J. D. Dana, G. W. Hawes, A. A. Julien, T. Sterry Hunt, Prof. R. Pumpelly, T. B. Brooks, and C. E. Wright. Nine coloured plates of microscopic sections of rocks occur in the volume, and enhance the value of the lithological descriptions. The lithographic and chromolithographic views, maps, and sections, illustrating the topography, geology, and mining, are numerous (44); there are also 23 woodcuts, chiefly sections of strata. Above all, the magnificent Atlas of Plates XVII. to XXX. inclusive, giving

many sections besides the coloured geological maps, is to be noticed as a most useful adjunct to this liberally published Report on the geological structure and capabilities of important parts of the great State of Wisconsin.

The Life of Immanuel Kant. *By* J. H. W. Stuckenberg, *D.D., late Professor in Wittenberg College, Ohio.* London: Macmillan and Co. 1882.

The work before us does not answer completely to its title. Our notion of a *Life* involves in its essence the being written chronologically. To put every thing in its proper place with respect to time may be difficult in the case of a philosopher who flourished one hundred years ago and whose life was remarkably uniform; but the task so accomplished would be more interesting to the general reader and more valuable to a philosopher. For example, at the end of Chap. XI. we are landed in the "return" to Kant's philosophy, while in Chap. XIII. we are led back to consider the old age and death of the philosopher himself. Our author is in consequence apt to fall into redundancy, a danger of which he is himself conscious; for he says at the beginning of Chap. XII., devoted to Correspondence and Correspondents, "Kant's letters have already been so extensively used in this biography, that little more need be said of them." In all other respects the workmanship of the volume seems to us most praiseworthy.

Our author in his Preface says, "If Kant's works throw light on his life, it will also be found that his life aids materially in understanding his works." In the thirteen following chapters he aims at giving all the data which can by any possibility throw light on the views of the philosopher—scientific, moral, and religious. There are data given, however, which one would think cannot throw much light; for example, an account given of his method of retiring (p. 435), the nature of which may be inferred from the following specimen:—"In summer one, in winter two nightcaps were worn."

With this book before him, the scientific man will be able to appreciate the qualifications, natural and acquired, which Kant brought to the task undertaken in the 'Critique of Pure Reason.'

XXXVII. *Intelligence and Miscellaneous Articles.*

CONSERVATION OF SOLAR ENERGY.

BY PLINY EARLE CHASE, LL.D.*

ALL forms of solar energy are due to solar radiation. The maintenance of the energy depends on the maintenance of the radiations. In investigating the relations of centripetal and centrifugal action and reaction, it seems desirable to consider the following hypotheses and conclusions:—

* Abstract of a Paper read before the American Association at Montreal, August 25, 1882. Communicated by the Author.

1. Laplace's estimate that the velocity of transmission, in gravitating acceleration, if finite, must be at least 100,000,000 times as great as the velocity of light.

2. Le Sage's hypothesis that gravitation and luminous radiation represent equal actions and reactions.

3. Faraday's search for a gravitating constant.

4. Herschel's comparison of the mean *vis viva* of light with that of sound.

5. Weber's identification of the velocity of light (v_λ) with the "electromagnetic ratio" (v_ϵ).

6. Berthelot's "explosive waves," and their action upon sound-waves.

7. The inquiries of Siemens into the combined influence of rotation, centrifugal action, gravitating fall, and chemical affinity.

To these considerations the following may be added:—

8. If there is a natural unit of force, we may look for a natural unit of velocity.

9. Oscillations may be orbital, pendulous, or wave.

10. Different transformations of similar oscillations are harmonic.

11. Rotation may be regarded as a pendulous motion, due to retarded and modified revolution.

12. The resemblance of Le Sage's theory to the kinetic theory of gases points to a probability that the natural unit of velocity is oscillatory. This probability is strengthened if we assume the existence of molecular and intermolecular elasticity.

13. In looking to the activities of the principal mass in our system for indications of a natural unit of velocity, we find that gravitating velocities may be represented by gt.

14. In order that gt may be constant, t must vary inversely as g, and therefore directly as r^2. This variation is found in the rotation of a nebulous sphere, where it holds good for all stages of expansion or contraction which are not affected by external influence.

15. Gravitating acceleration should do its whole work in stellar rotation as well as in planetary revolution.

16. Particles exposed to solar superficial gravitating acceleration, during a single oscillation of half-rotation, would acquire a velocity which is equivalent to the velocity of light. If we designate this acquired velocity by v_γ, we have $v_\gamma = gt = v_\lambda$ as a gravitating constant, which gives the following extension to Weber's analogy: $v_\lambda = v_\epsilon = v_\gamma$. In other words, the unit of velocity which is indicated by solar gravitation is the same as is indicated by light and by electricity.

17. The velocity of light, like the velocity of sound, thus represents an elastic atmosphere whose height, if homogeneous, would be twice the virtual fall which would give the velocity in question, and whose elasticity is in harmonic accordance with solar rotation and planetary revolution.

18. Subsidence, from Laplace's limit of synchronous rotation and revolution to the poles, gives a mechanical equivalent of 76,000,000 J for each pound of subsiding matter. The spiral character of the

subsidence produces solenoidal currents, which may help to explain the equality of v_λ, v_c, and v_γ.

ON THE APPEARANCES OF THE ELECTRIC ARC IN THE VAPOUR OF BISULPHIDE OF CARBON. BY M. JAMIN, WITH THE ASSISTANCE OF M. G. MANEUVRIER.

At the meeting of the 19th June I made known to the Academy the modifications undergone by the electric arc in the vacuum of an air-pump when the arc is produced by a Gramme machine with alternating currents of high tension. I soon perceived that the appearances are modified if gases or vapours are introduced into the glass vessel in which the experiment is made. In the vapour of bisulphide of carbon they are very remarkable.

The burner is formed by two parallel vertical carbons fixed at their bases; the upper extremities, which face each other, can by a simple mechanism be joined or separated. The apparatus is placed under a large receiver of an air-pump, in which a vacuum as complete as possible is produced. It is known that then the arc is not formed; it is replaced by the gleams of a Geissler tube; but when a few drops of bisulphide of carbon are introduced, so as to obtain an increase of pressure of 5 or 6 centim., the arc is seen to kindle between the points when they touch, and to persist after they are separated.

At that moment there is as it were an explosion of light, so vivid as to be insupportable, incomparably superior to the usual brightness of the arc. On looking at it through a dark-coloured glass, one sees a brilliant arc 5 or 6 centim. in height, resembling a horseshoe or a capital omega. The two extremities are at the two carbon points. Besides this a long flame is seen like that of a hearth, which overhangs the arc, escapes from it, and ascends vertically.

The points of the carbons appear red and very brilliant; but the arc is pale green; and as its light dominates that of the carbons, the whole room is illuminated with that tint, as it would be by a Bengal light with copper. The brightness increases with the increase of tension of the vapour, until it becomes intolerable; but as the resistance of the medium is augmented at the same time, the arc often goes out, and it is necessary to relight it every moment by joining the two carbons.

Examined with the spectroscope this light presents all the lines of carburetted gases in combustion, but more complete and sharper. They are those described by M. Thollon at the meeting on August 1, 1881. The spectrum is very discontinuous. At its red end a grooved region was seen—first a very bright line followed by several others thinner and close, then a broader line a repetition of the first and likewise followed by fine lines; these appearances were repeated in going towards the orange, but growing weaker till they disappeared. After a dark interval the same appearances were seen again in the yellow and the beginning of the green: then there

was a dark interval, then the repetition of the same effects in the green, and finally in the violet.

In brief, the spectrum is composed of four grooved portions, in the red, yellow, green, and violet, so identical that they might be taken, except the colouring, for one and the same design which had travelled from the red to the violet. It is quite probable that they obey one and the same harmonic law, which remains to be discovered.

Of these four regions the green is the most luminous; it is that which gives the special tint taken by the arc and colouring all objects green.

During the manifestation of these appearances a chemical action takes place. If any air has been left in the receiver, or if the apparatus is not quite closed, the bisulphide of carbon undergoes incomplete combustion, a mist of sulphur fills the space and is deposited on the sides; the carbon burns alone. If the air has been well purified, the mist does not form; a brown deposit settles on the sides, becomes black, sticks to the glass, and tarnishes it. This deposit is volatile; its odour reminds one of that of sulphur.

It is evidently a compound of sulphur and carbon, perhaps a protosulphide corresponding to the oxide of carbon, perhaps an isomeric modification of the ordinary sulphide. In fact, neither a deposit of sulphur nor one of carbon is seen, and the carbons of the burner have neither lost nor gained any thing. It is probable that the bisulphide of carbon is dissociated, the sulphur volatilized, the carbon in vapour disseminated in the arc, and that this carbon and this sulphur recombine in the flame to reconstitute a combination under different conditions. But this is only a conjecture, no analysis having yet been made.

To recapitulate, this experiment is remarkable for the extraordinary quantity of light produced, the magnitude of the arc, its colour, the composition of its spectrum, and the chemical actions which take place. It is not likely that it can ever be turned to account for lighting, on account of its colour, unless perhaps for light-houses or distant signals.—*Comptes Rendus de l'Académie des Sciences*, July 3, 1882, t. xcv. pp. 6, 7.

ON THE ELECTRIC RESISTANCE OF GLASS AT LOW TEMPERATURES.

BY G. FOUSSEREAU.

The method employed consists in passing the electricity supplied by a Volta's pile of from 1 to 100 elements across a reaction-tube of 1–2 centim. diameter and very regular thickness, closed at one end. The electricity is collected in a condenser of known capacity, the two armatures of which are connected with the two mercuries of a Lippmann electrometer of measured capacity. The time necessary for communicating to the mercurial column of the electrometer a displacement corresponding to a determined difference of potential is observed.

The reaction-tube dips into a wider test-tube; and its two faces

are bathed, up to a known height, by two conducting masses of concentrated sulphuric acid, into which dip some platinum wires carefully insulated from the sides above the level of the liquid. This apparatus is surrounded by a glass "muff," the air of which is dried by sulphuric acid before commencing the experiments.

In order to obtain a uniform and slowly variable temperature, the base of the apparatus is inserted, up to a level considerably above that of the acid, in an oil-bath, which is itself surrounded by a sand-bath which can be heated progressively. For the sand-bath a refrigerating mixture can be substituted. The observations were extended to $-17°$ C.

If E designates the electromotive force of the pile, ρ_1 and ρ_2 the internal and external radii of the tube, h the height of the liquid, r the specific resistance of glass per cubic centimetre, C the sum of the capacities of the condenser and electrometer, and e the difference of potential communicated to the electrometer (always very small in proportion to E), we have, expressing that the quantity of electricity transmitted through the glass in the time θ has been employed in charging the condenser,

$$r = \frac{2\pi h \mathrm{E}}{\mathrm{C}e \log n \frac{\rho_2}{\rho_1}} \times \theta.$$

Several experiments, made with different heights of sulphuric acid, permit the elimination of the influence of the bottom of the tube, the thickness of which is not the same as that of the sides.

At the instant of the completion of the circuit the glass tube is at first charged like a condenser. Its interior layers afterwards gradually absorb a certain charge of electricity, necessary for bringing them into the definitive state corresponding to the fall of potential established between the surfaces. During this variable state, more or less prolonged according to the nature of the glass, the effects of the charge of the glass are superposed to those of the conductivity. The observations are commenced when the time occupied in charging the glass has assumed a constant value.

I have also observed that rapid heating determines an apparent increase of conductivity greater than the normal increase: in like manner a rapid lowering of the temperature gives rise to an exaggerated resistance: but these phenomena quickly disappear, to give place to the normal resistance; and they are not again produced when the variations of temperature are slow*.

My observations have hitherto been made on three kinds of glass —common glass (with a base of soda and lime), Bohemian glass, and crystal.

In all three, raising the temperature produces a rapid increase of conductivity; the resistance can be expressed by exponential func-

* These phenomena appear to be due to variations in the dielectric power of glass under the influence of temperature.

tions of the form

$$\log x = a - bt + ct^2.$$

(1) For common glass, of density 2·539, expressing the resistances per cubic centim. in millions of megohms, we get the following results:—

Temperatures.	Resistances.
+61°·2	0·705
+20°	91·0
−17°	7970·0

In order to form an idea of the magnitude of this last resistance, it may be remarked that it represents nearly twice the resistance of a copper wire, of 1 square millim. section, reaching from the earth to Sirius.

The whole of the results obtained upon common glass are expressed by the formula

$$\log x = 3{\cdot}00507 - 0{\cdot}052664 \times t + 0{\cdot}00000373 \times t^2.$$

The term of the second order being very small, the values of $\log x$ are represented by a line which differs but little from a straight line. The resistance varies nearly $\frac{1}{9}$ of its value for each degree of temperature.

(2) Bohemian glass of density 2·431, upon which I worked, has from 10 to 15 times the conductivity of common glass at the same temperatures. Its resistance is given by the formula

$$\log x = 1{\cdot}78300 - 0{\cdot}049530 \times t + 0{\cdot}0000711 \times t^2.$$

(3) The crystal tried has for its density 2·933; and it, contrary to Bohemian glass, has from 1000 to 1500 times the insulating-power of ordinary glass at the same temperatures. Its conductivity only begins to be manifest at above 40°.

At 40°·2 its resistance is equal to 6182
At 105° „ „ ..... 11·6

The results are represented by the formula

$$\log x = 7{\cdot}22370 - 0{\cdot}088014 \times t + 0{\cdot}00028072 \times t^2 *.$$

—*Comptes Rendus de l'Académie des Sciences*, July 31, 1882, t. xcv. pp. 216–818.

ON THE SURFACE-TENSION OF SOME LIQUIDS IN CONTACT WITH CARBONIC ACID†. NOTE BY S. WROBLEWSKI‡.

If instead of water we take a liquid which mixes in all proportions with liquid carbonic acid—for instance, alcohol, essential oil

* The experiments were made in M. Jamin's laboratory at the Sorbonne.

† Abstract by the Author.

‡ See the preceding Note, Phil. Mag. Sept. 1882, p. 236.

of turpentine, ether, chloroform—the phenomena assume the following form:—

The surface-tension also diminishes with the increase of the pressure under which the gas is placed; the velocity of the diminution is also much greater at a low than at a higher temperature; but the surface-tension, instead of stopping at a minimum which would be something characteristic of the liquid, falls rapidly; and at 0° C., under the pressure at which carbonic acid is liquefied, all the liquids above mentioned, without distinction, have the surface-tension of carbonic acid—that is, a tension equal to 4·6 $\frac{\text{dyne}}{\text{centimetre}}$.

But can they in this state be regarded as the same liquids? Not at all. Let us take water as an example. When it is saturated with carbonic acid under the pressure of one atmosphere only, has it the properties of pure water? It has a different density, a different coefficient of expansion by heat: even the temperature of its maximum density is changed. The changes which take place in the liquids mentioned are much more considerable: we need only observe what takes place with ether when it absorbs carbonic acid under pressure. Its volume increases with such rapidity that, although my method enables me to measure the surface-tension of a liquid in a much shorter time than a minute, it is almost impossible to take exact measurements in this case.

It follows from all these facts that the phenomena described in these Notes have absolutely nothing to do with pressure. The diminution of the surface-tension of the liquids depends solely on the circumstance that the surface-tension of carbonic acid, with which they are compressed, is extremely slight. While the surface-tension of water is equal to 80 $\frac{\text{dyne}}{\text{centimetre}}$ nearly, and those of the other liquids examined, with the exception of ether, lie between 32 and 25 $\frac{\text{dyne}}{\text{centimetre}}$, the tension of carbonic acid is only 4·6 $\frac{\text{dyne}}{\text{centimetre}}$.

Capillary phenomena depend only on the molecular forces acting at the surface of the liquid. Therefore it is not surprising that the presence of a body which possesses so feeble a tension among the particles of the liquid of which that surface-layer is composed exerts an influence upon the resultant of all those forces—that is to say, upon the surface-tension.

The same mode of reasoning applies to the case of a liquid being compressed with a much less liquefiable gas than carbonic acid—such as oxygen, nitrogen, or hydrogen: for the more difficult it is to liquefy those gases the less must be their surface-tension. The effect of the compression will depend only on the value of that tension and on that of the solubility of the gas.—*Comptes Rendus de l'Académie des Sciences*, August 14. 1882. t. xcv. pp. 342, 343.

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

NOVEMBER 1882.

XXXVIII. *Comparison of Methods for the Determination of Resistances in Absolute Measure. By* LORD RAYLEIGH, *F.R.S., Professor of Experimental Physics in the University of Cambridge**.

AT the present time, and in view of the projected conference at Paris, the subject of the present paper is engaging a large share of attention; and Prof. G. Wiedemann † has published an interesting discussion of some of the methods that have been employed. I have thought it might be of service if I also were to place upon record the views that I have been led to entertain, and which are the result of a good deal of experience.

Resistance being of the dimensions of velocity, its absolute measurement involves the absolute measurement of a length and of a time. The latter is usually the time of a vibration of a suspended magnet, and can be determined without much difficulty. In the B.A. method it is the time of rotation of the revolving coil, and can be obtained with all desirable accuracy. In this respect there is not much to choose between one method and another; but when we come to consider the manner in which the linear measurement enters, important differences reveal themselves. These will be discussed in detail presently; but for the moment it will be sufficient to say that

* Communicated by the Author.

† "Ueber die bisherigen Methoden zur Feststellung des Ohm." Separatabdruck aus der *Electrotechnischen Zeitschrift*, July 1882. [Phil. Mag. for October, p. 258.]

the presumption is in favour of any method which requires only a single linear measurement. It is true that this question cannot be decided without regard to the subject of the measurement; but, with scarcely an exception, it is necessary to know the mean radius of a *coil* of several layers of insulated wire. This is apparently the measurement which fixes the limit of final accuracy; and, in comparison with it, determinations of the distances of mirrors and scales &c. are of secondary difficulty.

It will be convenient now to enumerate the principal methods which have been proposed for determining absolute resistances. Minor details, which are not likely to influence the final value of the results, will in general be passed over.

I. Kirchhoff's *Method*, Maxwell's *Electricity and Magnetism*, § 759.

The magnitude of a continuous battery-current in a primary coil is compared with that of the transient current induced in a secondary coil when the primary circuit is removed. Rowland* effected an important improvement by simply reversing the battery-current without motion of the primary coil. The time of vibration of the ballistic galvanometer employed for the transient current is the principal time-measurement. In Rowland's investigation a second galvanometer was employed for the battery-current, and the ratio of constants had to be found by auxiliary experiments. In Glazebrook's† recent determination by this method only one galvanometer was used, the battery-current being reduced in a known manner by shunting. It is shown that the evaluation of the resistance-ratios presents no serious difficulty.

Let h denote the ratio in which the primary current is reduced when it produces a deflection α upon the galvanometer, θ the throw from rest due to the induction-current when the battery is reversed, T the time of vibration of the needle measured from rest to rest, M the coefficient of induction; then the resistance of the secondary circuit in absolute measure is given by

$$\mathrm{R} = \frac{\pi \tan \alpha \mathrm{M}}{\mathrm{T} \cdot \sin \frac{1}{2}\theta} \div h.$$

Whenever, as in this method, the conductor whose resistance in absolute measure is first determined is composed of copper, frequent comparisons are necessary with standards of German silver or platinum-silver. Otherwise a variation of temperature of about $\frac{1}{4}$ of a degree Cent., which can hardly be detected

* American Journal, xv. 1878.

† Proc. Roy. Soc. June 1882.

with certainty by thermometers, would influence the result by as much as one part in a thousand.

If it be granted that the comparison of currents and the reference to the standard of resistance can be effected satisfactorily, we have only to consider the amount of error involved in the determination of M, the coefficient of mutual induction between the two circuits, which is the fundamental linear measurement. If the two coils are of very nearly the same size, it appears from symmetry that the result is practically a function of the mean of the mean radii only, and not of the two mean radii separately. It is also of course a function of the distance between the mean planes b. Leaving out of consideration the small corrections necessary for the finite size of the sections, we consider M as equal to $4\pi\sqrt{\mathrm{A}a}$ multiplied by the function of γ, given in tables appended to the second edition of Maxwell's 'Electricity,' where

$$\sin\gamma = \frac{2\sqrt{\mathrm{A}a}}{\sqrt{\{(\mathrm{A}+a)^2+b^2\}}},$$

or, if we identify A and a with their mean (A_0),

$$\tan\gamma = \frac{2\mathrm{A}_0}{b}.$$

The error in M will depend upon the errors committed in the estimates of A_0 and b. If we write

$$\frac{d\mathrm{M}}{\mathrm{M}} = \lambda\frac{d\mathrm{A}_0}{\mathrm{A}_0} + \mu\frac{db}{b},$$

then, since M is linear,

$$\lambda+\mu=+1.$$

Thus, if b were great relatively to A_0,

$$\lambda=4, \quad \mu=-3,$$

a very unfavourable arrangement, even if it did not involve a great loss of sensitiveness. The object must be so to arrange matters that the errors in A_0 and b do not multiply themselves unnecessarily in M. But since μ is always negative, λ must inevitably be greater than unity.

The other extreme case, in which b is very small relatively to A_0, may also be considered independently of the general tables; for we may then take approximately (Maxwell's 'Electricity,' § 705)

$$\mathrm{M} = 4\pi\mathrm{A}_0\log\left\{\frac{8\mathrm{A}_0}{b}-2\right\},$$

whence

$$\mu = -\frac{1}{\log(8\mathrm{A}_0/\mathrm{B})-2},$$

showing that as b diminishes μ approaches zero, and accordingly λ approaches unity, as is indeed otherwise evident. But when b is small, it is the absolute error db which we must regard as given rather than the relative error db/b; and thus we are directed to stop at a moderate value of b, even if the increased correction necessary for the size of the section were not an argument in the same direction.

The following intermediate cases, calculated by the tables, will give an idea of the actual conditions suitable for a determination by this method:—

γ.	$b/2A_0$.	λ.	μ.	M.
60°	·577	2·61	−1·61	·316
70	·364	2·18	−1·18	·597
75	·268	1·98	− ·98	·829
80	·176	1·76	− ·76	1·186

We may say that the error in the distance of mean planes will reproduce itself something like proportionally in the final result, and that the error of mean radius will be doubled.

Any uncertainty in the actual position of the mean planes relatively to the rings on which the wire is wound may be eliminated, as Glazebrook has shown, by reversing the rings relatively to the distance-pieces.

This method is subject to whatever uncertainty attaches to the use of a ballistic galvanometer*. In its favour it may be said that the apparatus and adjustments are simple, and that no measurements of distances between mirrors and scales is necessary for the principal elements. It should be noticed also that the error due to faulty determination of the distance of mean planes can be eliminated in great measure by varying this quantity, which can be done over a considerable range without much difficulty or expense.

With reference to the capabilities of the method for giving results of the highest accuracy when carried out in the most ambitious manner, it is important to consider the effect of increasing the size of the coils. The coils used by Glazebrook have a mean radius of about 26 centim.; the axial and radial breadths of the section are each about 2 centim. If we suppose the mean radius and the sides of the section to be doubled, the number of turns (about 800) remaining unaltered, the sensitiveness would be increased both by the doubling of M and by the diminished resistances of the coils, while at the

* See Phil. Trans. 1882, p. 669.

same time the subjects of the linear measurements would be of more favourable magnitudes. To enhance the latter advantage, it would probably be an improvement to diminish the radial breadth of the section, on which much of the uncertainty of mean radius depends. In either case it is clear that the limit of accuracy obtainable by this method has not yet been reached.

II. Weber's *Method by Transient Currents*, Maxwell § 760.

"A coil of considerable size is mounted on an axle so as to be capable of revolving about a vertical diameter. The wire of this coil is connected with that of a tangent-galvanometer so as to form a single circuit. Let the resistance of this circuit be R. Let the large coil be placed with its positive face perpendicular to the magnetic meridian, and let it be quickly turned round half a revolution. There will be an induced current due to the earth's magnetic force; and the total quantity of electricity in this current in electro-magnetic measure will be

$$Q = \frac{2g_1 H}{R},$$

where g_1 is the magnetic moment of the coil for unit current, which in the case of a large coil may be determined directly by measuring the dimensions of the coil and calculating the sum of the areas of its windings; H is the horizontal component of terrestrial magnetism; and R is the resistance of the circuit formed by the coil and galvanometer together. This current sets the magnet of the galvanometer in motion."

"If the magnet is originally at rest, and if the motion of the coil occupies but a small fraction of the time of a vibration of the magnet, then, if we neglect the resistance to the motion of the magnet, we have, by § 748,

$$Q = \frac{H}{G}\frac{T}{\pi} 2 \sin \tfrac{1}{2}\theta,$$

where G is the constant of the galvanometer, T is the time of vibration of the magnet, and θ is the observed elongation. From these equations we obtain

$$R = \pi G g \frac{1}{T \sin \frac{1}{2}\theta}.$$

The value of H does not appear in this result, provided it is the same at the position of the coil and at that of the galvanometer. This should not be assumed to be the case, but should be tested by comparing the time of vibration of the

same magnet, first at one of these places, and then at the other."

If a be the mean radius of the coil of the inductor and A that of the galvanometer, we may write, neglecting the corrections for the finite sizes of the sections,

$$g=\pi a^2, \quad \mathrm{G}=\frac{2\pi}{\mathrm{A}};$$

so that

$$g\mathrm{G}=2\pi^2\frac{a^2}{\mathrm{A}}.$$

This is the linear quantity of the method. With respect to the chances of error in determining it, we see that the error of the mean radius of the inductor enters doubly, and that of the mean radius of the galvanometer enters singly. Probably in this respect there is not much to choose between this method and the use in method I. of the same coils placed at a moderate distance apart.

A colossal apparatus for the use of the present method has been constructed and tested by MM. W. Weber and F. Zöllner*, the coils of which are as much as 1 metre in diameter. The principal difficulty arises in connexion with the galvanometer-magnet. Two magnets were used whose lengths were respectively 200 millim. and 100 millim.; and the results obtained in the two cases differed by as much as 2 per cent. The discrepancy is doubtless due to the influence of the finite length of the magnets causing the magnetic poles to be sensibly distant from the centre of the coil, for which point the effects are calculated; and the disturbance will be proportional to the square of the distance between the poles, or more properly to the "radius of gyration" of the ideal magnetic matter about the axis of rotation. But to assume that the disturbance from this source was exactly four times as great in the one case as in the other, and thence to deduce the result corresponding to an infinitely short magnet, appears to me to be a procedure scarcely consistent with the degree of accuracy aimed at. If this method is to give results capable of competing with those obtainable in other ways, it will be necessary to use a much shorter magnet; or, if that is not practicable, to devise some method by which the distance of the poles can be determined and a suitable correction calculated.

In carrying out the observations in the usual manner, it is necessary to measure the distance between a mirror and a scale. By using a double mirror with two scales and telescopes, MM. Weber and Zöllner avoid the principal cause of

* *Ber. d. Kön. Sächs. Ges. zu Leipzig*, 1880, ii. p. 77.

difficulty, *i. e.* the unsteadiness of the suspended mirror, all that is then necessary to know with accuracy being the distance between the two scales.

In using this and the three following methods great pains must be taken with the levelling of the earth inductor, since the deviation of the axis of rotation from the vertical (at least in the plane of the meridian) gives rise to an error of the first order with (in these latitudes) a high coefficient. In this respect it would be a decided advantage to carry out the experiments in a locality nearer to the magnetic equator (see "Account of Experiments to determine the value of the B.A. Unit in Absolute Measure," Phil. Trans. for 1882, p. 684). It is to be hoped that the measurements commenced by Weber and Zöllner will be carried to a successful issue, as it is only by the coincidence of results obtained by various methods that the question can be satisfactorily settled. At present no value in absolute measure of the B.A. unit or of the Siemens unit has been published as the result of their work.

III. *Method of Revolving Coil.*

This method, first, it would appear, suggested by Weber, was carried into execution by the celebrated Electrical Committee of the British Association*, and more recently by myself with the assistance of Dr. Schuster and others†. The greater part of what I have to say upon this subject has been put forward already in the papers referred to, from which alone the reader can form a complete opinion on the merits or demerits of the method as hitherto practised. On the present occasion I must take many of the conclusions there arrived at for granted, or at most give a mere indication of the nature of the arguments by which they may be supported.

Method III. differs from II. mainly in the fact that in III. the earth-inductor is, so to speak, its own galvanometer, the needle whose deflections measure the currents being suspended at the centre of the revolving coil itself instead of at the centre of another galvanometer-coil forming part of the same circuit. If, as in II., the inductor-coil were simply twisted through 180° when the needle passes its position of equilibrium, the disadvantages of the simplification would probably preponderate over the advantages. The diminution of effect due to the oblique position of the coil relatively to the needle (except at the moment of passing the magnetic meridian) would indeed be compensated by the diminished resistance of the complete circuit, and, as will presently appear, considerable advantage

* Brit. Assoc. Reports, 1862–1867. Reprint, Spon, 1873.

† Proc. Roy. Soc. May 1881, Feb. 1882; Phil. Trans. 1882.

would arise in respect of errors in the measurement of the coil; but an almost fatal uncertainty would be introduced from the influence of self-induction.

The important advantage of III., obtained, as I believe, without any really important sacrifice, arises only when the inductor is set into uniform rotation. In II., if the connexions were maintained without a commutator, the current in the galvanometer-coil would be alternating, and therefore unsuitable for measurement with a magnetic needle; but in III., although the current in the coil itself alternates, the reversal of the coil relatively to the needle causes all the impulses to operate finally in the same direction. When, therefore, the coil is caused to revolve in a periodic time small relatively to that of the free vibration of the needle, a steady deflection is obtained which varies inversely with the absolute resistance of the coil.

If we omit for the moment all secondary considerations, although some of them may not be without importance, the formula by which the resistance (R) of the revolving circuit is given in terms of the mean radius (a), the number of turns (n), the angular velocity of rotation (ω), and the angle of deflection (ϕ), runs

$$R = \pi^2 n^2 a \omega \cot \phi;$$

from which it appears that, in respect of errors arising from the measurements of the coil, this method is much superior to those hitherto discussed. There is only one linear quantity concerned; and the error committed in its determination enters but singly into the final result. Indeed we may say that in this respect no improvement is possible, unless it be in the direction of substituting for the mean radius of a coil of several layers some other kind of linear quantity more easy to deal with.

In requiring the absolute measurement of angle, II. and III. stand precisely upon a level.

The time of vibration in the experiments of MM. Weber and Zöllner was 17 seconds or 30 seconds—none too long relatively to the time (2 seconds) occupied in turning the inductor. If we suppose the coil to be uniformly rotated at the rate of, say, 2 revolutions per second, there would be 68 or 120 impulses upon the needle in the time of 1 vibration. It would no doubt be a great exaggeration to represent the increase of sensitiveness as being in any thing like this proportion, since by the method of recoil it is possible to make several observations of impulses during the time required for one observation of steady deflection. Nevertheless it cannot

be doubted that the advantage of III. in respect of sensitiveness is very considerable.

Experience has shown that there is no difficulty in controlling and measuring the rotation of the coil; but of course some auxiliary apparatus is required for the purpose. Against this may be set the escape from observations of the time of vibration, and from any uncertainty which may attach to the ballistic use of a galvanometer-needle. The suspended magnet may easily be made of such dimensions that no appreciable error can arise from supposing it to be infinitely small.

On the other hand, some new complications enter in method III. which I desire to state in full. In the first place we have to take account of the fact that the inductor moves in a field of force due not only to the earth, but also to the suspended magnet itself. I do not think that the correction thus rendered necessary (about 4 parts per thousand in my experiments) adds in any appreciable degree to the uncertainty of the final result; but we may take note of the fact that an auxiliary determination must be made of the ratio of the magnetic moment of the suspended magnet to the earth's horizontal force.

If the metal ring on which the wire is wound be on a large scale and sufficiently massive for strength, currents may be developed in it, even although it is divided into two parts by ebonite insulation. In my experiments the effect of these currents was very sensible, and had to be allowed for by careful observations of the deflection produced when the ring was rotated with wire circuit open. In any future repetition it will be worthy of consideration whether the ring should not be formed of less conducting material. It does not appear, however, that the final result can be prejudicially influenced; and the effect produced by secondary closed circuits allows us to verify the insulation of contiguous layers or turns of the wire by comparing the deflections obtained before the wire is wound with those obtained after winding, but with main circuit open, any difference being due to leakage.

But the most serious complication in method III., and one which in the eyes of some good judges weighs strongly against it, is the disturbing influence of self-induction. With respect to this, the first point to be noticed is that the action is perfectly regular, and that the only question which arises is whether its magnitude can be determined with such accuracy that the final result does not suffer. Now the operation of self-induction is readily submitted to calculation if a certain coefficient (L) be known. We find

$$R = \pi^2 n^2 a \omega \cot \phi \{1 - U \tan^2 \phi - U^2 \tan^4 \phi\},$$

where U is a numerical quantity dependent upon L, so that the influence of self-induction is approximately proportional to the square of the speed of rotation. The same law applies also to any disturbances depending upon mutual induction between the wire circuit and subordinate circuits in the ring.

It will be seen that, if the law of squares may be depended upon, the influence of self-induction (and mutual induction) can be satisfactorily eliminated by combining observations taken at different speeds. In my experiments four speeds were used, of which the greatest and the least were in the ratio of 2 : 1. The effect of self-induction was therefore four times as great at the high speed as at the low speed. In other words, the quantity (about 1 per cent.) by which the low-speed result is to be corrected in order to eliminate the influence of self-induction is only one third of the discrepancy between the uncorrected results of the extreme speeds. If, therefore, the observations are good for any thing at all, they are good enough to determine this correction with all desirable precision. If a check be considered necessary, it is supplied by the results of the intermediate speeds.

The above reasoning proceeds upon the supposition that we have no independent knowledge of the magnitude of the coefficient U. In point of fact, this coefficient can be calculated with considerable accuracy from the data of construction, so that the empirical correction is applied only to a small outstanding residue.

In considering the disadvantageous influence of self-induction as an argument in favour of II. as against III., we must remember that the magnitude of the influence can be greatly attenuated by simply diminishing the speed of rotation. At half the lowest speed above spoken of, for which the correction for self-induction would be reduced to $\frac{1}{4}$ per cent., the deflection (over 100 millim. at a distance of 2670 millim.) would probably correspond to a much greater sensitiveness than it is possible to obtain under II. If we prefer the higher speed, it is because we estimate the advantage of doubled sensitiveness as outweighing the disadvantage of a fourfold correction for self-induction.

The fourth objection which may be taken to this method, and it is one from which II. is free, lies in the necessary creation of mechanical disturbance in the neighbourhood of the suspended magnet.

How far these complications may be supposed to prejudice the result of carefully conducted experiments must be left to the estimation of the reader of my paper, in which very full data for a judgment are given. My own opinion is, that while

in the aggregate they must be allowed to have some weight, they are far from preponderating over the advantages which the method possesses in comparison with II.

If we take the view that the method itself is trustworthy, the principal error will arise in connexion with the mean radius of the coil; and it becomes an interesting question to consider whether advantage may be expected from a further increase in the dimensions of the apparatus. For this purpose we may regard tan ϕ as given. The total resistance R will be proportional to n^2a/S, where S denotes the aggregate section of the copper, from which it follows that ωS may be regarded as given, while a is left undetermined by the consideration of sensitiveness. Thus, if we retain ω and S unaltered in a magnified apparatus, we shall have the same sensitiveness as before, while the increased diameter of the coil and the relatively decreased dimensions of the section will conduce to a more accurate determination of the mean radius.

The angular deflection being given, the correction for self-induction is nearly constant whatever may be the proportions of the coil.

If we are of opinion that there is danger in the operation of self-induction, the case becomes strong for the introduction of a second coil in a plane perpendicular to that of the first *. By this means the relative correction for self-induction would be reduced to one quarter, while the deflection remained unaltered. It scarcely needs to be remarked that this use of a second coil would not, as in II., increase the uncertainty depending upon the linear measurements, the two mean radii entering into the result as parts, and not as factors.

This combination would lend itself especially well to low speeds of rotation; for the deflecting force, being uniform in respect to time, would not give rise to forced vibrations of the needle. The latter would have nothing further to do than to indicate the direction of a constant field of force.

IV.

This method, which was proposed by Foster †, and more recently by Lippmann, and to a certain extent executed by the former, is a modification of III., in which the electromotive force generated during the rotation of the inductor is balanced by an external electromotive force, and thus not allowed to produce a current. The external electromotive force is due to the passage of a battery-current through certain resistance-coils; and the current is compared with the earth's horizontal

* Proc. Roy. Soc. May 1881, p. 123.
† Brit. Assoc. Report, 1881.

intensity (H) by an absolute tangent-galvanometer. The difference of potential at the two points of derivation is thus known in terms of the included absolute resistance (R) and H. The circuit is continued through a sensitive galvanometer and the coil of the inductor, and is closed only when the latter coil is nearly in the plane of the meridian. When balance is obtained, the electromotive force of induction $n \cdot \pi a^2 \cdot H \cdot \omega$ is equal to $RH \tan \alpha / G$, where G is the constant of the tangent-galvanometer and α the angle of deflection. The result, from which H disappears, if it may be assumed to be the same in the two places, is thus

$$R = n\pi a^2 G \cdot \omega \cot \alpha,$$

or, if A be the mean radius of the galvanometer-coil,

$$R = 2n\pi^2 \omega \cot \alpha \frac{a^2}{A},$$

from which the value of the resistance-coils is obtained in absolute measure. One advantage of this method, which it shares with VI. below, is that the resistance immediately expressed may be that of well-constructed coils of German silver or of platinum-silver at a known temperature.

This method is nearly free from the secondary objections to III. discussed above. The self-induction of the revolving wire-circuit does not enter, as no appreciable current is allowed to form itself; but there would appear to be a possibility of disturbance from mutual induction between the wire-circuit and secondary circuits in the ring. It would certainly be necessary to prevent the flow of currents round the ring by the insertion of an insulating layer; and even with this precaution some control in the way of a variation of speed would almost be necessary. Again, it is a question whether disturbance, from thermo-electricity for instance, may not arise at the place where the contacts are made and broken.

It is to be hoped that a complete series of observations may be made by this method, which certainly possesses considerable merits; but at best it remains open to the objection mentioned under II., with which in this respect it stands upon a level, *i. e.* that errors may enter from the measurements of both coils, the error of A entering singly into the result, and that of a entering doubly.

In respect of requiring absolute measurements of angle, there is nothing to choose between II., III., IV., and V.

V. Weber's *Method by Damping.*

This is the method followed by Kohlrausch * in his investi-

* Pogg. *Ann.* Ergänzungs-band vi.; Phil. Mag. 1874, April and May.

gations upon this subject. It is founded upon II.; but in order to avoid the difficulty arising from the necessity of using a magnet small relatively to the coil in which it is suspended, no attempt is made to determine the constant from the data of construction. The inductor is connected with a sensitive galvanometer, and the constant of the latter is deduced from observations of the logarithmic decrement of the vibrations of the magnet when ths circuit is closed (λ), and when it is open (λ_0). The result, however, involves H the horizontal intensity, K the moment of inertia of the needle, as well as the time of vibration T. Expressed roughly, in the notation previously employed, it is

$$R = \frac{32a^4H^2T\lambda}{K} \cdot \frac{AB}{(A^2+B^2)^2},$$

where R is the resistance of the circuit composed of the inductor and galvanometer, A and B are the arcs of vibration in the method of recoil.

Interesting as this method is in some respects, I cannot but agree with Rowland in thinking that the final formula is enough to show that it cannot compete with others on equal terms, if the object be to obtain a result of high accuracy. The horizontal intensity itself is perhaps nearly as difficult to determine as absolute resistance; and the error thence arising doubles itself in the result. There is in addition the error of K. But even if H and K were not subject to error at all, I believe that the occurrence of the fourth power of the radius of the inductor is a fatal defect, and tends to explain the discrepant result obtained by Kohlrausch*. It is also worthy of note that the error of levelling enters twice as much as in II., III., and IV.

VI. Lorentz's *Method.*

This method, which, with the introduction of certain modifications not affecting its essential character, I am disposed to consider the best of all, was proposed and executed by Lorentz, of Copenhagen, in 1873†. A circular disk of metal, maintained in rotation about an axis passing through its centre at a uniform and known rate, is placed in the magnetic field due

* Oct. 1882.—It is very satisfactory to note that Kohlrausch (*Gött. Ges.* Sept. 1882) has recently detected an error in the value of the area of the windings of the inductor assumed in his previous calculations. Introducing the new value, obtained by an electrical process analogous to that described in Maxwell's 'Electricity,' § 754, he finds

$$1 \text{ B.A. unit} = \cdot 990 \times 10^9.$$

† Pogg. *Ann.* cxlix. p. 251.

to a battery-current which circulates through a coaxal coil of many turns. The revolving disk is touched near its centre and circumference by two wires. If the circuit were simply closed through a galvanometer, the instrument would indicate the current due to the electromotive force of induction acting against the resistance of the circuit. The electromotive force corresponding to each revolution is the same as would be generated in a single turn of wire coincident with the circumference of the disk by the formation or cessation of the battery-current. If this be called γ, and M be the coefficient of induction between the coil and the circumeference, m the number of revolutions per second, the electromotive force is $m\mathrm{M}\gamma$. For the present purpose, however, the circuit is not simply closed, but its terminals are connected with the extremities of a resistance R through which the battery-current flows, and the variable quantities are so adjusted that the electromotive force $\mathrm{R}\gamma$ exactly balances that of induction. When the galvanometer indicates no current, the following relation, independent, it will be observed, of the magnitude of the battery-current, must be satisfied,

$$\mathrm{R}=m\mathrm{M};$$

and from this, M being known from the data of construction, the absolute resistance R of the conductor is determined.

It will be seen that this method has pretty close affinity to I. The secondary circuit is here, in a sense, reduced to a single turn, or rather to as many turns as the disk makes revolutions in a time comparable with the time of swing of the ballistic galvanometer; but the disadvantage of a reduced number of turns is probably more than compensated for by the continuous character of the induced current, which allows of its being brought into direct opposition to that of the battery. During the months from April to August of the present year I have been occupied in carrying out a determination by this method. Space will not permit of a detailed consideration of the various questions which presented themselves; and I must content myself with a brief statement of the procedure, and with such a discussion of the sources of error as will allow a comparison of this method with others. I hope shortly to communicate a detailed paper upon the subject to the Royal Society.

One of the principal difficulties to be overcome arises from the exceeding smallness of the resistance R, less than $\frac{1}{200}$ B.A. in my experiments. Lorentz employed an actual column of mercury of known dimensions, so that the result is given at once in terms of mercury. I had intended to follow the same course, but, after some trials, came to the conclusion that there would be difficulties in the way of thus obtaining the

degree of accuracy aimed at, and ultimately adopted a method of shunting. The main current from the battery was divided into two parts, the larger of which passed through a resistance of half a unit, formed by combining two singles in multiple arcs. The resistance traversed by the other part of the main current was much larger (from 10 to 20); and it was to two points on this branch distant $\frac{1}{10}$ that the wires of the derived circuit were connected. With proper precautions this arrangement was found satisfactory, and the equivalent resistance R could be accurately expressed in terms of the standard B.A. units. The adjustment for obtaining the balance was effected by varying a large resistance placed in multiple arc with one of the others; or rather two effective resistances were used, one on either side of that required for balance, the latter being finally calculated by interpolation from the indications of the galvanometer.

By observing only the effect of reversing the battery-current the results are freed from the influence of terrestrial magnetism, and from the very sensible thermoelectric force having its seat at the sliding contact. These contacts were made by means of brushes of copper wire. One brush pressed against the cylindrical edge of the disk, which was about $\frac{1}{4}$ inch broad; and the other pressed against the shaft on which the whole turned. The area included by the secondary circuit was therefore not exactly that of the disk, but required a small correction, as to which, however, there is no difficulty.

The arrangements for driving the disk and for observing the speed were the same as for the revolving coil of method III. The results, which in the same arrangement have not differed by so much as $\frac{1}{10000}$ on different days, show that the sensitiveness was sufficient.

After these explanations I come to the main subject of the present remarks, viz. the degree of accuracy likely to be attained in the fundamental linear measurement. In the present case the quantity to be determined is M; and so far there is no difference between this method and I. But the fact that the secondary circuit is here represented by a disk whose diameter can be measured much more accurately than that of a coil introduces a certain modification. It is necessary also that the arrangements be symmetrical with respect to the middle plane of the disk, as, on account of the width of the brush, the place of contact cannot be considered as well defined. The necessary condition can be satisfied with a single coil by placing it so that its mean plane coincides with that of the disk. In this position slight errors of adjustment produce effects of the second order only, and every thing depends upon the radii.

Preparatory to the design of the apparatus for my experiments, I made some calculations of the values of the induction-coefficient and of its rates of variation for various ratios of the radius of the coil (A) to that of the disk (a). The angle γ (see method I.) is here ($b=0$) determined by $\tan^2\frac{1}{2}\gamma=a/A$. If we write

$$\frac{\delta M}{M}=\lambda\frac{\delta A}{A}+\nu\frac{\delta a}{a},$$

the sum of λ and ν will be unity. The following are the values found. Those under M are proportional only, and relate to the case in which A is constant.

a/A.	λ.	ν.	M.
·5	−1·2	+2·2	4·37
·6	−1·36	+2·36	6·65
·7	−1·5	+2·5	9·80
·8	−2·0	+3·0	14·4

In Lorentz's apparatus the value of a/A was even larger than the last in the table, and the radial dimension of the coil was no small fraction of $(A-a)$. On this account, as has already been pointed out by Rowland, no very accurate result could be expected.

In my experiments two similar coils were used whose radius (A) = about 26 centim., and in two distinct arrangements. In the first arrangement the two cells were placed close together; so that the case corresponded pretty closely with that just spoken of. The radius of the disk is about 16 centim.; and thus the proportions are nearly those of the second example in the table. It will be seen that the circumstances are not unfavourable to accuracy, the error of mean radius of the coil entering into the result to a less extent than in any of the methods hitherto described, except III. and IV. The disk is so much more easily measured, that the larger coefficient 2·36, applicable to it, should not lead to much error in the result.

This arrangement was worked at two speeds of rotation in the proportion of 10 : 16, and gave with close accordance

$$1\text{ B.A. unit}=\cdot9867\times10^9\text{ C.G.S.}$$

In the other arrangement the two coils were separated to a considerable distance, and the induction-coefficient depended not only upon the mean radii of the coils (and of the disk), but also upon the distance of their mean planes. The peculiarity of this arrangement, to which I wish to draw special

attention, is that it is possible so to proportion the quantities that *the error of mean radius of the coil does not affect the result*, which accordingly depends only upon the diameter of the disk and the distance of the coil's mean planes. How this may come about will be readily understood by considering the dependence of M upon A when a and b are given. It is clear that M vanishes, both when A is very small and when it is very large; from which it follows that there must be some value of A for which the effect is a maximum and therefore independent of small variations of A.

In carrying out this idea it is not necessary to approach the above-defined state of things very closely; for of course we have in reality a good approximate knowledge of the value of A. In my apparatus the distance of mean planes was about 30 centim., so that b = about 15 centim. With the actual proportions a calculation of the effects of the various errors shows that

$$\frac{\delta M}{M} = \cdot 12\frac{\delta A}{A} - \cdot 96\frac{\delta b}{b} + 1\cdot 8\frac{\delta a}{a};$$

so that the error of A enters in quite a subordinate degree. The positive coefficient of δA shows that with the given coils and disk the separation was somewhat too great to secure the greatest independence of δA.

The success of this arrangement depends principally upon the degree of accuracy with which b can be determined. The two rings on which the wire is coiled are separated by distance-pieces; and, as in I., by reversing the rings relatively to the distance-pieces the result may be made to depend upon the mean length of these pieces and the mean thicknesses of the rings at the places of contact. The three distance-pieces were held together in one length and measured under microscopes; and the thicknesses of the rings were taken with verified callipers. There can hardly be a doubt but that this determination is much more accurate than that of the mean radius of a coil; and, what is also of some importance, it admits of repetition at pleasure with comparatively little trouble.

The value of the B.A. unit resulting from the measurement with this arrangement was $\cdot 9869 \times 10^9$ C.G.S.*

There seems no reason why a further increase of accuracy should not be obtainable by enlarging the scale of the apparatus. If we suppose the scale doubled, the number of turns in the coil and the angular speed of the disk being unaltered, the value of M would be doubled; and thus with the same

* The reductions not being yet finally completed, these numbers are liable to a change of one or two units in the fourth place of decimals.

battery-current the sensitiveness would be improved. Or, if we suppose the circumferential linear speed of the disk rather than its angular speed to be constant, the sensitiveness would be unchanged. If the larger coil were made of the same kind of wire as the smaller, its resistance would be augmented; but if the dimensions of the section were also doubled, so as to keep the proportions throughout, the advantage in this respect would lie with the larger apparatus.

On the whole, I am of opinion that if it is desirable at the present time to construct apparatus on the most favourable scale, so as to reach the highest attainable accuracy, the modification of Lorentz's method last described is the one which offers the best prospect of success. Before this is done, however, it appears to me important that the value now three times obtained in the Cavendish Laboratory by distinct methods should be approximately verified (or disproved) by other physicists. To distinguish between this value and those obtained, for instance, by Kohlrausch, by Lorentz, or by the first B.A. Committee, should not require the construction of unusually costly apparatus. Until the larger question is disposed of, it appears premature to discuss the details of arrangements from which the highest degree of precision is to be expected.

XXXIX. *On the Correlation of the Chemistry of the Carbon Compounds with the Phenomena of Life.* *By* C. F. Cross *and* E. J. Bevan*.

IT is not for us to dilate upon the marvellous progress of Organic Chemistry during this century, nor to find fault with the inevitably specializing tendency of research in the province of the carbon compounds; and we certainly owe an apology for entering upon a subject of such magnitude as the correlations of chemical with biological science. That which we offer is derived not so much from the consciousness of being able to originate views of these correlations which shall be more productive than certain which appear to prevail, as from the practical necessities of the investigations in which we find ourselves engaged, the paucity of the landmarks to which we have to look for guidance, and the misleading character of certain of the recognized principles and methods which has become manifest in the results of their application. In fine, there are numerous points in that portion of Biochemical Science the study of which we are prosecuting which call for critical discussion; and the existence of the imperfections

* Communicated by the Authors.

which it shall be our aim to expose we can only account for by the influence of the specializing tendency preventing chemists generally from following up the science in its wider relations.

It is certainly the ideal issue of organic chemistry to co-ordinate the multitudinous facts already and to be amassed concerning the carbon compounds, with the genesis, changes, and ultimate fate of the substances which go to build up the tissues of living organisms. Beyond this, indeed, many chemists do not hesitate to indulge in expectations as to the possible achievements of synthesis, which know no limits short of the inconceivable. The special "vital force" of a previous age they dismiss as an ancient cloke of ignorance long since discarded, under the genial influence of the sun of knowledge, even by those who most tenaciously opposed its sheltering folds to the stormy blast of "unbelief;" and thus by removing the great barrier, the whole universe of matter and force is opened out to the "conquering progress of man." In the words of an authoritative modern text-book, "At the present day the belief in a special vital force has ceased to encumber scientific progress. We now know that the same laws of combination regulate the formation of chemical compounds in animate and inanimate nature"*. The authors of this manifesto, however, leave us in doubt as to whether they regard the belief in question as itself obsolete, *or*, by being modified in accordance with the invincible array of facts by which their second dogma has been established, as brought within the pale of natural truth, and thus to have become an aid, and not an encumbrance, to scientific progress. The two propositions are certainly not equivalent unless this latter interpretation be allowed; and as equivalence is evidently intended, we take it that the authors, leaving the mystery of life to vindicate itself, also intended this interpretation, and would allow the chemical phenomena of life to be as special, say, as the phenomena of heat.

But it is difficult to rest satisfied with the isolated and physical interpretation of the passage; the generalizing tendency of modern physical science impels us to give it a wider consideration. Thus, to develop our parallel, we have long ceased to regard heat as having any special objective existence; and although its phenomena are, in relation to our perceptions, still sufficiently special to admit of classification apart, we no longer allow the exigencies of science to impede our progress towards a better understanding of the unity of nature, but recognize in heat "a manifestation of energy as a mode of molecular motion," a definition which is sufficiently exclusive

* Roscoe and Schorlemmer, 'Treatise on Chemistry,' iii. p. 10.

of subjective impressions. Further, chemists are in the habit of referring the phenomena of their science to the existence of a force of chemical affinity, and that without any justification more or less elaborate, in deference to its conditional character or to the metaphysical questionings which underlie all our natural science. And when we examine into the grounds upon which our belief in a vital force may be said to be dismissed, we find that we have in them only the basis of a truer knowledge than heretofore of what the vital force is and what it is not. Since the dismissal of the hypothesis of spontaneous generation, the distinction of matter into animate and inanimate has assumed a very sharp character. In the animate world we have a province of distinct phenomena; into this world, matter is coerced and is made to manifest properties distinct from those which it otherwise possesses; and in this world force is distributed and co-ordinated in such a way as to compel the acknowledgment of agency. Speaking physically, we admit that life is one of the narrowest—*i. e.* most extensively conditioned of phenomena; but this does not lessen our belief in the working, under these conditions, of a special agency. The minutely intimate correlation of life with its chemical phenomena doubtless leaves in the minds of many but a very narrow margin for the operation of the special agency in question, and makes its assumption appear proportionately gratuitous. At the same time we have no proof that the science of "energy" affords the ultimate criterion of natural truth; and we cannot recognize that it has done more than *modify* the belief in a special vital force, though the modification has been so deep as to convert it from being an encumbrance to an effective aid to scientific progress.

How far our progress, thus emancipated from a serious impediment, may be expected to go, is a question which must be relegated to metaphysics; at the same time we hold that the results of physical inquiry have as yet given no warrant for anticipating, as the realizable ideal, that our science will ever overleap the barrier of structural organization. The limitation herein expressed seems to us so obviously to define the *natural* attitude of the chemist towards living matter, that not without the strongest proofs will he change it for the extreme of conceivable ideals; and in the practical work of investigating the products of life and growth he will find such abundant confirmation of his natural impressions as to remain convinced that the distinction of the material universe into animate and inanimate is real and transcendental*.

* This subject will be found exhaustively discussed in 'Chemical Difficulties of Evolution,' by J. J. McLaren (1877).

Chemistry and biology occupy to one another an antithetic relation as regards their subject-matter : the goal of the one is the starting-point of the other; the protoplasm as yet undifferentiated, to which the biologist complacently refers the origin of life, is to the chemist a perfect microcosm, and its synthesis perhaps the highest possible achievement of his science. With regard to the supervening phenomenon of organization —the entrance of life—there is also a distinction of attitude. To the biologist it is axiomatic—beyond that, a fact without the range of observation, and one of which we venture to say, therefore, he can give no account. Now the chemist finds his attention challenged, and his work of speculative investigation begin, at an earlier point in the history of the planet, when as yet life was impossible ; his speculations moreover have a certain basis in observation and analogy; and having initiated his career, why break off and affect to view with veiled eyes a *change* merely in the disposition of the matter and force already existent and active? Why, further, should we attach to the entrance of structural organization a mystic significance, when not only are we familiar with the inverse change from the organized to the amorphous condition, but find it in many cases to involve no change in the inner subsensible molecular structure, or at least no deeper change than can be included within the province of isomerism? Cellulose, for instance (in the form of cotton), is dissolved by the ammoniocupric reagent, and on adding excess of acid is reprecipitated, and may be recovered without loss of weight ; the change undergone cannot, therefore, be more than morphological. Further, the amorphous cellulose behaves towards reagents so similarly to the original, that the change in question appears to have affected merely the external structure. Add to these considerations many others of a similar character, which need not be here specified, together with the numerous syntheses of the products of life and growth which have been achieved in the laboratory, and lastly the narrow range of physical conditions under which life is possible, and we have a fair conception of the intellectual position from which might emanate the dogma, "we have ceased to believe in a special vital force." We, speaking personally, see no more reason, in these teachings of molecular philosophy, for ceasing to believe in life as resulting from special agency, than for ceasing to recognize the living individuality of our English language or constitution, and the agency of Englishmen in their establishment and development, because they are originally a rearrangement of materials and forces once the possession of now obsolete or effete nationalities. We are not aware that in any philosophy,

worthy the name, life was or is regarded as any thing more than a rearrangement and special disposition of the preexisting: it cannot be doubted that life, as known on this planet, had a beginning; nor, further, that the whole analogy of the origin of subsequently derived life points to it as an individual and special phenomenon. In a word, the doctrine of Spontaneous Generation has been expunged from biological science; and a revival of its analogue from the point of view of chemical science we hold to be groundless. Not only so, but, to give a more practical expression to these views, a loose adoption of the non-belief in a special vital force will materially impede progress in the domain of biological chemistry. We cannot find a stronger proof of the want of recognition of the special and peculiar nature of the chemical phenomena of life than in the prevalence of empirical methods for the resolution of plant substances, their empiricism consisting chiefly in their unreasoned foundations upon the methods of separation of inorganic substances. To this portion of the science our attention has been specially directed; and we proceed to discuss it in certain of its bearings upon the main subject of this paper.

The work of the plant, considered in its intrinsic results, is to grow, to form tissues and organs; a side issue of growth is the elaboration of substances which subserve future growth; and a subsidiary result is the formation of certain substances which we may regard as excreta, as unavailable for the main end. These excreta are often bodies of well-defined physical properties, of more or less simple molecular structure, whose constitution is so far comprehended as to allow of their synthetical production in the laboratory. For the isolation of such bodies, methods founded upon the fixity of their constitution are general and satisfactory. But, on the other hand, substances whose essential condition is that of continual differentiation, whose constitution is but little understood even when viewed statically, whose relationship to the former group is probably of a parental character, should be treated with due regard to these distinctions: indeed it seems hopeless to attempt to comprehend their chemical functions in disregard of these their special biological correlations. The neglect of these considerations has led to the adoption of empirical and, to that extent, arbitrary methods of analysis and classification; names have been multiplied to individualize bodies which, occurring in a developmental series, should have received a corresponding general or group definition; and much time and capability have been spent in establishing facts before their probable and relative value had been taken into consideration.

The chemistry of cellulose is a case in point. This substance will be found, even to this day, treated by chemists as a well-defined body, of intrinsically fixed characteristics; whereas physiologists, with special regard to its functions in the living plant, have long since observed it to be capable of many modifications, more or less profound, and have not hesitated to regard it as the parent substance of that large class of aromatic bodies of which tannin is the type.

Geology had made us acquainted with the very profound modification of cellulosic structures which are seen in coal; and the chemical study of coal in its various forms had revealed a progressive increase in aromatic potentiality; and yet the obvious generalization of these several results has been widely ignored, the genetic connexion suggested by this large group of naturally occurring substances has been practically neglected, and chemists have remained satisfied with a purely empirical treatment. Taking this connexion as a working hypothesis and investigating the essential relations therein suggested, we feel assured that a much more productive field of inquiry is opened out. The essential difference between coal and cellulose is measured by the difference of their products of decomposition by dry distillation: in coal we have the source of the vast series of aromatic compounds which constitute the subject matter of the most important development of our science. Although the constitution of coal is still unsolved, and we cannot yet say to what extent it might be made to yield aromatic bodies by less drastic processes of resolution than that of destructive distillation, yet the general fact that in relation to cellulose it possesses an increased (immediate) aromatic potentiality has been sufficiently established. The nitrogen, further, derived from the proteid matters of the parent tissues exhibits a progressive diminution; and the chemistry of the formation and decomposition of coal may be considered independently of this element*. It remains therefore to investigate the conversion of cellulose into substances of the aromatic group of compounds.

The first link in the chain of development would appear, from the researches of physiologists, to be contained in the phenomenon of the lignification of cellulose structures. In the life of the plant, extreme processes of reduction and oxidation, of synthesis and resolution occur simultaneously and continuously. The formation of cellulose and its lignification have been ranged by physiologists on the basis of this antithesis. The connexion between lignin and the great group of

* W. A. Miller, 'Elements of Chemistry,' iii. p. 145, by whom this subject appears to be *developed* more consequently than by most writers

astringent substances has been already established*; and the latter have been assumed by physiologists to be residues of the oxidation of carbohydrates. In regard to the lignification of structures originally consisting of pure cellulose, we have two alternative hypotheses to account for the change:—(1) that it results from an intrinsic modification of the cellulose itself effected *in situ*; and (2) that it is the result of combination of cellulose with aromatic bodies formed elsewhere in the plant, and probably as residues from the oxidation of carbohydrates. There is perhaps more negative evidence on the subject of the second than there is positive in favour of the first hypothesis; and the difficulties which beset the elucidation of the question are an apt illustration of the limitations which impede the solution of the chemical phenomena of life. If the cellulose combine with astringent substances presented in solution, to form insoluble products, these may be assumed to resemble those compounds which form the basis of the dyeing of the cellulose fibres. A superficial study of any of the non-cellulose or lignified fibres will satisfy the observer that they behave rather as modified celluloses than as a compound of the above weak order. In the first place, the resolution of the fibre-substance of lignified fibres can only be effected by means of drastic reagents, whereas a compound of cellulose with a body that it has merely removed from solution (that is to say, in what we may term adhesive combination) is always easily decomposed. We have further made a series of observations upon seedlings, which show that the astringent substances formed during germination are present in the juices and yet absent from the substance of the fibro-vascular bundles. Again, if lignification followed the course in question, it is difficult to account for the comparatively invariable composition of bast-fibres. Not only are they remarkably uniform in composition from end to end, but they may be obtained white and lustrous by a process of bleaching which occasions a minimum loss of weight; they may be converted into explosive nitro-compounds, an examination of which shows them to be homogeneous; they are soluble in the ammonio-cupric reagent, a property which has been denied to them by certain observers; and the precipitate obtained on adding an acid is simply the amorphous modification of the originally organized fibre-substance. The characteristic yellow coloration moreover which lignified fibres give when treated with a solution of aniline sulphate, and upon which much stress has been laid as distinguishing them from the

* Sachs. *Handb. der exper. Phys.* (1865) pp 361–369; Sachse, *Farbstoffe* &c. (1877), p. 113; Cross and Bevan, Chem. Soc. Journ. xli. p. 90.

cellulose fibres, we have found to be so considerably weakened by previously boiling the fibre in a solution of acid sodium sulphite as to be afterwards in many cases quite undistinguishable. As the loss in weight due to this treatment is imperceptible, and the lignin substance remains practically unchanged, the colour-reaction in question is referable to the presence of some body resulting from a superficial decomposition (oxidation) of the lignin substance. These observations are a strong confirmation of the view enunciated with much emphasis by Sachs* many years ago, that the only inference to be drawn from the biochemical facts then established is that lignin and cellulose are genetically connected.

In our early work on bast-fibres, we were led to regard the jute-fibre as typical of a class of bodies analogous to the glucosides, and which we termed cellulides, a name which sufficiently explains our views. Subsequently to this, we found that Hlasiwetz† had arrived at similar conclusions in discussing the chemico-physiological relationships of the tannins, phlobaphenes, resins, and glucosides. He not only regards cellulose, tannins, and resins as genetically connected, but is convinced of the *à priori* probability of the existence of series of gummides and mannides parallel with the glucosides. We take this as an additional warrant for the correctness of our view; and in conformity with these conclusions and subsequent experience, we may state our hypothesis with more emphasis to be, that the fibre substance of lignified fibres is, in its chemical constitution, dominated by the cellulose molecule, upon which aromatic molecules, resulting from intrinsic modification of the cellulose itself, have been built. Whether the aromatic molecule is of the nature of a quinone, as would seem to be indicated by the products of the action of chlorine, we have some hesitation in affirming, recognizing more clearly than we then did the difficulty of reasoning from the products of decomposition of once living substances back to the condition under which they are formed.

The celluloses, which constitute the framework upon which the plant world is developed, being regarded thus as capable of modification, and lignification as the first of the series of changes through which they pass from the group of carbohydrates, to which they originally belong, to the extended range of naturally occurring substances of aromatic character included in the large group of astringents, in the several varieties of coal, and probably also of other important groups,

* Sachs, ibid. *Cf.* Koroll, *Quant. Chem., unters. Zstg. Kork, Bast* &c.: Diss., Dorpat (1880).

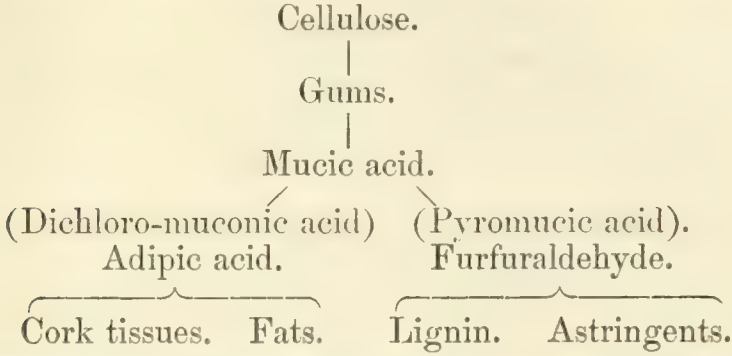
† Chem. Soc. Journ. xxxviii. p. 666; *Ann. Chem. Pharm.* cxliii. p. 40.

such as the resins, it is surely incumbent upon chemists to recognize the call to investigate the natural origin and history of the carbon compounds, and first of all in their relation to cellulose. We know the objection in the minds of many to forsake the familiar landmarks of positive physical definition, such as crystallization and molecular volume, for a province where the absence of the criteria hitherto regarded as all important makes the results to be obtained appear to that extent conjectural; we know indeed that the objection in many cases takes the more active form of almost refusing credence to any results obtained with substances that are amorphous and essentially transitional; and against this attitude a most emphatic protest is to be lodged. Arithmetic cannot cope with the physics of living matter; and we shall need to promote our equations and constants several degrees before we can include its chemical phenomena. Moreover the purity of substances, as the only condition in which to be approached by the chemist, will need a very elastic interpretation in presence of matter undergoing differentiation; and such properties as have hitherto been regarded as affording the only guarantees of purity will have no place in a vast amount of research that requires to be done. We clearly recognize the large amount of work already accomplished by isolated effort in this department of chemical science; but these remain for the most part uncorrelated, and, as a glance at the text-books will show, in a great measure unrecognized.

Not only have the suggestions of physiologists in regard to the probable origin of aromatic substances in the plant been but little developed by chemists, but the equally important correlation of the carbohydrates with the fats, which follows from their physiological equivalence, still lies without our science. Here also a transformation in series is suggested, the intermediate terms of which are probably to be found in cutin and analogous bodies, constituents of cork structures. The changes through which the transition is accomplished are probably very profound, more so than in the conversion of cellulose into lignin. Of the mechanism of the conversion we are as yet entirely ignorant; but we have the conviction, in this as in every other case, that the vital force of the plant operates through the same materials and forces which lie at our disposition, and that its results can therefore be studied and in some measure reproduced.

The study of these transformations must, in the first instance, of course be analytical; and most important correlations will follow from a comparative examination of the products of resolution of plant-substances. Take, for instance,

mucic acid, and represent its connexions by a diagram, and we find a large number of very important correlations implicated, thus:—



A quantification of these relationships, and a multiplication of investigations according to such *à priori* correlations, could not fail to establish important truths concerning the genetic connexions of the carbon compounds.

A further illustration of the want of correlation of chemical with biological investigation is to be found in the prevalence of empirical and statistical analyses of plant-substances in agricultural work. To a certain extent it is evident that statistical must prevail over molecular methods where the complex substances and mixtures of substances which are elaborated in the plant world are the subject of inquiry; but we contend that these may be ordered on a much more rational basis than is at present adopted.

The somewhat arbitrary choice of reagents, such as Schulze's for the isolation of cellulose, of boiling dilute sulphuric acid and alkali successively applied to plant-substances for the determination of their so-called crude fibre, the dismissal of whole groups of bodies as "extractive matters" or as "incrusting and intercellular substances," and the general absence of the recognition of the genetic connexion of these substances with those from which they are separated—in fine, the almost exclusive choice of the indirect and statistical before direct methods of observation, argues a certain misapplication of time and capability, and sufficiently accounts for the indifferent, if not critical, attitude of the greater number of chemists towards Biochemical science. We may cite the literature on the subject of chlorophyll, the proteids, the carbohydrates, including cellulose, the group of pectous substances, and, until the recent work of O'Sullivan and Brown and Heron, starch. A more special illustration is to be found in the work of Meissner and Shephard on the origin of hippuric acid in the urine of Herbivora. In order to identify the parent substance—that constituent of grasses which could yield the necessary benzoic

residue—the authors, adopting the statistical method of exclusive separation, arrived at length at the substance in question —a body which Foster*, in quoting their work, terms a form of cellulose. This substance was found to differ from cellulose, on the result of an aggregate elementary analysis, by a somewhat higher carbon percentage, such as, according to the reasoning of these authors, would be due to the presence of a body of the composition of quinic acid; and this they regard as a constituent of this substance, and as converted, during the process of digestion, into hippuric acid. Pushing then the statistical method of inquiry beyond its limits, and evidently for the purpose of confirming *à priori* views, the authors appear to us to have missed the most important development of their otherwise valuable work. Had they examined the "form of cellulose" at which they arrived by a direct method of proximate resolution, they would have obtained the aromatic substances, allied to the astringents and phenols, which we have obtained from lignified fibres. That they are digested by the Herbivora has been established by numerous observers†; that they are the source of the benzoic residue necessary to form hippuric acid is *à priori* very probable: in fact it must be regarded as first in the order of the probabilities to be investigated. As such, indeed, it is occupying our attention; and whatever be the result it will be more valuable, because more definite, than any conclusion arrived at by means of the indirect method.

We refrain from extending our discussion of these subjects in anticipation of the more detailed publication of our researches. We think we have shown grounds for our statement that there is a general want of correlation of chemical with biological research, especially in the hesitation on the part of chemists to adopt, as working hypotheses, the wider-reaching conclusions of physiologists as to the natural origin and history of the carbon compounds. We have also endeavoured to show that, while our conception of the vital force has been modified so as to have entirely lost the significance that belonged to it in a previous age, we have no ground for dismissing it for the alternative view of life as immanent in the universe of matter and force. We have expunged an error that was partial, and are in danger of generalizing the negative by which it needed to be met.

Postscript.—Since writing the above, our researches have

* *Die Hippursäure* (Hannover, 1866); 'Text-book of Physiology,' 2nd edit. p. 354. *Cf.* Weiske, *Zeitschr. Biol.* xii. p. 241.

† Weende, *Berichte.*

established a connexion of the closest order between the aromatic portion of the molecule of lignin (bastose) and the triatomic phenols—a fact which considerably strengthens the views advanced by physiologists as to the correlations of the carbohydrates with the aromatic group, and the reasoning by which we have sought to emphasize them. The researches in question, on this point, will be published in due course.

XL. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* *By* OLIVER J. LODGE, *D.Sc.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE discussion which has been carried on in your pages* respecting the dimensions of a magnetic pole serves to illustrate the divergency of thought between those in this country who have been brought up, electrically, under Faraday and Maxwell, and the continental philosophers so eminently represented by Prof. Clausius. From one point of view the discussion may be said to have been roused by a simple mare's nest constructed by dropping a factor out of one side of an equation (as was pointed out at once by Prof. Fitzgerald and by Mr. J. J. Thomson); but from another point of view it is the natural and inevitable consequence of the different aspects from which these matters can be regarded:—the English standpoint, in which the medium is recognized as the active agent, and is continually present both in the mind and in the formulæ; and the continental standpoint, from which the medium is perceived as so much empty space, and is taken account of as such in the formulæ. Both these aspects of the subject are worth consideration; and it may be conducive to future clearness to discuss them at moderate length.

Coulomb's measurements provisionally established the fact that in air the mechanical force between two electrically charged bodies was proportional to ee'/r^2; but the subsequent researches of Faraday proved that this proportionality only holds so long as the medium enveloping the bodies is unchanged, and that the above quotient must be multiplied by different factors in order to give the force exerted in different media. Thus if the same two charged bodies were immersed in bisulphide of carbon, they would repel one another with much less vigour than they do in air.

Introducing therefore as a factor the electric inductive

* Phil. Mag. [5] vol. xiii. pp. 376, 381, 427, 429, 431, 530; and vol. xiv. pp. 124 & 225.

capacity K, we have the general equation

$$F = \frac{ee'}{Kr^2}.$$

Now all that is solemnly essential with respect to the dimensions of the quantities here involved is that e^2/K must be a force into an area, or that

$$[e] = [L][KF]^{\frac{1}{2}}.$$

If we proceed to define the unit of electricity so as to make K of no dimensions and to equal 1 for air, that is a convention, and it is the basis of the electrostatic system; but the above statement is no convention, but a natural truth.

Precisely the same kind of thing is true in magnetism; and we now know that the force between two magnetic poles is not independent of the medium surrounding them, but that if the torsion-balance were full of, say, ferric chloride, the force between the two poles would be measurably less than if it were full of mere air. Thus we again need a factor for completeness; and the real law is that

$$F = \frac{mm'}{\mu r^2},$$

or that

$$[m] = [L][\mu F]^{\frac{1}{2}}.$$

The conventional *magnetic* system of units is based on the definition of m in such a way that μ, the magnetic inductive capacity of the medium, shall be of no dimensions, and shall for air be simply 1.

All then that is objectively and physically fixed about the matter is that the dimensions of $e \cdot \sqrt{K}$ and of $m \cdot \sqrt{\mu}$ are absolutely and mechanically definite, being each of them a length into the square root of a force, or

$$M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}.$$

But observe that the two system of units, the electrostatic and the magnetic, the arbitrary definition of e and the arbitrary definition of m, are in their origin utterly independent; not that they are *unrelated*, but their relation must be a matter for future and experimental investigation. All we can so far say about them is, that, in every possible consistent system that can be adopted,

$$[eK^{-\frac{1}{2}}] = [m\mu^{-\frac{1}{2}}] = [LF^{+\frac{1}{2}}] = [MK^{-\frac{1}{2}}]. \quad . \quad . \quad (1)$$

(The last term of this triple identity is added for the sake of completeness, though it does not directly belong to the present

subject : the letter k is intended for the gravitation-constant as determined by the Cavendish experiment. I am not aware whether the question of the possible dependence of this constant on the optical density of the medium surrounding the attracting masses has ever been considered; but I feel sure that a direct experimental attack on this question would not be uninteresting, and it might lead to important results.)

We now come to the Œrsted-Ampère discovery of the connexion between m and e—the form of the connexion being that an electric current flowing in a closed circuit can produce a magnetic potential, and therefore of course can act on magnets, precisely as if it itself were a magnet of a certain strength and form. The potential so caused at any point in air is found to be simply proportional to the strength of the current and to the solid angle which the circuit subtends as seen from that point ; or, in other words, the moment of the magnet which is equivalent to the current is simply proportional to the strength of the current and to the area of the contour round which it flows.

The unit of current most simply and directly applicable to these electromagnetic phenomena is not the old electrostatic unit at all, but a new unit which may be defined in many ways—as, for instance, these :—

The electromagnetic unit of current is that which produces unit magnetic potential at a point whence its circuit subtends unit solid angle;

It is also that which produces unit magnetic intensity, in a given direction, at a point whence the solid angle subtended by its circuit is changing at unit rate, per unit displacement, in that direction;

And, again, it is that current which when flowing round a contour of unit area is equivalent to a magnet of unit moment,—

all these statements being derived directly from the unit magnetic pole thus :—

Unit magnetic potential is defined to exist wherever a solitary and stationary unit pole would possess unit energy ;

Unit magnetic intensity exists wherever unit pole would experience unit force ; and

Unit magnetic moment is that possessed by two unit poles of opposite sign rigidly connected by a bar of unit length.

(The connexion between the old electrostatic unit and this new electrical unit thus magnetically defined may be expressed, if I am not mistaken, by saying that a ring charged with the electrostatic unit of electricity would have to revolve in its own plane with an angular velocity of about 3×10^{10}

radians per second in order to produce the same magnetic effects as the electromagnetic unit of current flowing in the same ring.

Or, conversely, the electrostatic unit magnetic pole would be that which would experience unit force if placed at the centre of a circle of unit radius in which the electrostatic unit of electricity was moving with unit velocity.

This definition I believe to hold equally well in any homogeneous medium; for it is pointed out below that the electromagnetic effect of a current is independent of μ; while as regards K, a quantity which we might perhaps think would be likely to affect the result, we must remember that electric displacement is totally independent of any such circumstance. So, corresponding to the common electrokinetic equation,

$$\text{Force} = m\int\frac{C\,ds}{r^2},$$

we shall have, for a moving charge,

$$\text{Force} = \frac{mev}{r^2},$$

whence

$$[me] = [\mathrm{M L^2 T^{-1}}]. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If statements like these are in the main correct, and after the experiments of Rowland we are bound, I suppose, to believe in the truth of something of the kind, they ought to remove Dr. Everett's objection (Phil. Mag. June 1882, p. 431) as to the introduction of electrostatic units into magnetism; unless indeed he maintains the thesis—no doubt a tenable one—that directly you begin to carry a charged body about, the discussion of its performances no longer belongs to electro*statics*.)

Returning from this digression, we have now to ask whether the statements above made are really definite and independent of the magnetic properties of the medium surrounding the circuits, or must we introduce a factor to express the influence of this medium when it is other than air?

Mr. J. J. Thomson has instructively raised this question (Phil. Mag. for June); and he and others at Cambridge consider it a matter to be settled by experiment; and they further consider that, in order to agree with Maxwell's view, experiment ought to make the magnetic effect of a solenoid and its air-equivalent magnetic shell differ, as soon as they are both introduced into some medium for which μ is not unity. Now, though agreeing with this as far as it goes, I venture with diffidence to think that Maxwell would have drawn a distinc-

tion between the medium inside the region of the solenoid corresponding to the substance of the magnetic shell, and that outside. He over and over again lays stress upon the fact that artificial solenoids can only be compared with magnetic shells for the space outside the shells, and that the line of integration must never be allowed to thread the circuits. Let us follow this out and see what it means when applied to the above question.

I will assume it possible (for it certainly is theoretically possible) to imitate any steel magnet whatever by a proper arrangement of electric circuits, both being at present immersed in a non-magnetic (*i. e.* non-*magnetizable* or $\mu=1$) medium. The two arrangements are completely equivalent for all the region outside both—*the region outside both being defined by the shape of the steel.* For the comparison is not to be urged within the steel, because of the magnetized surfaces, which would have to be cut through, a circumstance which would completely alter all the conditions ; and it is not to be urged within the region of space near the solenoid which is the counterpart of the steel-occupied region, simply because here there are *no* magnetized surfaces to be cut through, and therefore the conditions will be continuous.

Now take some non-magnetic medium, which for shortness I will call "clay," mould it into the shape of the steel, and place it in or around the solenoid so as to mechanically define the limits of the "outer region." And now immerse both magnet and solidified solenoid in any medium for which μ differs from unity: I venture to assert that the equivalence which existed in air will be entirely maintained in the magnetic medium—even though that medium be iron or bismuth, —and that, for both, the magnetic intensity at any point will be its air-value divided by μ.

Still keeping both the things in the magnetic medium, remove the clay from the solenoid and permit the medium to flow into the space it occupied. If what I said before is true, the solenoid will now be too strong for the magnet, for the magnetic permeability of the interior will increase its effect μ times, while that of the exterior will, as before, diminish it $\frac{1}{\mu}$th ; so that the effect of the solenoid completely immersed in the medium is precisely the same as it was when in air, while the effect of the magnet, from whose interior the magnetic substance is of necessity excluded, is still $\frac{1}{\mu}$th of what it was in air. This latter seems to be the kind of experiment which Mr. J. J. Thomson suggests in his June letter (p. 429), and

which, he says, Mr. Sargant then intended (and I hope still intends) to carry out.

The solenoid being now completely surrounded with homogeneous magnetic substance, wall a portion of it in with paper or glass to the shape of the steel of the magnet, and then pull both magnet and solenoid out into the air again. Naturally the solenoid will still be μ times too strong for the magnet, but no further discrepancy need be expected; and if the current of the solenoid has been weakened when inside the magnetic medium so as to restore the disturbed equivalence, they will remain equivalent when the region external to both is again filled with common air.

These statements, if in their essence granted, require to make them complete certain provisos about the boundary of the vessel containing the magnetic medium, unless it be infinitely large, and also a discussion of what happens in the case of more than one magnetic medium. But the magnetization of bounding surfaces, and the accidents which happen to lines cutting surfaces of discontinuity, are perfectly understood and need not be here entered into.

Moreover, in making these statements I am merely saying what one would *expect* to happen without evidence to the contrary; but I am not for an instant implying that direct experimental investigation is unnecessary and would not be highly desirable. On the contrary, I think it would be most desirable and satisfactory to have the matter thoroughly sifted.

Supposing, then, that I have so far made no mistake, we can make the general statement of the equivalence of a current and a magnet thus—

The magnetic moment of a circuit is equal to the strength of its current multiplied by its effective area and again multiplied by the magnetic inductive capacity (or permeability) of the medium in the interior of the region enclosed by the contour (which region for a simple plane circuit is a mere shell), but is wholly independent of the magnetic properties of all the rest of the surrounding medium.

The corresponding dimensional equation is

$$[m] \equiv [\mathrm{L}\mu e \mathrm{T}^{-1}].$$

Substituting in this the value of $[m]$ from (1), we obtain the relation

$$[\mu \mathrm{K}] \equiv \frac{1}{v^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

whence

$$\left[\frac{e}{m}\right] = \left[\frac{\mathrm{K}}{\mu}\right]^{\frac{1}{2}} \equiv [\mathrm{K}r] \equiv \frac{1}{[\mu r]}. \quad . \quad (4)$$

These relations must all hold in any consistent system of units, since they express physical truths; but of course they are not all independent. The number of independent relations must be limited by the number of fundamental experiments, viz. three—Coulomb, Coulomb, and Oersted; and the shortest way of writing the independent relations is this :—

$$\left.\begin{aligned} &[\mu e^2] \equiv [\mathrm{K}m^2] \equiv [\mathrm{ML}] \\ \text{and} \quad &[\mu \mathrm{K} v^2] \equiv 1. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (5)$$

The electrostatic convention makes $[\mathrm{K}]=1$; the electromagnetic convention makes $[\mu]=1$.

So far every thing being pretty clear and straightforward, we have now to ask how it was that Prof. Clausius should have jumped to the conclusion that Maxwell had fallen into error*, or else that he held a theory of magnetism different from (and not merely an amplification of) Ampère's and Weber's†. With this latter horn of the dilemma, by the way, he is half allowed by Mr. J. J. Thomson (September) to have succeeded in transfixing Maxwell; and Dr. Everett (June) is not extremely energetic in his repudiation even of the alternative of the first.

Now it is perfectly true that Maxwell, in stating the current theory of magnetism, says without any kind of retraction or hesitation that "the magnetic action of a small plane circuit at distances which are great compared with the dimensions of the circuit is the same as that of a magnet whose axis is normal to the plane of the circuit, and whose magnetic moment is equal to the area of the circuit multiplied by the strength of the current. . . . . and if a magnetic shell . . . . &c. be substituted . . . . then the magnetic action of the shell on all distant points will be identical with that of the current." And in discussing Ampère's theory, he ignores the existence of magnetic media whose μ does not equal 1 as completely as Prof. Clausius could wish.

But then, according to Weber's extension of Ampère's theory (an extension I suppose universally accepted), the properties of magnetic substances of all kinds are explained by molecular electric currents, and no magnets or magnetic substances other than those consisting of current-conveying molecules exist. [And with reference to a remark of Mr.

* Phil. Mag. June, pp. 387 & 392. † Ibid. August, p. 126.

Thomson's in the September number, p. 225, I may say in passing that it seems to me that Maxwell held, though no doubt tentatively and hypothetically, the view that electric currents and small magnets are *identical* and not only *equivalent*.]

The coefficient μ is thus foreign to Ampère's theory applied universally; and this is how it has happened that Prof. Clausius has failed to recognize its existence and has been led into error*. A system dealing with Ampèrian magnets in media for which μ does not equal 1 is a mongrel combination which may no doubt be occasionally convenient but which never can be thoroughly satisfactory.

We may accept then without hesitation Clausius's presentation of Maxwell's views, viz. both that a small magnet *is* an electric current, and that *magnetic moment* ALWAYS *equals simply integral current* × *area*—remembering, however, that there exist currents in molecules besides the gross and artificial currents in our copper wires, that these are directed by our artificial currents and add to their effects, and that in all cases they are most distinctly to be taken into account.

In air, so far as it is non-magnetic, these molecular currents are zero, and the magnetic induction and the magnetic force are everywhere equal; but in media consisting of Ampèrian

* Very many errors, I now find; for he has also ignored K, Faraday's simple old electrostatic constant; and accordingly his equations (1), (2), (4), &c. express mere conventions (if they were any thing more, then truly m_s would have to equal m^d, and $e_s = e^d$, as he begins to perceive in his August letter); while his general equation (3), which is the foundation of his reasoning, is quite wrong, and is indeed at the bottom of the whole confusion. In using the term "error" here, I would be understood to mean rather "divergency from opinions commonly held in this country" than absolute incorrectness as to matter of fact. For it would not be becoming to apply the latter term to views held by Prof. Clausius when the experimental foundation of opposing views is confessedly incomplete. The views held by Prof. Clausius are no doubt perfectly consistent, and would probably be in accord with fact if only the medium produced no effect such as it is here commonly supposed to produce; and whether the medium does or does not produce such an effect appears to some extent at present a subject of legitimate debate and a matter for experimental investigation. It will be understood therefore, that in stating one side strongly I have been influenced by the wish to be clear, rather than with the desire to dogmatize.

Since the above letter was in type Dr. Francis has kindly called my attention to a paper by Prof. Helmholtz in Wiedemann's *Annalen*, No. 9, 1882, to which I might have further referred if I had known of it in time. As far as I can hurriedly understand his position, Prof. Helmholtz in part endeavours to reconcile the views of Maxwell and of Clausius by throwing a doubt upon the Weber-Ampère theory: and in fact he appeals to pure physicists not to abandon the old electrostatic for the more cumbrous and less surely founded electromagnetic system.

[A translation of Professor Helmholtz's paper will appear in our next number.—Ed. Phil. Mag.]

molecules there is an extra magnetic induction, due to the pointing of these along the lines of force, which is 4π times the magnetization, and which has to be added to the other, thus making the total magnetic induction at any point μ times the magnetic force there.

The effect of the medium is a physical fact; and no theory can presume really to dispense with the constant μ. All that the Ampèrian theory does is to give a physical interpretation to it, and to render one independent of it so soon as one takes account of every current-conveying circuit, whether molecular or other, existing in the field, and does not arbitrarily elect to deal only with those gross solenoids which we can excite and immediately control by batteries.

There can be no doubt, I think, that the mind of Maxwell on this subject, as on most others, was as clear as daylight; and so far from falling into the least suspicion of an error, he expresses himself in art. 615 (1st edit.) almost as if he were joining in the present discussion, saying :—

"There is one result . . . . which is of very great importance. If we suppose that no magnets exist in the field except in the form of electric circuits, the distinction which we have hitherto maintained between the magnetic force and the magnetic induction [and therefore also the difference $\mu-1$] vanishes, because it is only in magnetized matter that these quantities differ from each other. According to Ampère's hypothesis . . . . . the properties of what we call magnetized matter are due to molecular electric currents, so that it is only when we regard the substance in large masses that our theory of magnetization is applicable; and if our mathematical methods are supposed capable of taking account of what goes on within the individual molecules they will discover nothing but electric circuits, and we shall find the magnetic force and the magnetic induction everywhere identical. In order, however, [N.B.] to be able to make use of the electrostatic or of the electromagnetic system of measurement at pleasure we shall retain the coefficient μ, remembering that its value is unity in the electromagnetic system."

I am, Gentlemen,
Your obedient servant,
OLIVER J. LODGE.

University College, Liverpool,
September 28, 1882.

XLI. *On the Electric Discharge in Rarefied Gases.* *By* Dr. Eugen Goldstein*.

I.†

I HAVE shown in two former papers‡, and more completely in my book 'A New Form of Electrical Repulsion' (published by Springer, Berlin), that the discharge cannot be effected by the actual projection of gas-particles. The same considerations which oppose the theory of the propagation of electricity by projected gas-particles, also at once exclude the assumption that other ponderable particles, having access to the space in which the discharge takes place, play any essential part as carriers of electricity in the discharge. Such particles might consist of disintegrated portions of the substance of the electrodes, particles of the wall of the vessel, or of dust.

The theory that the kathode-rays at any rate are produced by projected particles of the substance of the electrodes has been recently defended by Gintl§ and Puluj‖. Numerous arguments¶ may be urged against it, over and above those which are at the same time opposed to a special convection by the particles of the gas. I will briefly mention one or two points. I have mentioned on a former occasion** that a system of pores in an insulator, or a single opening of relatively small dimensions, sends out rays whose properties are equivalent to those of the rays which issue from a metallic kathode. The rays of narrow openings, for example, possess the property of rectilinear radiation, and of exciting phosphorescence, which cannot here be explained by a projection of the substance of the pole.

Kathodes imitated by systems of pores represent special cases of the phenomenon of secondary negative light, which is

* Translated from the *Annalen der Physik und Chemie*, 1881, new series, vol. xiii. Communicated by the Author.

† The readers of this Magazine will find that some observations and conclusions in the first chapter of the above paper, concerning the conductivity of vacuum, do not differ from the views expressed by Prof. Edlund in a paper reprinted in the January number of the Philosophical Magazine for 1882. I beg to mention therefore, that my paper appeared in print in the February number (1881) of Wiedemann's *Annalen*, and that Prof. Edlund presented his to the Royal Swedish Academy, April 23, 1881.

‡ Phil. Mag. [5] x. pp. 173 & 234.

§ Gintl, 'Studies of Crookes's Radiant Matter' (Prague, 1880).

‖ Puluj, *Sitzungsberichte Wien. Akad.* 1880, p. 864.

¶ E. Wiedemann (Wied. *Ann.* x. p. 252, 1880, Phil. Mag. x. p. 418) thinks it possible to conclude from Zahn's experiments (Wied. *Ann.* viii. p. 675, 1879) that the view of Gintl and Puluj is untenable.

** Phil. Mag. x. p. 177.

produced with the properties of rectilinear propagation, excitation of phosphorescence, &c., at openings of any width, if these openings occur in diaphragms or in tubes connecting vessels or introduced into them, the area of which is considerable in comparison with the diameter of the opening. A consideration of these bundles of rays possessing the properties of the kathode-light issuing from wide openings also protects the experiments on narrow openings against the objection that the coincidence of their rays with the kathode-rays depends on a conductive action of particles of the insulating substance which might possibly be torn off from the edges of the opening.

It has further been shown* that the positive light also possesses the property of rectilinear propagation, and of exciting phosphorescence when the exhaustion has been carried far; it does not seem to be reasonable to adopt an explanation for the phenomena of the kathode-light the principle of which is not applicable to the exactly similar properties of the positive light.

The most convincing proof, however, is given by the following observation, which I have made in experiments on the kathodic deflection. This last name was proposed in the book referred to above for the deflection of the electric rays there described, of which I have made use† in determining the velocity with which the electricity propagates itself. I may assume it to be known that a kathode of aluminium produces no deposit on the walls of the tube even after several hours' use, whilst a kathode of platinum of no great thickness soon produces a completely opaque metallic deposit on the part of the tube played upon by the kathode-light. Two straight smooth wire electrodes, *a* and *b*, are inserted in the end of a cylindrical discharge-tube parallel to the axis of the tube. If both are made at the same time kathodes of the same discharge, then each of the two wires causes a deflection, in the rays of the other which pass by near to it, of the nature of a repulsion. We have then, as already described, two sharply-bounded surfaces, of which the one receives no rays from *a*, whilst none of the electric rays emitted by *b* fall upon the other. At the density of gas favourable to the production of phosphorescence, both surfaces are distinctly seen upon the surrounding brilliantly phosphorescing surface. This phenomenon is also observed when one of the kathodes *a* is of platinum, until the increasing thickness of the platinum-deposit prevents the phosphorescence of the wall.

According to the view of Gintl and Puluj, that portion of the glass wall on which no rays fell from the platinum elec-

* Phil. Mag. x. p. 236. † Ibid. p. 246.

trode ought evidently to remain free from the platinum deposit. If, however, we examine the glass wall, we find that the surface from which the kathode-rays are deflected—as determined at high pressures by the visibility of the blue rays themselves, and at low pressures by means of the phosphorescence which they excite—*is just as thickly covered with the platinum as the surrounding portion of the wall*, and exactly as we observe to be the case in cylinders where the platinum wire alone acts as kathode while *b* is not excited. It follows that the rays of the kathode-light are deflected, but not the projected particles of the electrode; the two cannot therefore be essentially connected.

The discharge is therefore not to be explained by a projection of material particles, either of the substance of the electrodes or of the gas. It follows from the experiments on the order of magnitude of the velocity of propagation of electricity, taken together with the views held on the constitution of gases, that the assumption of oscillations of these particles does not afford a satisfactory explanation, and the assumption of motions of rotation remains equally unfruitful. The wall of the containing vessel is not altogether neutral in the passage of electricity through the space enclosed by it; it shows itself phenomena of charge and discharge which appear to be not altogether without influence upon the main discharge between the metallic electrodes. If we wish to go so far as to ascribe to particles possibly torn off from the walls of the vessel when they are discharged the same function which, in the view just considered, gas-particles or electrode-particles were unable to perform, this assumption, quite apart from all new objections, is open to all the objections urged against the previous hypothesis.

Recent investigations have shown that the dust suspended in gases plays an important and previously unsuspected part in the loss of electricity suffered by feebly-charged conductors in the open air, or in gases not specially purified. In almost all cases in which we have hitherto regarded a mass of gas as a carrier of statical electricity, we must now regard the dust suspended in the gas, in cases where drops of liquid cannot be present, as the only vehicle of the electricity. We might easily imagine an hypothesis ascribing a similar essential function to the dust in gases in the case of current electricity as in the case of statical electricity, but that the objections we have previously considered might easily be employed to refute an assumption of the kind.

The discharge cannot then in general be explained by the motions of ponderable particles; it follows therefore directly

from the experiments which prove this, that that medium must be essentially concerned in the discharge which, according to our present views, together with the gas-molecules, the particles of the electrodes of the walls, and any other solid substances which may be present, occupies the space in which the discharge takes place—that is to say, the æther.

According to my view, the discharge is a process which takes place *in the free æther*. I have already indicated this view in the work already several times mentioned, and will now give other evidence supporting the observation made there.

Hittorf* found that the resistance of the positive light always decreased as the exhaustion of the gas increased; on the other hand, he thinks he has shown that the resistance increases with the exhaustion in the kathode-light and at the surface of the kathode. Changes in the form and magnitude of the anode have no influence on the resistance. The great resistance which offers itself to the discharge at an extreme exhaustion, and finally leads to its extinction in a vacuum as perfect as possible, depends therefore altogether upon the resistance at the surface of the kathode and in the space filled by the kathode-light. After I had recognized that the peculiarities of the negative light might be produced at any point whatever of the column of positive light by simple changes in the cross section of the discharge-tube, and that each separate stratification of positive light is nothing else than a modified bundle of negative light, this opposition between kathode-light and positive light appeared to me just as doubtful as already a number of other supposed differences between the two, which I had found not to exist.

I found in fact that, exactly as with the positive light, *the resistance of the kathode-light at small pressures becomes vanishingly small in comparison with the total resistance of the discharge*. Hence, since no specific resistance exists at the anode, and since further, as already mentioned, the resistance of the positive light vanishes in comparison with the total resistance of the discharge, it follows that *the resistance to the discharge at very small pressures takes place entirely at the surface of the kathode*.

My experiments on this subject were not made by means of a galvanometer, like those of Hittorf, but by means of the spark-micrometer, which is here much more efficient. The spark-micrometer was included in a second circuit connecting the electrodes, and the distances of the poles of the micrometer compared for the different densities of gas and lengths of

* Hittorf, Pogg. *Ann.* cxxxvi. p. 1 (1869).

kathode-light at which the currents of the inductorium either cease to pass through the exhausted tube or else pass no longer through the micrometer*.

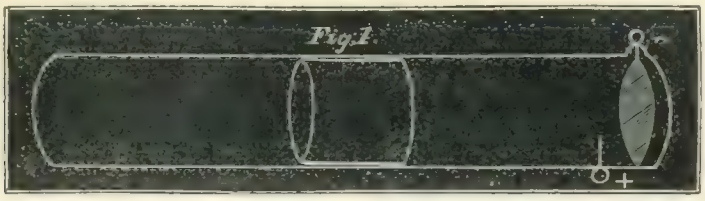
* The distances of the balls of the spark-micrometer corresponding to these two alternatives are not exactly identical. The discharges do not pass exclusively through the exhausted tube up to a certain distance of the balls, and then with a certain small decrease of this distance exclusively across the air-space between the balls; but between the distances of the balls at which the spark takes one only of the two courses open to it are to be found positions of the micrometer at which both paths are taken —sometimes the one, sometimes the other—and the one path the less frequently the nearer we come to the distance at which the other is taken alone. This apparently unstable character of the resistance in the tube does not affect the accuracy of the measurements here in question; we may compare the resistances by comparing the distances of the balls apart at which for a certain fixed time, say two minutes, no spark passes in the tube or between the poles of the micrometer. The distances so measured agree upon repetition to $\frac{1}{6}\frac{1}{0}$ millim.—that is to say, to a fraction of 1 per cent. of the distance measured. The alternation of the discharges with certain distances of the balls no doubt depends, partly at least, upon the same cause as the following phenomenon, which I have observed in the same experiments, and which, as far as I know, has not been previously described.

If we include the spark-micrometer in a branch circuit of an exhausted tube which transmits both the discharge at "make" and that at "break," then, if the distance between the balls of the micrometer be gradually diminished, the current at "break" completely leaves the tube and passes altogether in the free air, whilst that at "make" continues to traverse the tube with undiminished light. Consequently we are able to study the discharge of the "make" current in the exhausted gas separate from that of the "break" current; whereas hitherto a separation of the two currents has been effected by introducing an air-break in the direct circuit including the tube, with which arrangement the tube is traversed only by the discharge due to the "break" current. The reason of this phenomenon may lie in the different maximum tension of the current on "making" and "breaking" contact, and may correspond to the observation of telegraphists, that discharges of great tension (as observed in electrical storms) will sooner traverse a short distance through air in the form of a spark than traverse a long metallic circuit in the form of a current.

Such phenomena show that in the case of discharge through gases, the division of currents cannot be calculated according to Ohm's law. This we see from the so-called Holtz's "funnel-tubes." If two similar tubes are placed opposite to each other side by side in the same induced current, then at suitable pressure of gas the current does not subdivide itself between the two in any definite ratio to the resistances of the tubes, but the one tube remains entirely dark, the current goes altogether through the other.

The law according to which currents divide when the discharge takes place in gas must therefore be investigated empirically in the first place. I take this opportunity of remarking that I have made an erroneous assumption on this subject in a series of experiments in my book previously referred to, which, however, does not actually affect the result obtained. I believed myself justified in assuming as the evidence of certain phenomena (p. 146), that if a part of the discharge traverse a metallic circuit from the kathode a to an electrode b, and then the resistance of a moistened thread be introduced between b and a wire c, that then the

One of the experimental tubes is represented in fig. 1. A cylindrical tube is provided with a flat kathode placed



at right angles to the axis of the tube, and nearly as large as the section of the tube. The anode is placed close in front of it, or in other cases consists of a very short wire in the plane of the kathode itself. Inside the cylinder is a movable partition *c*, consisting of a short glass cylinder terminated by a plane surface at the end turned towards the kathode. In accordance with what I have noted on former occasions*, the positive light disappears for such an arrangement of the electrodes when the exhaustion has reached a certain limit, or it is confined to the immediate neighbourhood of the anode; the kathode-light, on the other hand, expands to any extent if the exhaustion is sufficient, and the expansion of its rays is limited only by their striking upon a solid wall. Hence, when a sufficient exhaustion has been reached, we can vary the expansion of the kathode-rays within wide limits by sliding the piece *c* (by inclining the discharge-tube and tapping it) along the tube; since its length is always equal to the distance between the kathode and the end-surface of the movable cylinder, which can be moved right up to the anode†.

If now the expansion of the kathode-light is made to vary in the ratio 1 : 30, the total resistance of the discharge at low pressures does not alter so much as in the ratio 1 : 1·05. Hence the resistance of the kathode is a vanishing quantity in comparison with the resistance at the surface of the kathode.

Hence we see that the resistance of the whole quantity of gas contained in a discharge-tube becomes smaller the more

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density of the discharge at *a* is not changed. There is in fact a certain change; but its influence upon the phenomena considered in the place referred to is so small that, upon repetition of these experiments with actual constancy of density at *a*, results were obtained partly exactly corresponding, partly nearly corresponding.

The examples given on p. 149 for the magnitude of the deflective power in a particular case consequently represent these values at least very nearly. The same method was employed (p. 131) to confirm a result obtained by two other methods; so that the result given there is not affected by the failure of the experiment in question.

* Goldstein, Phil. Mag. iv. p. 362; 'A new Form of Repulsion,' p. 8.

† In order to resist blows without deformation, the anode in these cases was made of strong iron or steel wire.

the density of the gas is diminished ; the space in which the discharge takes place conducts better the less gas it contains ; and since this change is always in the same direction while the density is continuously reduced, so far as the experiments can be extended, we must conclude that the greatest conductivity would be obtained after complete removal of the gas. But after complete removal of the gas the discharge-space contains only *free æther* ; and I therefore regard this as the true medium of the discharge. Any gas present seems to act only as a hindrance to the æther.

Detailed speculations on the concrete form of the motion of the free æther to which the discharge is due are, in my opinion, premature.

We are not justified in regarding the discharge as essentially a progressive motion of the æther so long as, on the one hand, we regard Doppler's principle as valid in optics, and, on the other hand, refuse to attach any considerable value to the progressive motion of gas-particles in the discharge. We must then ascribe to the motion of the æther amongst the relatively stationary gas-molecules an optical effect precisely similar to that produced by the motion of gas-molecules in æther at rest. Experiments on the constancy of gaseous spectra when observed in directions parallel and at right angles to that of the electric rays, show at once the absence of progressive motion of the æther (of velocity comparable with the velocity of propagation of the ray-discharge), after we have shown by other experiments* the relatively stationary condition of the gas-molecules.

It appears to me safest to characterize the motion of the æther in discharge as *radiant*, in accordance with what has been previously stated†. Every particle concerned in a pencil of negative light assumes the same form of motion which is excited at the point of origin of the pencil.

The behaviour of the discharge towards magnetic forces has frequently been brought forward in support of the view that the gas-mass forms the medium of the discharge, since this behaviour may obviously be explained by regarding the gas-molecules as carriers of electricity. If the magnet acts upon the positive light of an equatorially-placed cylindrical tube with electrodes at the end, and finally compresses the light which at first filled the width of the tube into a thin thread lying in the equatorial plane against the wall of the tube, it is generally assumed that the gas-molecules are compressed together with the electricity; I have not found this confirmed.

* Goldstein, 'A New Form of Electric Repulsion,' Chap. IV.

† Phil. Mag. x. p. 184.

A piece of metallic sodium was introduced into a cylindrical vessel 4 centim. in width and more than 20 centim. long, having the electrodes at its ends; and the tube was then quickly filled with dry nitrogen. After the tube had been so far exhausted that its positive light filled the section of the tube, the sodium was brought upon a part of the wall of the cylinder, in a horizontal position, played upon by the positive light. The sodium was next warmed until no more hydrogen was evolved, the tube was refilled with fresh nitrogen, and exhausted again to the same density as before. The sodium is then heated strongly until it begins to volatilize, and the discharge, which was reddish purple before, assumes a golden-yellow colour in its neighbourhood. If the heating be carefully managed, it is seen that the sodium vapour diffuses itself very slowly; so that the discharge in the upper part of the tube still shows the red colour due to the nitrogen, whilst it is of a golden yellow in the lower part of the tube. If the tube be brought in a horizontal and equatorial position near to a strong magnet, whose poles are so placed that the positive light is drawn upwards, the discharge, which at first filled the whole width of the tube, is concentrated into a thread of greater or less tenuity against the upper surface of the tube. But this thread possesses the pure purple colour of the nitrogen discharge without any trace of the sodium-yellow. The sodium vapour is consequently not displaced by the magnet, together with the current, as we are accustomed to see with movable carriers of electricity; the current seems to obey the magnet without affecting the gas-molecules. The result is exactly the same in experiments made with the Holtz machine instead of the induction-coil.

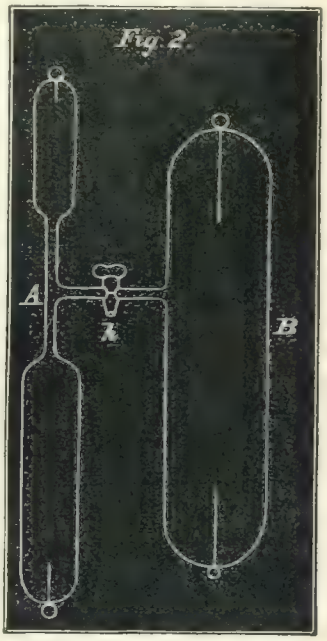


Fig. 2.

I have further examined whether it is possible to recognize a transport of gas-molecules by means of the magnet, in the local increase of density which must result from the assumed transport in a closed space traversed by the discharge. Two discharge-tubes, A and B, fig. 2, were joined together in the manner shown by means of a tube in which a stopcock was inserted. A second stopcock at the end of a short tube shuts B off from the pump during the time occupied by an experiment. The cylindrical portions of A were sufficiently long to show stratified positive light, at least in the cylinder containing the anode,

when pure dry air was employed. The distance between the similar boundaries of the different layers, or the number of layers in a given distance, is a very delicate test for change of density in the gas. Changes in the residual gas which correspond to less than $\frac{1}{100}$ millim. mercury are shown by very marked changes in the interval between the layers*.

The current of a coil was first sent through A, the number of layers in the anode-cylinder determined, and the boundary of each on the kathode-side marked on the tube with ink, thus marking the size of each layer. The current was regulated so as to give perfectly steady stratifications. Opening and closing the tap *h*, before or after turning on the current, did not affect the position of the stratifications. The combination was now brought near to an electromagnet while the current was passing and while the tap *h* was open, the poles of the magnet being so placed that the discharge appeared compressed towards the side of the tube B furthest from the tube A. If this concentration of the discharge depended upon an increase in density of the gas, then the gas in B outside the column along which the discharge passes must be rarefied, and gas from A would enter and rarefaction in A would result. After a few seconds *h* was closed, the current of the magnet was interrupted, the tube removed from the neighbourhood of the magnet to guard against the effect of any residual magnetism, and the discharge again sent through A. The number and position of the layers were found to be exactly the same as before the action of the magnet on the discharge.

The same result was obtained by operating as follows:— The initial density of the stratified discharge of an induction-coil through A was noted; then, after interrupting the current of the coil, the rapidly following discharges of a powerful Holtz machine were sent through B; and this discharge was subjected to the action of the magnet. On now again passing the current of the coil through A, there was no sign of any change in density; any change which had taken place must therefore have been less than $\frac{1}{100}$ millim. mercury; on the other hand, the change of density necessary to produce (when possible) effects upon the discharge, similar to those produced by the action of the magnet at constant density, must be measured by centimetres of mercury. The theory which makes the gas molecules the carriers of the current is therefore in no way supported by experiments with the magnet.

The view which I take of the part played in the dis-

* By the interval between two consecutive layers I understand here and elsewhere the distance between the bounding surfaces turned towards the negative pole.

charge by the free æther is commonly supposed to be refuted (where it is mentioned at all as a possible case, *e. g.* in text-books) by a reference to the experience of spectrum-analysis. If the æther were the vehicle of the discharge, it is said, all gases would give the same spectrum—the spectrum of æther—when subjected to the discharge. But since each gas has a special characteristic spectrum, the gass mass must be regarded as the conductor of the electricity. But it is well known that the æther itself has no power of emitting light. The fact that it has no "spectrum" is therefore no evidence that it cannot conduct electricity. With equal justice it might be argued that the phenomena of light and heat due to a current can only be produced in the molecules of the conducting substance; every conductor in whose mass non-conducting particles are embedded proves the contrary. The luminosity of a gas produced by the discharge depends entirely upon its molecules possessing the form and period of oscillation which are necessary for the emission of visible rays. That this vibrating motion is accompanied by motion due to electricity, executed by the particles themselves or their æther-envelopes, does not seem to be necessary; but, as the phenomena of fluorescence and phosphorescence in sunlight show, the molecules of bodies may execute motions of the form and period of the vibrations of light by taking up the vibrations of the surrounding free æther.

I assume that a similar process take place when a gas is rendered luminous by the electric discharge. The discharge itself represents a motion of the free æther, and is in itself non-luminous. This motion of the æther disappears, being communicated to the gas-molecules and their constituent atoms; the particles of each molecule then vibrate in accordance with their special structure and the conditions as to elasticity of the molecule, and communicate again to the æther the transversal vibrations so produced as such; thus the original motion which the æther possessed as electricity is converted into light, and of course into light whose oscillation-periods depend upon the specific nature of the gas-molecules. The difference in spectrum between chemically different gases thus in no way disproves the conduction of electricity by the æther. I thus regard the luminosity of gases traversed by the electric current as a phenomenon of resonance. I should not be disposed to regard it as a phenomenon of fluorescence or phosphorescence, for two reasons:—(1) Because in fluorescence and phosphorescence the vibrations are transferred from the æther to the atoms, and back again to the æther, without changing their character as transversal

vibrations; here, on the contrary, a motion of the æther which does not consist of transversal vibrations is converted into transversal vibrations. We have, however, a phenomenon analogous to this in acoustic resonance, where we see the longitudinal motions of particles of air transformed into the transverse vibrations of a resounding string. (2) I am disposed to reject the name phosphorescence for the phenomena under consideration, because, according to all the ideas which we have so far associated with the name phosphorescence, an hypothesis as to the temperature-conditions of the discharge would be introduced by the choice of this name, since the temperature of a gas is always supposed to be lower than that of a gas of like emissive power rendered luminous by heat. But this, even assuming that the conclusions of E. Wiedemann as to the temperature of the discharge should be confirmed by further experiments, is not yet accepted as the true character of the light of the discharge; and my speculations on the nature of the discharge do not in any way prejudice the question of the temperature of the discharge. For the present I leave this entirely out of the discussion.

The assumption that a vacuum conducts electricity has consequences which are far-reaching, especially in the domain of cosmical physics. The usual fate of attempts to found a cosmo-physical theory upon experimental results is scarcely such as to encourage imitators. At the same time I venture to point out at least so much as this, that certain terrestrial phenomena of an electric or magnetic nature which, because of the coincidence of their periods or epochs with solar changes, have been explained as due to the statical influence, magnetic induction, &c. of the sun's mass, might possibly be more conveniently referred to electric currents radiating through interplanetary space from the central body. Experiment shows no limit to the expansion of that remarkable motion which we observe in the kathode-rays as we eliminate the ponderable medium more and more completely—it is conceivable that the sun radiates electric rays as well as light-rays through space. We see that even when the two poles are placed close together, the kathode-rays stream out into space without limit, without reference to the position of the anode; consequently for electrical communication with the sun, it would not be necessary that the earth should be the source of electricity or pole of the current, but discharges for which both poles are situated on the sun might produce rays radiating from the sun into space.

II.

I consider two processes to be necessary for the production of the discharge:—(1) A change in the condition of the æther, preceding the discharge, which produces a certain condition of unstable equilibrium in the arrangement of its parts: this condition may be called, for shortness, tension of the æther. (2) The restoration of equilibrium: this constitutes the discharge itself.

The tension which precedes the discharge is not equally great in all cross sections of a discharge-tube, even when the tube is of equal section throughout; it may even equal zero in certain parts of the tube. The tension has either finite or maximum values at the surface of the metallic poles and at those points which appear as points of issue of the separate positive layers or of the secondary negative pencils. The resultant of the opposing force produced by the tension on each element of the kathode is directed away from it; at the other points of issue also, it is directed at each point towards the side turned away from the kathode. When the restoration of equilibrium commences, a motion results in consequence of the finite or maximal tension on the surfaces, which advances to the side of each surface of issue remote from the kathode, and, originally excited in free æther, transforms itself secondarily on its way into transversal vibrations of the material atoms. The distances in which the tension before the discharge was zero, and in which the motion excited at the surfaces of issue does not propagate itself, remain dark; such places are the distances between the positive light, on the one hand, and the kathode-light or secondary negative light, on the other hand.

The greater the exhaustion becomes, the more do the distances between the surfaces of origin increase, and at the same time also the distances to which the motion excited at the surfaces of issue extend. This latter increase, however, is completed more rapidly than the increase in the distances between the surfaces of origin; hence it comes that one and the same section of the space occupied by the discharge may be affected by motions which radiate from two or more surfaces of origin. (Penetration of the kathode-light into the positive light or of the stratified pencils into each other.) Experience shows that such motions penetrating each other do not sensibly alter each other when their directions are the same; but when their original directions are inclined to each other at a considerable angle, they show marked phenomena of deviation *.

* Goldstein, *Wien. Ber.* 1876, 23 Nov.

That the so-called æther-envelopes of the gas-molecules or atoms take part in the emission of light resulting from the discharge is a matter of course; but the part which they play in the processes of charging and discharging must remain for future discussion. The forces exerted by the material particles upon which the formation of the æther-envelopes depends, tend to produce a different disposition of the æther from that which would result from the electrical forces only. Consequently the more gas-molecules are included in the space occupied by the discharge, the greater the electrical forces must be in order to bring about the disposition of the æther which must precede the discharge. Hence we understand how the gas acts as a hindrance to the discharge, and why conductivity of the space occupied by the discharge continually improves as the gas is more and more completely removed. In any case, I am unable to accept E. Wiedemann's view*, according to which the æther-envelopes are the real medium of the discharge. If, moreover, the æther-envelopes suffer deformations without the free æther taking any part in the process (and in the case of the kathode-discharge Wiedemann excludes any such participation), then, as regards the velocity of propagation of the discharge, Wiedemann must assume a pure distance-action between the æther-envelopes, since in highly rarefied gases we regard the times during which the æther-envelope and the sphere of action of a molecule are in contact with those of other molecules, or penetrate them, as small in comparison with the times during which the sphere of activity is isolated.

The assumption that the direction of the negative current from the kathode is the direction in which the electric discharge is propagated in the kathode-light, and also in the secondary negative pencils and positive stratifications, contrary to the usual view, seems to be justified by numerous experimental results. I would call attention, first of all, to the phenomena of shadows, which, formerly observed only with the kathode-light, have caused this phenomenon to be represented as a motion from the kathode, even by the defenders of the convection theory.

If a solid body be placed in a pencil of the kathode-light, or of the secondary negative light, then, as may be observed directly, that portion of the pencil falling upon the object which lies between its end turned towards the kathode and the object itself remains in every case intact, but that portion of the incident pencil lying on the further side of the space occupied by the object is wanting. The shadows previously described†,

* E. Wiedemann, Wied. *Ann.* x. p. 245, 1880; Phil. Mag. [5] x. p. 419.

† Phil. Mag. [5] x. p. 236.

formed in the phosphorescent surfaces excited by the positive light, and their position indicate similar behaviour. If the electric rays in the kathode-light proceeded from its exterior boundary towards the kathode, and in the secondary negative and positive pencils from the side of the anode towards the side of the kathode, then, on introducing an object, the pencil of rays would, on the contrary, remain intact from the exterior boundary up to the object, and the shadows would appear upon the wall upon the kathode side of the object.

Another argument for propagation in the direction of the negative current is found in the phenomenon described above, that the properties of secondary negative rays are, even for considerable distances, such as correspond to the conditions which exist at the negative boundary of the pencil of rays—that is, the one nearest to the kathode. The pencil, which with increasing evacuation radiates continually more and more from the mouth of a narrow tube opening into a wider vessel, contains rays possessing the properties of the light of narrow tubes. If the pencil had its origin in the wider vessel and propagated itself from it into the narrower tube, we should expect to find its properties more in accordance with the conditions of discharge offered by wide tubes. The pencil between a narrow cylinder and a wider one following upon this upon the side of the anode would show then the same colour and spectrum as those pencils which have their origin in the wide cylinder and compose the column of its positive stratifications.

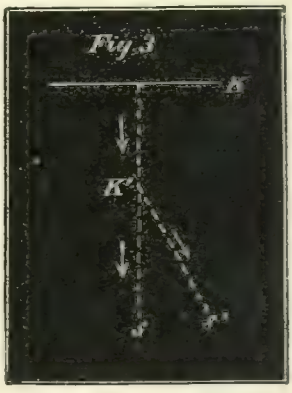
A further criterion for the direction in which the electric rays propagate themselves is found in their magnetic behaviour as described above for the kathode-rays, in accordance with Hittorf's conclusions.

It is characteristic of this behaviour that if a (sufficiently weak) magnet is allowed to act upon the end of a long kathode-pencil remote from the kathode, only this end is affected by the magnet, whilst those portions of the pencil near the kathode retain their form and position unaltered. If the magnet is brought into the neighbourhood of the kathode itself so as to act upon the portions of the rays nearest the kathode, then the whole pencil is deflected together with these portions even to its furthest point, upon which, in consequence of its great distance, the magnet could exert no action directly.

The electric particles (or the electric motion) at the end of the ray remote from the kathode therefore follows the direction impressed upon the particles at the kathode itself; but the particles at the kathode are not influenced by action upon the particles at the further end. Both phenomena agree with

the theory that the particles at the outer end of the ray were previously at the kathode, and are immediately opposed to the view that the particles nearest to the kathode have already passed through the place occupied by the remote ends; that is, the motion in the kathode-light must propagate itself from the kathode outwards.

Exactly correspondent are the phenomena of the rays of the secondary negative pencils, and also of the rays of the separate positive stratifications, when these are sufficiently expanded by high exhaustion. Hence the discharge propagates itself also in each separate stratification from the bounding surface on the kathode side to the boundary on the side of the anode. The often-mentioned phenomena of deflection are to be interpreted in a similar manner. If K (fig. 3) be the projection of a plane kathode, K′ that of a thin wire, *s* the natural direction of an electric ray issuing from K, then the ray through K′ takes the form *s* K′ *s*′; at K′ it bends round through a considerable angle, and beyond K′ follows again a straight course, which, however, deviates considerably from the direction *s* K′; *i. e.* the portion of the ray beyond K′ obeys the deviation which was exerted upon the electrical particles at K′. Hence the forces which produce motion at any point of the ray influence also all portions of the ray beyond this point, but are without influence upon the portions between the first point and the kathode. This is very simply explained by the hypothesis that the electrical motion in the ray propagates itself from the kathode outwards (in the direction of the arrow).



III.

The velocity and direction of the discharge of a pencil of electrical rays is to be distinguished, *à priori*, from the velocity and direction with which the tension that precedes the discharge propagates itself. We are not here further concerned with the velocity of this tension, but only with its direction. I believe that the phenomena observed indicate very plainly a propagation of the tension also in the direction of the negative current; that is, the tensions for the separate positive stratifications are developed in the same order of time as that in which they follow each other in space from the kathode towards the anode. I draw this conclusion from the fact that the position and peculiarities of the separate completely formed stratifications, and in particular the position of

the heads of the stratifications, *i. e.* of the points from which the separate discharges formed by the stratifications issue, depend altogether upon the position and peculiarities of the kathode, and not at all upon the conditions of the anode*.

Let the discharges pass in a cylinder with terminal electrodes which can be moved along the axis of the vessel towards each other by means of an arrangement which need not at present be further described.

If the anode in such a vessel is caused to approach the fixed kathode, no displacement whatever of the stratifications in front of the anode is observed; they remain altogether immovable and unchanged, so far at least as their continued existence is consistent with the new position of the anode. Since the positive light in every case reaches only to the anode, the layers which were situated in the portion of the tube

* The experiments described in what follows are performed with stratified discharges in pure dry air or in dry highly exhausted hydrogen, produced from a sufficiently powerful induction-current by regular interruption of the primary current. Under these conditions we obtain thick stratifications which do not vibrate to and fro like the so-called "saucer" stratifications, but, with a constant pressure of gas, preserve their position unaltered. I think it necessary to mention this, because to many who are still accustomed, upon mention of stratifications, to figure to themselves only the "saucer" stratifications and their behaviour, the mention in the following of motionless stratifications with constant intervals between them may appear surprising. The thick stratifications, strange to say, are always treated, sometimes as quasi-pathological developments, as phenomena which are disturbances of the normal phenomena of stratification; sometimes as optical illusions, caused by rapid vibration to and fro of the "saucer" stratifications assumed to exist alone. According to the evidence I have given, the meaning and mutual relation of the different forms of stratification are tolerably clear; each separate layer in a cylinder is qualitatively analogous to the discharge at a kathode which occupies the whole section of the cylinder. When the exhaustion is small this kathode-light is only a thin layer, corresponding exactly to the thin "saucer" stratification. If the exhaustion proceeds, the electric rays which make up either structure lengthen and so increase its thickness; and just as the kathode-rays finally lengthen so much as to completely fill the dark space and reach to the first positive layer, so the rays of stratifications extend so much as to completely occupy the dark spaces between them. As the density of the gas decreases the thickening proceeds further, the intervals between the heads of the layers continually increasing, and the rays issuing from the heads continually occupying the increasing intervals. When the stratifications appear most plainly in a cylinder with dry air, their thickness is very nearly equal to the diameter of the cylinder, so that with wide vessels they are of considerable thickness. As little as a kathode-discharge with extended rays is a monstrosity or disturbing phenomenon, or consists entirely of a luminous layer vibrating to and fro, so little can we entertain similar views about the thick layers; and as the laws of the kathode-light may be studied most easily in the longest pencils of it which can be obtained, so also it is just the thickest layers which represent the phenomenon most completely developed, which are in the first instance best suited for the study of the stratification.

passed over by the anode in its approach to the kathode disappear one after the other, presenting the appearance of the layers being gradually absorbed by the anode. If the anode is caused to move from its original position away from the kathode, then all the layers originally present remain unaltered, whilst new layers appear in the space left by the anode, of which each immediately upon its formation remains completely indifferent to the further motion of the anode.

Let us now move the kathode, and first let the motion be an approach to the fixed anode; at once the whole of the layers present begin to move, and are displaced each in the same direction and exactly by the same amount as the kathode itself. (In the distance occupied by the discharge, shortened by the approach of the poles, there is consequently room only for fewer layers than before; each layer disappears as soon as it is pushed up against the anode by the motion of the kathode.) If the kathode is removed from any initial position whatever further away from the anode, all the layers present follow the kathode, and keep exact time with the motion of the kathode itself; new layers appear in the space between the last of the layers originally present and the anode as the kathode moves further away, and each immediately after its formation follows the motion of the kathode*.

The interval between every two layers in a cylinder is so little different in passing from one pair to another, that in a cylindrical column of stratified light at given density of gas and intensity of discharge, we may speak simply of the stratification-interval of the column.

The number of layers present is therefore equal to the quotient of the length of the column by the stratification-interval. If the distance of the electrodes varies continuously, this quotient will only be a whole number in particular cases. If the division of the space occupied by the discharge into layers advanced from the anode, then in the case when the length of the positive light is not divisible by the stratification-interval without remainder we should expect that the incomplete and abbreviated layer corresponding to this remainder would be found at the negative end of the positive light, whilst at the anode there would be nothing but complete layers.

Observation shows exactly the contrary: the positive layer nearest the negative end of the positive light, *i. e.* nearest to the kathode, preserves the same constant extension with every distance of electrodes; so also the following layers, only the one directly in contact with the anode shortens or lengthens

* Goldstein, Phil. Mag. [5] iv. p. 362.

exactly in proportion as the excess of the above-mentioned quotient above a whole number changes.

The changes of colour are also strikingly characteristic of the influence of the kathode, as explained above. The consecutive layers of a column of positive light may show very striking differences of colour, even when no differences in form and magnitude can be perceived; these differences are very marked when hydrogen is employed.

The colour of a layer when the difference of electrodes changes is always, *cæteris paribus*, dependent on its position with reference to the kathode. Suppose, for example, that we observe a cylinder the positive light of which is divisible into five layers, the one next the kathode being blue, and the following ones in order being yellow, red, greyish red, and grey. Next to the anode we have, therefore, a grey layer. If the distance of the electrodes is now diminished by the length of one layer, whether by the motion of the anode or of the kathode, it is the grey layer which disappears, and we have a greyish-red layer next the anode followed by the rose-coloured, yellow, and blue layers in order. If the distance between the electrodes be further diminished by the length of one layer, the greyish-red layer disappears, and the rose-coloured layer is in contact with the anode, followed by the yellow and blue layers in order. When the poles are caused to approach further so as to leave only two layers, the yellow layer is next the anode, and the blue layer follows it. If, therefore, we count the layers from the anode, then with every change of distance between electrodes, the first, second, and every layer change colour; but if we count from the kathode, then the colour of the nth layer is independent of the distance of the electrodes, and each layer present possesses always the same colour.

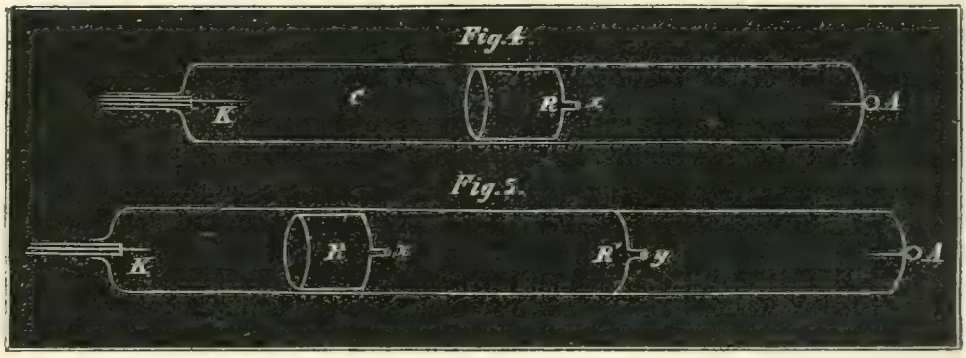
Hence the colour of each layer is regulated by the position of the kathode, and depends on its position in the series, counting from the anode.

Lastly, we may vary the size of the anode indefinitely without causing any change in the position of the layers present; but if the magnitude of the kathode be changed, the position of all the positive layers changes. The smaller the kathode becomes, under conditions otherwise similar, the larger becomes the interval between the kathode and the first positive layer, but the interval between the positive layers is not altered; so that each single layer lies further from the kathode the smaller the kathode is made.

We cannot, however, assume that the kathode, or the physical conditions which obtain at the kathode determine the conditions of tension and discharge of the whole stratified

column; but it appears that the position and properties of each separate layer depend mainly or entirely upon the position and properties of the layer preceding it on the side of the kathode. The influence of the kathode on all the members of a stratified column, which appears so markedly in the experiments just described, would then be only an indirect one, inasmuch as the properties of the kathode determine the properties of the kathode-light; this determines the position and properties of the first positive layer, this the position and properties of the second layer, and so on. This conclusion is drawn from experiments on the secondary negative light.

We saw that in a cylinder where the kathode is moved, all the layers move in the same direction as the kathode and through an equal distance. If now we introduce into the cylindrical tube C (fig. 4), with movable kathode K, a por-



tion of tube R fitting C closely and also movable in it, having a narrow opening at x, then, as already explained, x acts as a secondary negative pole for the portions of the whole discharge between x and the anode A*.

If now, whilst R retains the same position in any portion of the discharge-cylinder, the kathode K is moved, all the layers between K and x move as in the previous experiments; but the layers between x and A remain immovable, in spite of the displacement of the kathode.

If, on the other hand, K be fixed and R be displaced, so that the secondary negative pole x moves with the secondary negative light radiating from it, then all the layers between x and A are displaced exactly like the stratifications of a simple cylinder having a metallic kathode at x.

If the separate layers of the discharge show different colours, then we observe further that, when K moves, the colours of all the layers between K and x behave as previously described

* A may with advantage be made short, and placed somewhat excentrically but parallel to the axis of the tube, so that it may not be struck by the movable piece R.

for the simple cylinder, but the colours of the layers between x and A show no regular relationship to their position in order from K; their colours remain the same however their position in order varies. But if x be moved, the same law holds good for these layers as if x were a metallic kathode—the colour of each depending on its position in order from x, and the colour of every nth layer, counting from x, remaining the same for every position of x.

The dependence of the stratifications upon their secondary negative pole, and the complete analogy with the dependence upon the metallic kathode, is seen, lastly, also in the influence of the magnitude of the secondary pole. If its magnitude be diminished the layers become further apart, as if from a diminished metallic kathode; and the displacement is in both cases more marked when the surface of the pole is diminished in a greater ratio*.

If we have now a tube (fig. 5) provided with two secondary poles x and y of this kind, of which only x is movable and y is fixed (the piece R′ of which y is the mouth may be conveniently united with the wall of the large vessel by fusion when

* The diminution of a secondary negative pole may be effected in a variety of ways. Fig. 6 shows in one diagram three different simple arrangements for effecting this:—

In cylinder I. a glass diaphragm is arranged perforated with two round openings of different sizes; a glass ball is also enclosed in the tube, whose diameter exceeds that of the largest opening. By allowing the glass ball to rest upon the one or the other of the two openings, the discharge issues from a larger or smaller secondary pole. (Of course the opening acts on the side towards the kathode as a secondary positive pole.)

In cylinder II. the opening x of the communicating glass tube is the secondary negative pole; a glass rod, provided at one end with a knob to prevent its falling completely through, is movable to and fro in r. It is clear that by this means the magnitude of the opening x may be varied.

Cylinder III. shows the simplest arrangement, a glass tap the perforation of which replaces the communicating tube. The magnitude of the secondary pole is a maximum when the tap is completely open. If we gradually turn the tap from this position, and so gradually reduce the magnitude of the secondary negative pole, we see the layers gradually recede from the pole. The advantage of this arrangement is found in the power of gradually altering the magnitude of the pole; its disadvantage in the gradual alteration of the quantity of gas contained in the tube, by the evolution of gases produced by the action of the discharge on the substance with which the tap is lubricated.

the apparatus is constructed), x lies between the kathode K and y. If the kathode K is movable, we observe first that its motions only affect the stratifications between it and x, but, on the other hand, all between x and y, as well as those between y and A, are unaffected. If x is moved, we find that the motion of this pole causes a motion only of the stratifications between x and y; the stratifications between y and A remain unmoved. In the same way, when the magnitude of the negative pole varies, it is found that only changes in the pole y affect the position of the stratifications between y and A.

Hence the position of each stratification depends on the position and properties of the secondary negative pole, or pencil of secondary negative light nearest to it. But since each separate positive layer, even in a simple cylinder, is, as I have shown*, only a form of the secondary negative pencil (the section of its origin is itself a secondary negative pole), it follows that the position and properties of each separate layer, even in a simple cylinder, do not depend so much upon the kathode and kathode-light as upon the position and properties of the layer immediately near it. If, then, in a simple cylinder all the stratifications are put into motion by displacing the kathode, it follows that the motion of the kathode itself properly causes only a corresponding change in the position of the kathode-light which issues from it; the displacement of the point of origin of this last displaces the surface of origin of the first positive layer; this change displaces the surface of tension for the second layer, and so on.

I have mentioned that, in a simple cylinder, the consecutive intervals between the stratified layers of the positive light differ very little from each other; but if we observe also the very small differences which present themselves, we find that the intervals gradually decrease from the kathode towards the anode. If the space in which the discharge takes place be contracted at any point whatever so as to produce a secondary negative pole, then the intervals diminish only up to this pole; beyond it the intervals suddenly increase and a new series of decreasing intervals begins, again increasing beyond a new secondary pole, and so on. We find, then, that the magnitude of each stratification-interval is determined by the ratio of the secondary negative pencil produced by the change of section which immediately precedes the interval under consideration.

In passing to infinitely small changes in section when the secondary negative pencil passes into a positive layer, we find that the magnitude of the interval between any two layers

* Goldstein, *Berl. Monatsber.* 1876, p. 280: Phil. Mag. [5] iv. p. 361.

depends on the properties of the layer forming the component of the pair nearest to the kathode; or, shortly, each layer, or the conditions existing at its point of origin, always influence the layer following next to it, on the side of the anode, but does not influence the preceding, on the side of the kathode. The conditions under which the nth layer forms seem to stand to the properties of the $n+1$th layer in the relation of cause and effect; and hence it seems to me only a verbally different expression of the observed facts if we assume, as above, that the propagation of the electrical tensions, or the production of the separate layers, takes place in the direction of increasing values of n, *i. e.* advances from the kathode towards the anode.

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XLII. *Carbon Dioxide as a Constituent of the Atmosphere. By* Ernest H. Cook, *B.Sc. (Lond.), A.R.C.S., Lecturer upon Chemistry and Physics at the Bristol Mining School*.*

OF all the agents which have brought about geologic changes and modified the surface of the earth from time to time, the atmosphere seems to have been the least studied. Nor is this very surprising when we remember the peculiarity of its action. So general and cosmopolitan are its effects that their very abundance causes us to overlook them—and, again, so slowly acting that the changes effected require the employment of long periods of time. The two constituents of the atmosphere which have been most active in producing these changes are the oxygen and the carbon dioxide. The latter substance occurs in the air in such a relatively small amount that we are apt to underrate its influence. But when it is remembered that, were it not for the presence of this substance in the air, no coal and very little limestone could have been formed, we at once see its importance. In fact, to come somewhat nearer home, without carbon dioxide in air no vegetable growth could take place; and without plant life very little, if any, animal life would occur. Thus this substance, although in itself inimical to most forms of animal life, is absolutely necessary in the atmosphere in order that those animals may exist. In the present paper an attempt is made to consider some of the results arising from the presence of this substance.

* Read before the American Association for the Advancement of Science at Montreal, on August 25, 1882. Communicated by the Author.

Amount of Carbon Dioxide in the Atmosphere.

This question has been made the subject of experiment by many of our leading chemists. In order to calculate the absolute amount, we require to know two things—viz. the capacity or weight of the air, and the percentage of CO_2 which it contains. Fortunately the data for doing this have been determined with very great accuracy. The lengths of the diameters of the earth have been determined to be very nearly 7899 miles for the polar and 7925½ for the equatorial; "and in these measures it is pretty certain that there is not an error of a quarter of a mile" *. Applying the ordinary rule for the cubic content of an oblate spheroid, we obtain 259,026,554,299 cubic miles as the capacity of the earth. Now the height of the homogeneous atmosphere is found to be 26,214 feet†, or very nearly 5 miles; calculating the capacity of the spheroid formed by adding this distance to the lengths of the diameters given above and subtracting the capacity of the earth, we obtain the cubic content of the atmosphere supposed homogeneous: this is found to be 591,647,337 cubic miles. With regard to the amount of carbon dioxide present in the air, the older experimenters, Dumas and Boussingault (*Ann. Ch. Phys.* iii. pp. 257, 288), Löwy and Saussure (Pogg. *Ann.* xix. p. 391), have published results which yield a mean of 4 vols. in 10,000 of air, or 4½ parts in 10,000 by weight. Thorpe (Journ. Chem. Soc. vol. xx. p. 189) has shown that over the sea the average is 3 vols. in 10,000; while Saussure states that at high altitudes the proportion of dioxide is greater than at lower levels. Without deviating very far from the truth we may take 4 vols. in 10,000 of air; and we thus find (assuming capacity of air to be 592,000,000 cubic miles) 236,800 cubic miles as the amount of CO_2 in the atmosphere. Finally, calculating from the specific gravity, we find the weight to be 4287 billions of pounds. Expressed on the metric system these figures become:—

Cubic capacity of air... 2,439,987,200,000,000,000 kilolitres.
Weight of CO_2 in air... 1,913,685,908,480,000 kilogrammes.

I have given these calculations somewhat in detail because of the great difference between my numbers and those hitherto published. Thus, Dumas and Boussingault (*op. cit.*) say that the air is equal in weight to 581,000 cubes of copper each having a side of 1 kilometre: this gives 4,200,000,000,000,000,000 kilolitres as the capacity of the air, or very nearly 40 per cent.

* Herschell, 'Familiar Lectures,' p. 53.
† Maxwell, 'Theory of Heat,' p. 228.

too high. Again, Roscoe and Schorlemmer ('Chemistry,' vol. i. p. 449) state that "the amount of CO_2 in the atmosphere reaches to upwards of 3000 billions of kilogrammes," which is about 33 per cent. in excess of the truth*.

Sources whence the Air derives its Carbon Dioxide.

These are mostly natural; but the progress of civilization has added a large artificial supply to those already existing. We may state them as follows:—

(1) Combustion of carbonaceous bodies.
(2) Respiration of animals.
(3) Decomposition of vegetable and animal substances.
(4) Volcanos and other subterranean supplies.

Under the first heading is included the amount produced by the burning of coal, wood, peat, &c. From the most recently issued statistics with regard to the amount of coal raised in the world that I have been able to consult†, I find that for the last three years at least 280 millions of tons have been raised annually. This is probably a slight underestimate. Assuming that 75 per cent. of this consists of pure carbon, which if completely burnt in air would produce CO_2, and allowing a further 10 per cent. for the carbon thrown away with the ash, we leave 182 millions of tons which are annually converted into carbon dioxide. This will produce 1,800,000 tons per day, or very nearly 1800 millions of kilogrammes per day. Assuming that by the combustion of wood, peat, oil, &c. there is added one third more, we produce a total of 2400 millions of kilogrammes daily.

In the case of the respiration of animals we can only form an approximate estimate. The population of the world is at present about 1500 millions; and it has been shown by experiment that each individual produces on an average about a kilogramme of CO_2 per day of 24 hours. Thus the human race, in respiring, add to the air about 1500 millions of kilogrammes of carbon dioxide per day. Remembering the large

* The above calculations are made on the figures deduced from the results of the experimenters cited above. Recent investigations have, however, thrown some doubt on the correctness of these numbers, the general opinion being that 4 vols. in 10,000 is much too high. Thus, Fittbogen and Hasselbarth (*Chem. Centr.* 1875, p. 694) give 3·4 vols. in 10,000 as the average; Farsky (*Chem. Centr.* 1877, p. 198) found 3·4, while more recently Reiset (*Comptes Rendus*, lxxxviii. pp. 1007–1011) deduces 2·942. Taking the mean of these numbers, we have

Weight of CO_2 in air .... 1545 billions of kilogrammes nearly.

† Mineral Statistics for Great Britain for 1881; and Smyth's 'Coal and Coal-Mining,' latest edition.

amount of animal life existing on the globe, and also that many of the larger species produce a greater quantity in a given time, we may with a sufficiently near approach to accuracy say that from the lower animals the air receives twice as much daily as from man. Hence from the whole animal kingdom we derive about 4500 millions of kilogrammes.

The amount of dioxide which the atmosphere receives from decaying animal and vegetable substances is impossible to estimate. Most of it is produced in regions far away from the abode of man. That a considerable quantity is produced from this source, however, is evident when we consider the vast quantity of vegetable matter which year after year falls to the ground and undergoes decomposition. In fact, if the estimate of the amount of action exerted by plants given later on in this paper is a correct one, we must conclude that a much greater amount of dioxide is produced by this process than has been hitherto supposed. Although it is evidently impossible to give figures, yet, in order to arrive at a numerical estimate, we may assume that the same quantity is yielded as by man, viz. 1500 millions of kilogrammes daily.

The last source whence the air receives its supply of carbon is from volcanos and the fumaroles and rents in the ground in volcanic districts. The amount thus supplied is enormous, both active and extinct volcanos joining in increasing the quantity. Considering the area occupied by the volcanic districts, and the immense quantities of gas which are given off from the craters and fumaroles, we must readily come to the conclusion that from this source by far the greater part of the atmospheric carbon dioxide is derived. In fact Poggendorff has calculated that at least ten times as much is derived from this source as from all others put together. The numbers given above for the amount yielded by other sources are probably greater than similar numbers deduced by Poggendorff, since the amount of coal used and the population have both increased since his time. Instead, therefore, of taking ten times, if we take five we shall perhaps approach very near to the absolute amount given by Poggendorff. This will give us about 40,000 million kilogrammes daily given to the atmosphere from subterranean sources*.

Taking the whole of these results together, we have that from all sources there is daily added to the atmosphere the

* Supposing this CO_2 produced according to the equation

$$CaCO_3 = CaO + CO_2,$$

we shall have daily decomposed about 90,000 million kilogrammes of limestone.

enormous amount of at least 50,000 millions of kilogrammes of carbon dioxide. Dividing the absolute amount given above by this number, we find *that the amount of carbon dioxide in the atmosphere would be double what it is at present in about one hundred years if there were no means of compensation.* In arriving at this estimate no account has been taken of the amount of oxygen used up in producing the dioxide. This obviously affects the first three sources only; but by taking it into account we should reduce the time somewhat; but practically this correction is so slight that it can be neglected. Poggendorff made a similar calculation, and gave 386 years as the period which it would take to double the amount of the dioxide, supposing there were no compensating influences at work. The discrepancy in the two numbers is explained, first, by the absolute amount of CO_2 in the air being much less according to my calculations than that previously supposed, and also by the circumstance that Poggendorff's estimate of the amount yielded by the combustion of carbonaceous substances was much less, owing to the defective data at his command.

Compensating Influences.

Having now arrived at an estimate of the amount of carbon dioxide daily added to the atmosphere, let us examine the causes which bring about its decomposition and removal from the air. The known causes which are at work producing this change may be considered under three heads, viz.:—

(1) Fixation of carbon by growing plants.
(2) Removal of dioxide by zoophytes.
(3) Absorption of dioxide by inorganic chemical actions.

The first cause here mentioned is one which is essential to almost all forms of vegetable growth. In estimating its magnitude we are met by the want of reliable experimental data, making it almost impossible to arrive at any definite conclusion. It is, however, the only one which restores the oxygen to the atmosphere, in the other two actions the dioxide being absorbed bodily without being decomposed. Also most, if not all, the decomposition effected by plants will occur during the spring and summer, the most active period of plant-growth. The second and third causes act continuously. Certain experiments have shown that a square metre of leaf will decompose in sunlight about a litre of CO_2. Also Mr. Trelawny Saunders, some years ago, calculated for Sir Charles Lyell the area of the land-surface of the globe. The figures he gives are*:—

Total area of land	57,600,000	square miles.
Area of Arctic and Antarctic land .	8,200,000	„ „

* Austed's 'Physical Geography,' p. xxxviii.

Thus the land-surface bearing vegetation capable of decomposing carbon dioxide amounts to 49,400,000 square miles. A large portion of this land, however, is uncovered by vegetation: cities are built on it; barren mountains rise out of it; and large rivers run through it. Estimating the absolute area of leaf (*i. e.* chlorophyll-bearing organs) borne by the plant-bearing land of the earth as 50 per cent. of the total area, we find that 24,700,000 square miles of leaf are engaged in purifying the atmosphere. This is equal to about 63,973,000,000,000 square metres, which gives the number of litres of CO_2 decomposed per hour. But sunlight only lasts, on an average, about ten hours a day; consequently the total amount daily decomposed is equal to ten times this amount. Finally, allowing 25 per cent. for the diminution of the action which takes place in winter, we find that the enormous amount of 479,000 millions of kilolitres, or over 900,000 millions of kilogrammes of carbon dioxide are decomposed daily. This amount is much greater than that produced from all sources taken together. But it must be remembered that a large portion of the carbon thus withdrawn by plants during the spring and summer months is returned to the air again by the decomposition of the leaf in autumn. Although we have allowed for this above, yet if plant-action is anything like so powerful as these calculations show, that allowance will have to be considerably increased. Again, a reduction, and perhaps a considerable one, will have to be made on account of the respiration which has been proved to take place in some plants during the hours of darkness; but I am unable to find an account of any experiments upon this point. The magnitude of this action given by these calculations is astonishing. This paper was commenced under the idea that the action usually attributed to plants was greatly overestimated, and that their purifying effect was exaggerated. It will be seen that the vegetable life on the globe is sufficient of itself to keep up the purity of the air. The author wishes this statement to be received with caution, because of the unsatisfactory nature of the fundamental experiment upon which the calculations are based, and also of our total want of knowledge of the amount of plant-respiration. This latter action may be much greater than is usually supposed.

The second great action going on in nature is effected by the interposition of animal life. It consists in the removal from sea-water of the carbon dioxide held by it in solution by certain low forms of animal life. The most important of these are Actinozoa and Foraminifera—the former being concerned in the building of coral reefs, and the latter in forming those

immense masses of rock-material of which the chalk and nummulitic limestone may be taken as examples. Certain other forms of animal life, such as Brachiopoda &c., also add their influence to that of these lower forms; but their effects, however, are comparatively insignificant. The immense influence exerted by these minute creatures is evident when we remember the vast masses of limestone entirely of organic origin occurring in geological formations of all ages. Millions of tons of limestone formed in this way occur in the solid crust of the earth; and every ton of limestone contains about nine hundredweight of dioxide. Nor is this action confined to the past. It is as active now, in all probability, as when engaged in building up those immense deposits of white chalk so abundant in some parts of Europe. Recent deep-sea soundings have revealed the fact that Foraminiferal life still flourishes in the depths of the ocean, while the coral-polypes are still building reefs in the warmer seas. On the other hand, we must not forget that Darwin has shown that these coral-polypes can only exist in water of a certain temperature, which is only attained in the warmer seas, and at a certain depth below the surface of this water. Their influence, therefore, is limited and confined to a comparatively small area of the globe. Another circumstance which seems to have been overlooked by most writers upon the subject is, that this dioxide fixed in the solid state in this way is contained in the water, and not in the atmosphere. It is generally supposed that all of it has been derived from the air; but a very large portion *must have been obtained from submarine volcanic eruptions*, and never formed part of the atmosphere at all. Taking all things into consideration, this cause, although very powerful, seems rather to be one whose influence is only felt after the lapse of many years, and, for activity, cannot be equal to the first one.

The third action going on in nature effecting the purification of the air is a strictly inorganic one. Included under this head are such processes as the conversion of felspar into kaolin, the decomposition of such silicates as hornblende, pyroxene, &c. The large deposits of kaolin and decomposed felspar which are met with in the earth sufficiently prove the magnitude of this action. Calculations were made many years ago by Ebelmen (see the *Recueil des Trav. Scient. de M. Ebelmen*, Paris, 1855), and have recently been recalculated and very clearly stated in an excellent paper by Dr. T. Sterry Hunt, F.R.S.* A glance at the numbers given in these memoirs will show the vast and important effect which these processes must have exerted.

* "Chemical and Geological Relations of the Atmosphere," American Journal of Science, May 1880.

Thus, Dr. Hunt says "that a weight of carbonic dioxide equal to more than twenty-one times that of our present atmosphere would be absorbed in the production from orthoclase of a layer of kaolin extending over the earth's surface with a thickness of 500 metres, an amount which evidently represents but a small proportion of the results of felspathic decay in the sedimentary strata of the globe." Evidently, then, here we have a cause which has removed, and is removing, a vast amount of carbon dioxide from the atmosphere. Any estimate of the rate of its action is obviously impossible. It must not be forgotten, however, that subaerial felspathic decay is a very slow process, and that therefore the large deposits of decomposed felspar found in the earth seem to point rather to a comparatively slow process acting through an immense number of years than to a rapid process such as that effected by plants.

General Conclusions.

It is of course evident that, if the compensating influences are just equal in amount and in rate of action to the producing ones, the amount of carbon dioxide in the air will remain constant. Unfortunately an insufficiency of reliable data prevents a definite answer being given to such a question. The foregoing considerations, however, seem to show that in all probability the causes at work removing atmospheric dioxide are more powerful than those producing it. As a consequence, the atmosphere is being robbed of this constituent, the greater part of which is becoming fixed in the solid earth as carbonate of lime. But this process has already gone on for so long a time, that there is already fixed in this way an immense quantity of CO_2 equal to many hundreds of times the amount contained in the existing atmosphere. The question of the source of this large amount naturally arises; but the answer to be given must simply be an admission of our want of knowledge. The idea that it all at one time formed part of the atmosphere of the globe has been suggested by Brongniart; and Dr. Sterry Hunt considers (*loc. cit.*) that a universal atmosphere of the same quality as that of the earth exists, from which the carbon dioxide now fixed in the earth's crust has been derived.

There can be no doubt that, unless we accept the latter of these theories, there must at some antecedent period have been an atmosphere covering the globe much richer in this gas than the present one; but whether such an atmosphere would account for the luxuriant vegetation of the Coal Period is at present an open question. If Dr. Hunt's hypothesis be a correct one, it is interesting to remember that the carbon which we contain in our bodies may have existed at one time as a

portion of the body of an inhabitant of the most distant member of the universe. But whichever way we consider the subject in the light of the facts which we have stated, it is full of unusual difficulty, and is singularly devoid of accurate experimental data.

XLIII. *On the Dimensions of the Magnetic Pole in Electrostatic Measure.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE had the honour of reading a letter upon the dimensions of the magnetic pole in electrostatic measure which Dr. Lodge addresses to you this month. His suggestion seems to me to reconcile the views of Prof. Clausius and Mr. J. J. Thomson on this subject. A model of the magnetic system must be made in a substance of the same magnetic permeability as the medium that is to surround the current-system, and must be substituted in the place of the magnets, before any comparison can be effected. The two systems, current- and magnetic, will then be always equivalent if once equivalent.

Dr. Lodge treats this as a suggestion; but I think it is almost susceptible of demonstration. According to Weber's law, a current flowing in a closed circuit can be replaced by a simple magnetic shell of which the edge coincides with the circuit. The shell may be as thin as we please; but its *strength* must have a definite value. This law we only know to be true for air. Consider any equipotential surface of the positive magnetism on one face of the shell, at a distance from it infinitely smaller than the thickness of the shell. It passes through the substance of the shell, issuing at the edges, and covers the positive face. Similarly such an equipotential surface of the negative magnetism on the other face passes through the substance without cutting the former surface, and covers the negative face.

If on each of these surfaces we spread a surface-magnetism of which the density is the quotient of the magnetic force by 4π, *then for all points outside the pair of surfaces and the shell we may replace the latter, and therefore its equivalent current-system, by the magnetic coats upon the equipotential surfaces.*

Now consider any diaphragm, S, completely enclosing these surfaces and the shell. If instead of air we substitute a medium of magnetic permeability μ throughout space *outside* S, a surface-density σ is developed upon S. But the equipotential surfaces and the coats of magnetism thereon are not affected by this development; for they relate only to the en-

closed system. Hence the magnetic and current-systems, if equivalent in air, are equivalent in a medium which does not penetrate through any portion of the space occupied by the magnetic substance.

It is particularly to be noticed that, while in air any two of the infinite number of magnetic shells equivalent to two closed currents exercise the same attraction upon one another, this is no longer true when they are immersed in the medium μ. Then each pair exercises the same attraction as the two currents when the corresponding diaphragms S are drawn in the shape of the magnetic shells.

An experiment made by me in the Cavendish Laboratory last June confirms, as far as it goes, Dr. Lodge's view; but, fearing to make my letter too long for insertion, I must postpone any account of it for the present.

I am, Gentlemen,
Your obedient servant,

October 20, 1882. E. B. Sargant.

XLIV. *Notices respecting New Books.*

The Concepts and Theories of Modern Physics. *By* J. B. Stallo. London: Kegan Paul and Trench. 1882.

TO write an adequate criticism of this book would involve writing a book of equal size. In the present review it is intended to make clear the standpoint from which the author speaks, and the general conclusions reached, rather than to enter into a detailed criticism of these conclusions. The failure to appreciate the position from which the author writes has already led to some misunderstanding and not a little confusion. It is stated in the preface that the work is intended "as a contribution not to physics, nor certainly to metaphysics, but to the theory of cognition." There is probably no word which is more quoted and less understood than the word "metaphysics." It is used by hardly any two writers in the same sense; and it is not too much to say that in many cases it is merely used as a term of abuse without any clear conception of its meaning. As in the present work this word is constantly used, and, moreover, as the author's main purpose is to show that certain scientific theories are in reality metaphysical, it will be important to understand precisely the signification which he puts upon the word. It is abundantly evident from the whole book that the subject which Mr. Stallo condemns, and rightly condemns, under the name of metaphysics is that which is far better designated by ontology; and in fact he himself frequently uses these terms as synonymous. The assumption of fictitious entities as causes, the belief that "the true nature of things can be discovered only by divesting them of their relations—that to be truly known they must be known as they are in themselves in their absolute essence," and in fact the whole procedure of ontology, is what is meant by

"metaphysics." Against metaphysics in this sense of the term the author, in common with many other scientific writers, wages unceasing war. Admitting the utter futility of the ontological method, it is very questionable whether this has not been overdone, whether this reiteration of abuse against ontology is not mere slaying of the slain. It is far otherwise if, after a man has clearly understood what ontology means and has been convinced of the falsity of its method, he sets himself to see whether these same errors exist in subjects non-metaphysical, and even in the reasoning of those who were loudest with their revilings. This is action, not profession, and indeed noble and useful work in the field of criticism. As Prof. Huxley most truly has said, "It is the business of criticism, not only to keep watch over the vagaries of philosophy, but to do the duty of police in the whole world of thought. Wherever it espies sophistry or superstition they are to be bidden to stand, nay, they are to be followed to their very dens and there apprehended and exterminated, as Othello smothered Desdemona, 'else she'll betray more men.'" This in truth is the task which Mr. Stallo has undertaken in the present volume, having chosen the field of science for his beat.

Besides having critical value, it is stated that the work is "intended as a contribution to the theory of cognition." Now this term, which is a translation of the German "Erkenntniss-Theorie," is but seldom used in this country in precisely the same sense which is here, in common with modern German usage, adopted. In the present work it is used in a sense very similar, if not identical, with what I have called the "New Metaphysic" (Phil. Mag. xiv. p. 75)—that is to say, in regard to method. The author, as we have seen, entirely rejects the "old metaphysic," or ontological method of inquiry—"all cognition being founded upon a recognition of relations;" and his theory of cognition seeks to discover these relations. The "thing *per se*," the "Ding an sich" or thing-in-itself of Kant, and "the absolute," as well as the assumption of other fictitious entities as the "fountain and origin of all phenomenal existence," is distinctly repudiated; and hence these conceptions form no part of his theory of cognition. From this standpoint the author proceeds to examine the validity of the reasoning upon which the mechanical theory and other scientific theories rest. It may fairly be asked, what are the qualifications of the author for this by no means easy undertaking? The title-page of the book affords no information on this point. It will therefore be probably unknown to most readers of the book in this country that the author is an American judge of no small reputation, who (I believe myself to be correct in saying) was formerly a professor of physical science.

The book itself bears witness to the author's wide acquaintance with philosophical and scientific writings. The volume may be divided into two parts. The first, consisting of eight chapters, is devoted to a rigid examination of the atomic theory, the kinetic theory of gases, and the doctrine of the conservation of energy. While admitting the value of the atomic theory as a "working hypothesis," the

author attempts to show that, as a scientific explanation of the constitution of matter, it is of little or no value. The kinetic theory of gases is condemned without reservation, as not even satisfying the conditions of a scientific theory and as based upon ontological assumptions. The doctrine of the conservation of energy is considered to be sound; and the chapter which is devoted to its examination shows an intimate acquaintance at least with the history of the subject. A detailed criticism of the validity of the author's conclusions (for there is much to be said against them) would be impossible within the limits of a review; and therefore this will not be attempted here; but one or two positive errors and misconceptions will be noticed. In the first place, it is incorrect to say (p. 23) "that with few exceptions scientific men of the present day deem the validity of the mechanical explanation of the phenomena of nature to be, not only unquestionable, but absolute, exclusive, and final. They believe that this validity is not conditioned, either by the present state of human intelligence, or by the nature and extent of the phenomena which present themselves as objects of investigation." The reverse of this is nearer to the truth. A man of science, in the capacity of a scientific investigator, is logically compelled to consider no explanation or theory as final, but to be prepared at any moment to abandon an explanation which should prove to be insufficient or at variance with facts, in favour of another which more adequately accounts for them. This is what all scientific men hold and are bound to hold, as Liebig expressed it in a passage actually quoted by the author in another part of the book, "The secret of all those who make discoveries is that they regard nothing as impossible." Throughout the book the author seems to lose sight of the necessary tentativeness of all scientific theories. Under the head of the Atomic Theory the author discusses the doctrine of the indestructibility of matter. In Chapter II. he states that the true correlate of motion is not matter, but mass; and hence this term is used where ordinarily the word matter is employed; but when discussing the indestructibility of matter in Chapter VII. we find that he uses this term, and not indestructibility of mass, which is really what chemists mean. The current doctrine is first stated, "That the constancy of mass is attested by the balance, which shows that neither fusion nor sublimation, neither generation nor corruption, can add to or detract from the weight of a body subjected to experiment. When a pound of carbon is burned, the balance demonstrates the continuing existence of this pound in the carbonic acid, which is the product of combustion, and from which the original weight of carbon may be recovered. To test the correctness of this interpretation we may be permitted slightly to vary the method of verifying it. Instead of burning the carbon, let us simply carry it to the summit of a mountain or remove it to a lower latitude: is its weight still the same? Relatively it is: it will still balance the original counterpoise. But the absolute weight is no longer the same. This appears at once if we give to the balance another form, taking a pendulum instead of a pair of scales." What the author means by talking about "absolute

weight," when he so rigidly insists "that there is nothing absolute or unconditioned in the world of reality," is by no means clear, especially as we find from the context that what he calls "absolute weight" is evidently and, indeed, necessarily based upon a relation. In any sense the expression "absolute weight" is a contradiction in terms. From this the author argues that "the ordinary statement of the fact is crude and inadequate;" and adds that it is "further necessary to remember that this weight may be infinitely reduced, without any diminution in the mass of the body weighed, by a mere change of its position in reference to the body between which and the body weighed the relation subsists." It is this very constancy of mass or quantity of matter amid all changes, of necessity relatively determined, which chemists mean to indicate by the expression "indestructibility of matter;" and the author's criticism merely amounts to a quibble about words; he has mistaken the letter for the spirit. Of the second half of the volume, Chapters IX. to XII. are devoted to the theory of Cognition, and contain an analysis of scientific ultimates, Matter, Force, Time, and Space—that is, a consideration of how we really know these, to what realities these words correspond. This being not strictly a scientific inquiry, although of much interest and importance, the results will not be considered here. In Chapters XIII. and XIV. transcendental geometry receives most severe and lengthy treatment. Lobatschewsky's non-Euclidean geometry and Riemann's doctrine of the manifoldness of space, which have occupied the attention of the most eminent mathematicians during recent years, are considered to be absurd. The same ontological error "which has given rise to the atomo-mechanical theory in physics, has led to the doctrine of pangeometry in mathematics." Even admitting the author's criticism as to the nature of space, it by no means follows that transcendental geometry is not a legitimate department of mathematics. The Nebular Hypothesis is considered in Chap. XV. as the cosmogony of the atomo-mechanical theory. The author rejects the hypothesis, first, because "all speculations respecting the universe as an unlimited whole" are fundamentally inadmissible; and, secondly, the hypothesis has proved to be at variance with a number of important astronomical facts. The last chapter of the book consists of a summary and forecast, the author concluding that "the atomo-mechanical theory cannot be the true basis of modern physics," and looking to the Conservation of Energy as a basis for the future.

On the whole the book deserves careful consideration from all physicists; for although the author has more than once mistaken the letter for the spirit, which gives some of the criticism a ludicrous aspect, and, further, has in many cases attributed to Science speculative doctrines and opinions held by individual scientific men, yet there is much acute and careful critical work in the volume. Certainly Science ought to be the first to welcome and the last to reject candid criticism of her methods and theories; for, perhaps even more than other pursuits, Science maintains *Magna est veritas et prævalebit.*

WYNDHAM R. DUNSTAN.

XLV. *Intelligence and Miscellaneous Articles.*

ON DR. C. W. SIEMENS'S NEW THEORY OF THE SUN.

BY M. FAYE.

IT would appear that this theory has greatly struck our physicists; for it had scarcely appeared in London when it was translated and published in France in various forms, and especially in the last number of the *Annales de Chimie et de Physique*. I suppose that the principal object of this haste was the announcement of fresh experiments which have been instituted by the author upon the chemical action of light. It is well known that, under the action of light *and with the intervention of the chlorophyll of plants*, aqueous vapour and carbonic acid are decomposed at ordinary temperatures, and brought back to the combustible form, carbon and hydrogen variously associated. Dr. Siemens has tried whether the action of the light of the Sun alone would not produce this decomposition if we submit to it, without any other intermediary, aqueous vapour and carbonic-acid gas excessively rarefied, brought for example to the vacuum of $\frac{1}{1800}$. His experiments, which, in my opinion, only require a counter-test which it would be easy to institute, have given perfectly affirmative results. Thus, the burnt gases having been brought to such a rarefaction that they no longer permitted the passage of the induction-spark, a few hours' exposure to the light of the Sun sufficed to enable the mixture to allow this spark to pass with the well-known coloration that it acquires in hydrocarburetted media *.

Regarding these beautiful experiments as decisive, Dr. Siemens has been led to inquire whether this phenomenon does not perform in the universe a part still more considerable than in vegetable life. Supposing Space to be filled with analogous gases, already burnt, the light of the Sun would revivify the combustibles hydrogen and carbon, which would then be quite ready to furnish the food of a fresh combustion.

By drawing them to himself and burning them afresh, the Sun would recuperate a good portion of the enormous heat which one is grieved to see him radiating in pure loss into celestial space.

Dr. Siemens has thus been led to put forward the following hypothesis:—Space is filled with burnt gases, aqueous vapour and carbonic acid, mixed with inert gases, nitrogen &c., pretty nearly the same as those of our atmosphere, at a pressure of $\frac{1}{2000}$. These gases are partially converted into combustibles under the action of the solar light; then, by a mechanism like that of the fan of a blower, the Sun draws them to himself, burns them, and sends them back again into space. This immense source of heat would be continually resuscitated; the only part of its radiation lost would be that which is not absorbed by the cosmical medium of a density of $\frac{1}{2000}$.

* A vacuum produced in a bell-glass into which a drop of oil of turpentine has previously been introduced, for example.

It is perfectly true that, for the physicist, air at $\frac{1}{200}$ would be an almost absolute vacuum, so much so indeed that in such a vacuum the electric spark would no longer pass. But to the astronomer such a medium would be very dense. When we speak, in Astronomy, of the resistance of a medium or of the æther, and when by the aid of the most delicate observations and the most profound calculations, we seek for traces of this resistance, we have to do with a very different thing.

Without entering upon these discussions, I will remark that the trajectory of a cannon-ball with a velocity of 500 m. is sufficiently altered at the end of a few seconds to compel artillerists to take into account the resistance of the air in their tables.

If the air is reduced to $\frac{1}{2000}$, but the velocity of the projectile becomes that of the celestial movements, 60 times as much for example, these palpable effects will become, for a multitude of celestial projectiles of dimensions comparable to those of our cannon-balls, twice as great as in our firing-grounds, and this not merely at the end of a few years or a few centuries, but at the end of a few seconds.

In the second place, it seems to me that the celebrated English physicist has somewhat neglected to examine into the quantity of matter which he adds to the solar system. Under the influence of attraction this matter would go to unite itself with the preexisting stars, with the sun especially, and would continually augment their mass. Nothing is easier than to form an idea of this. A litre of air containing the required proportion of aqueous vapour weighs at least 1 gr. at the ordinary pressure. At a pressure of $\frac{1}{2000}$ this will be 0·0005 gr., and a cubic metre will weigh 0·0005 kilog. This being settled, if we restrict the solar system to a sphere including all the planets as far as Neptune, the weight of the excessively rarefied matter added by the hypothesis would be, in kilogrammes,

$$\tfrac{4}{3}\pi\,(6400000 \times 24000 \times 30)^3 \times 0{\cdot}0005 \text{ kilog.}^*$$

The actual weight of the Sun is, in kilogrammes,

$$\tfrac{4}{3}\pi\,(64000000)^3 \times 5{\cdot}6 \times 324000\,\dagger.$$

The first is 100,000 times as great as the second. It is therefore 100,000 times the mass of the Sun that this hypothesis adds to those of which celestial mechanics has hitherto kept so minute an account.

It is not very probable that the astronomers will adopt such hypotheses. No doubt they would be pleased to think that Nature has provided the Sun with resources to make his heat last longer; but as his final refrigeration is still, under any circumstances, a

* The first number is the radius of the earth in metres; the second the distance of our globe from the Sun in terrestrial radii; the third the distance of Neptune in parts of the distance of the Sun.

† The first number is the radius of the earth in decimetres; the second the mean density of our globe referred to that of water: the third the mass of the Sun referred to that of the earth.

tolerably distant catastrophe, they will console themselves by the thought that the things of this world, even the most beautiful, do not appear to be made to last for ever.

As to the fundamental experiments of Dr. Siemens, they will lose none of their importance in their eyes. The business is to surprise a secret of living nature, one of the laws of the organic world; and their desire will be that Dr. Siemens may pursue the course in which he has commenced so brilliantly, even though they cannot hope to have a very bright light thrown by it upon their own researches.—*Comptes Rendus*, October 9, 1882, p. 612.

ON THE CONNEXION BETWEEN THE GAS-DENSITY AND STRATUM-INTERVAL IN GEISSLER TUBES. BY DR. E. GOLDSTEIN.

Let the total length of a series of immediately consecutive strata of the positive light, of which the first is that which is next to the positive end of the tube, divided by the number of strata, be called the mean interval. The following data respecting this quantity are abstracted from experiments with dry air, hydrogen, and mixtures of the two, under such conditions of the discharge that the strata do not exhibit the to-and-fro vibrating saucer-shapes which escape any precise measurements, but appear in the so-called nebulous forms, which can be brought to a considerably greater degree of stability. In opposition to the generally current view that these clouds represent degenerations and derangements of the proper stratification, I have already* called attention to the far greater probability that they only represent the full development of that phenomenon. The thick cloudy strata stand in precisely the same relation to the thin saucer-like strata as a long-rayed tuft light at the cathode to thin films at first investing the cathode, from which, with diminishing density of the gas, the elongated rays are developed. In order to form a clear conception, I wished, further, to be able to presuppose as known that in cylindrical tubes the stratum-interval increases with increasing width of the tube†, so that, in tubes filled with air, the intervals between the individual strata, when the latter are most distinctly formed, are about equal to the diameter of the tube‡.

If, now, we determine, for cylindrical tubes of different widths inserted in the current-circuit, from an equal number of strata the mean stratum-interval J, J′, J″, . . . for any two pressures of gas d and δ, we obtain constantly

$$\frac{J_d}{J_\delta} = \frac{J'_d}{J'_\delta} = \frac{J''_d}{J''_\delta} \cdots,$$

in words:—For cylindrical tubes of different widths, the mean stratum-interval constantly varies in the same ratio between the same gas-pressures. Therefore, if, for example, in one of the tubes

* Wied. *Ann.* xii. p. 272.

† *Monatsb. der Akad. Berlin*, 1876, p. 294; Phil. Mag. [5] iv. p. 353.

‡ Wied. *Ann.* xii. p. 272.

the stratum-interval has by the rarefaction of the gas been raised to twice or three times its first-measured value, then in all the other tubes the intervals have been doubled or trebled.

The tube-diameters in my experiments varied between 2 millim. and 4 centim. The above-mentioned regularity came out, independently of whether the different cylinders formed separate vessels with two metallic electrodes each, or whether, united into a single tube, they were inserted in a line one behind another in the current.

The law is moreover found to hold good equally whether the mean interval be taken from a large or a small number of strata, provided the numbers in the different tubes be equal, although at the same time the absolute value changes. From this it can be concluded that *each single* interval also increases in accordance with the above-mentioned law.

The value of the mean interval is found with great regularity to be, in every tube, as much smaller as the number of strata from which it is derived is greater. Yet the amplitude of the undulation, multiplied by the greatest number of strata employed, never reaches the value of the smallest of the single intervals. Consequently, from the above law it follows that, if in any tube the magnitude of the mean stratum-interval for a series of gas-densities $D_1, D_2 \ldots D_n$ is known, and also, for a number of other tubes, each value of the mean interval that corresponds to any one of those densities, the number of the strata which these tubes can show at all the densities from D_1 to D_n can be calculated.

The proportion $\frac{Jd}{J\delta} = \frac{J'd}{J'\delta}$ permits us to conclude that the function according to which the mean interval varies with the gas-density is the same for tubes of different widths. Experiments for the purpose of ascertaining that function gave the following result:—

If the rarefactions of the gas increase in a geometrical series, the stratum-intervals are augmented also, very nearly, in a geometrical series. But the exponents of the two series are not identical; that is, the stratum-intervals are not (as was once maintained by the other side) inversely proportional to the pressure of the gas. On the contrary the measurements prove that the intervals increase much more slowly than the rarefactions—approximately at the rate of $\frac{4}{3}$ when the rarefaction is 3. I will defer more definite statements until I have determined the exponents with the greatest possible exactness, for which I am at present experimenting on a Toepler pump in the form described by von Hagen*.—*Monatsberichte der Kön. Akad. der Wissenschaften zu Berlin*, 1881, pp. 876–878 (separate impression, communicatd by the Author).

ON THE ELASTICITY OF RAREFIED GASES. BY E. H. AMAGAT.

This subject has already been treated by Mendeleef, Kirpitchoff and Hemilian, by Silgerström, and by myself. Those researches

* Wied. *Ann.* xii. p. 425.

having led to different results, I thought it necessary to resume my experiments, considerably improving my apparatus, especially in what concerns the measurement of the pressures, which is the only difficulty peculiar to this investigation. The method employed having been already described in the *Annales de Chimie et de Physique*, t. viii. (1876), I shall only dwell on the modification which the differential barometer has undergone; it is the essentially delicate part of the apparatus, all the other parts of which have also been considerably improved. This barometer consists of a single glass tube bifurcating, at about 70 centim. above the level of the mercury in the cistern, into two wider cylindrical branches, one of which forms the barometric chamber, and the other is put into communication with the space filled with the gas the pressure of which is to be measured. The immediate result of this disposition is that there is no need to attend to the difference of temperature between the two mercurial columns, which are here joined into one at a little distance below the meniscuses. The branches of the bifurcation are prolonged upwards by stems of very small diameter, having each a glass cock, and joining again to form a single stem. The rest of the apparatus is disposed so that the manometer can be charged in place by the process generally adopted nowadays, which consists in first exhausting it with a Sprengel pump; this was worked, moreover, during all the time of the filling, so as to maintain the vacuum continually dry by the intervention of a tube containing phosphoric acid. All suction of air through the slender point was avoided by covering the surface of the mercury with a layer of sulphuric acid, which remained in the cistern during all the experiments. Above the lower single branch was a glass cock, by closing which the differential barometer could be transformed into an ordinary truncated barometer, and thus the errors due to variations of the atmospheric pressure be eliminated—which is extremely important.

In order to avoid as far as possible the errors due to refraction and capillarity, the two branches of the manometer, before being soldered to the single stem, were rounded and polished inside with the same copper mandrel, so as to be rendered perfectly cylindrical; a plane facet was then cut on the exterior, quite parallel to the generating lines of the interior cylinder. This done, the pieces were soldered, the necessary precautions being taken to keep the plane facets rigorously in the same plane.

These pieces are very difficult to obtain: a large number of them break or split, either during the rounding, or the soldering, or even after these operations are finished. The cylinders were smoothed and cut by M. Lutz; the manometers were afterwards finished by M. Alvergniat; that is to say, they were made with all the skill and perfection that could be wished for.

To eliminate capillarity-errors, an internal diameter of 2 centim. was given to the cylinders: and they were evidently perfectly equal: it was easy to verify that the mercury in them was in perfect equilibrium under the thread of the cathetometer.

I shall not dwell upon the precautions which I took with respect to the illumination of the meniscus (by means of a pencil of electric light sifted by passing through a column of water coloured with a little bichromate of potass) in order to be certain of viewing the upper part of it. Here a cause of error exists which is much more frequent than is generally thought, especially when the black silhouette of the meniscus is projected upon a bright ground.

The measurements were performed with a small cathetometer of a peculiar construction, on which $\frac{1}{500}$ millim. could be read off, and which I had made by M. Benevolo, in my private atelier, specially for these researches.

In my first investigation I descended only to 6·5 millim. pressure; this time I have often operated with pressures below 1 millim. I always arrived at the result that the deviation is of the order of magnitude of unavoidable errors. Indeed, for initial pressures of 12 millim. (in round numbers), two series composed of numerous fairly concordant results gave for the value of $\frac{pv}{p'v'}$ (v being sensibly $= 2v'$) the numbers 0·9986 and 1·0020 relative to air; for the initial pressures comprised between 3 and 4 millim. the results varied between 0·9999 and 1·0040; and for pressures near 1 millim. the extremes are 0·999 and 1·015: this divergence corresponds to an error of 15 millim. in the measurement of the pressure. All these numbers are means.

In his experiments M. Mendeleef obtained a series of products pv. This appears a more favourable condition for showing how those products vary. According to him, they go on decreasing with the pressure, starting from a certain pressure which would be 6 decim. for air. In order to prevent any delusion in this respect, it is well to observe that every sensibly constant cause of error in the estimation of the pressures, taking effect upon smaller and smaller pressures, and consequently giving a relatively greater and greater error, will produce the illusion of a regular augmentation or diminution of the products pv. This is what must result, for example, from the want of absolute vacuum in the barometric chamber, even if it be only on account of the effect produced by the mercury vapour.

In short, a minute examination of the possible errors has shown me that, even if one could attain in the readings the precision spoken of by M. Mendeleef (thousandths of a degree, and thousandths of a millimetre), that pressure would be illusory in the presence of errors proceeding from manifold causes—such as the refraction- and capillarity-errors (which, even when the precautions I have indicated are taken, are never absolutely cancelled), the error due to the compelled imperfection of the barometric chamber (which, causing all the pressures to appear a little too low, tends to produce the illusion of a negative deviation), that due to condensation of gases on the sides of the vessels or even on the mercury, &c. &c.

By admitting an error of one or two hundredths of a millimetre,

which is no exaggeration, we arrive at divergences of the order of magnitude of the deviations found; it is therefore impossible to pronounce a decision upon either the direction or even the existence of those deviations. All that we can say is that at the lowest pressures at which experiments have been made (1 millim. or even less; I have experimented at two tenths of a millimetre) no abrupt change in the law of the compressibility of gases appears to be produced; they still follow Mariotte's law, with the exception of divergences for which the experiments cannot be responsible. It is certainly possible that sufficient rarefaction, acting like a great elevation of temperature, would cause other gases to follow the law $p(v-\alpha)=c$, as takes place for hydrogen; but there is a great distance from this to the boundary state spoken of by Mendeleef and Siljerström, in which the gases would become infinitely little compressible—a mere hypothesis, to which the numerical results of M. Siljerström do not even appear to lead, as M. Petier has already remarked in the *Journal de Physique*.

The study of carbonic acid has led me to analogous conclusions. For hydrogen the deviations found have varied between −0·0010 and 0·0028 for initial pressures between 3 and 6 millim. in round numbers.—*Comptes Rendus de l'Académie des Sciences*, Aug. 7, 1882, t. xcv. pp. 281–284.

ON THE INFLUENCE OF TEMPERATURE UPON THE SPECTRA OF METALLOIDS. BY M. D. VAN MONCKHOVEN.

Kirchhoff and Bunsen have shown that the temperature of the flame in which a substance is reduced to vapour has no influence upon the position of the bright lines of its spectrum. When, for instance, sodium or lithium is volatilized in the flame of a spirit-lamp, or in that of the oxyhydrogen blowpipe, the lines remain the same, but their brilliancy increases with the temperature: most frequently some new thin lines appear at the elevated temperatures; but it never happens that those which have already been emitted at lower temperatures disappear. If this is always the case as regards the metallic vapours, it is never so with the lines emitted by the metalloids*. Plücker has in fact shown that oxygen, nitrogen, sulphur, selenium, &c. give two different spectra which have no line in common, according as the spectral tubes containing these substances are heated by the ordinary spark of the electrical machine or by that of a Leyden jar. He admits therefore, and with him nearly every physicist, that certain elementary bodies give, at a high temperature (Leyden jar), a spectrum different from that given by the same body at a low temperature (ordinary spark).

But numerous and varied experiments have proved that we can obtain those spectra called those of *high temperature* at very

* Hydrogen is an exception; but this gas is known to be a true metal, not only as to its chemical properties, but also as to its physical. Hydrogen bears, as regards conductivity of heat and electricity, the same relation to other gases as mercury to the other liquids.

low temperatures, and *vice versâ*. Thus, at very feeble pressures (0·001 metre), with tubes of oxygen or nitrogen and with very small Leyden jars, we obtain the spectrum which Plücker attributes to high temperatures, while the tube is scarcely warm after the experiment has last several minutes, and the brilliancy of the light emitted by the incandescent gas is very feeble. The same tube, traversed by the current of a very powerful induction-coil (without the interposition of a Leyden jar), emits, on the contrary, an extremely bright light, becomes rapidly hot, and nevertheless gives the spectrum which Plücker attributes to low temperatures.

But here is a still more decisive experiment. Let us take a tube in the form of an H with four electrodes, and filled with nitrogen*, oxygen, or one of those gases (or vapours) which give two spectra. Through this tube let us pass at the same time the currents of two induction-coils, of which one has a Leyden jar interposed. *We shall observe the two spectra superposed—the spectrum assigned to elevated temperatures* (*Leyden jar*), *and the spectrum assigned to low temperatures* (*ordinary spark*).

According to Plücker's hypothesis, the gas would have, at the same physical instant, two different temperatures, which is inadmissible.

It may perhaps be objected that, the interrupters of the two coils not working strictly in unison, the perception of the two spectra is due to the persistence of the images upon the retina. But this is not the case, as some tubes, especially with oxygen, give forth light for several tenths of a second after the current has been interrupted.

We attribute the change in the spectra given by these metalloids to a vibratory state peculiar to their molecules, directly depending upon the nature of the electricity employed. Thus, a tube of highly rarefied hydrogen gas, submitted to the action of ordinary sparks, presents quite a different aspect from the same tube submitted to the action of the condensed spark.

Highly rarefied gases, traversed by the continuous current of the battery, or by a current interrupted by sparks (induction-coil), present a dynamical state well known under the name of *stratification*. But this stratification differs entirely, according as we employ the ordinary spark, the condensed spark, or the continuous current of a battery of very high tension.

We shall see in further communications that with each different behaviour of an incandescent gas (alteration in the stratification, colour of the light emitted, &c.) there is always a corresponding modification, and often an entire change, in its spectral lines—an effect assuredly independent of the temperature.—*Comptes Rendus*, Sept. 18, 1882.

ON A THERMOSCOPIC METHOD FOR THE DETERMINATION OF THE OHM. BY G. LIPPMANN.

It will be remembered that Mr. Joule† employed a calorimetric

* Nitrogen in the electric arc gives a different spectrum from that given by Geissler's tubes or the spark in air.

† Reports of the Committee &c., pp. 175–190 (London, 1873).

method for the determination of the ohm. The method which I am about to describe differs from that of the eminent physicist in not requiring the quantities of heat to be measured or the mechanical equivalent of heat E to be known. This last point is not unimportant; for in Joule's calorimetric method the final approximation is limited by the uncertainty at present existing respecting the exact value of the number E; that is to say, the possible error is near $\frac{1}{100}$.

The wire of which I wish to know the resistance is placed in the middle of a vessel arranged as a calorimeter in the centre of an enclosure with a constant temperature. An electric current is passed into the wire, and its intensity i measured. I wait until, in consequence of the heat liberated by the current, the vessel attains a stationary temperature; I leisurely ascertain that it is so by employing a thermometer, or, rather, a sensitive *thermoscope*, placed inside the vessel. This done, I interrupt the current, and then set in action a motor which produces friction in the midst of the vessel that already contains the wire. The heat evolved by the friction is substituted for that just before evolved by the current. I manage so that the stationary temperature resumes its former value; I then have $ri^2=T$, T being the work expended, whence the value of r. It is scarcely necessary to add that the friction-apparatus must remain in the vessel which contains it, even when it is not in operation, and be furnished with the known arrangements for measuring T. It is also more convenient to commence by the friction-experiment, and afterwards to regulate the intensity i so as to recover the same stationary temperature. Lastly, it may be advantageous, for apparatus of large capacity, to replace the observation of the stationary temperature by that of the velocity of the heating.

In the form given to it by Joule in 1867, the calorimetric method of the English physicist rests equally upon the measurement of i and the measurement of a mechanical work, namely the work done at the time of the determination of E. Moreover, it involves two calorimetric measurements, which are to be mutually eliminated from the final result—namely, the calorimetric measurement which accompanies the determination of E, and that which accompanies the passage of the electric current. These intermediate determinations bring in their causes of error and their corrections, owing to the imperfections of the calorimeters employed in making them. I dispense with them by taking care to expend the work T and the electric energy ri^2 in one and the same calorimetric vessel. It becomes as needless to ascertain the quantity of heat evolved in that vessel as to ascertain the weight of the tare in a double weighing; and the advantage obtained appears analogous to that which there would be in replacing two successive single weighings, made with different balances and different weights, by a Borda's double weighing.—*Comptes Rendus de l'Académie des Sciences*, Oct. 9, 1882, t. xcv. pp. 634, 635.

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XLVI. *On Variations in the Vertical due to Elasticity of the Earth's Surface.* By G. H. Darwin, *F.R.S., formerly Fellow of Trinity College, Cambridge**.

1. *On the Mechanical Effects of Barometric Pressure on the Earth's Surface.*

THE remarks of Signore de Rossi, on the observed connexion between barometric storms and the disturbance of the vertical, have led me to make the following investigation of the mechanical effects which are caused by variations of pressure acting on an elastic surface. The results seem to show that the direct measurement of the lunar disturbance of gravity must for ever remain impossible.

The practical question is to estimate the amount of distortion to which the upper strata of the earth's mass are subjected, when a wave of barometric depression or elevation passes over the surface. The solution of the following problem should give us such an estimate.

Let an elastic solid be infinite in one direction, and be bounded in the other direction by an infinite plane. Let the surface of the plane be everywhere acted on by normal pressures and tractions, which are expressible as a simple harmonic function of distances measured in some fixed direction along

* Appendix to the Second Report of the Committee of the British Association on the Lunar Disturbance of Gravity. Read at the Meeting at Southampton, August 1882. Communicated by the Author.

the plane. It is required to find the form assumed by the surface, and generally the condition of internal strain.

This is clearly equivalent to the problem of finding the distortion of the earth's surface produced by parallel undulations of barometric elevation and depression. It is but a slight objection to the correctness of a rough estimate of the kind required, that barometric disturbances do not actually occur in parallel bands, but rather in circles. And when we consider the magnitude of actual terrestrial storms, it is obvious that the curvature of the earth's surface may be safely neglected.

This problem is mathematically identical with that of finding the state of stress produced in the earth by the weight of a series of parallel mountains. The solution of this problem has recently been published in a paper by me in the 'Philosophical Transactions' (part ii. 1882, pp. 187–230); and the solution there found may be adapted to the present case in a few lines.

The problem only involves two dimensions. If the origin be taken in the mean horizontal surface, which equally divides the mountains and valleys, and if the axis of z be horizontal and perpendicular to the mountain-chains, and if the axis of x be drawn vertically downwards, then the equation to the mountains and valleys is supposed to be

$$x = -h \cos \frac{z}{b},$$

so that the wave-length from crest to crest of the mountain-ranges is $2\pi b$.

The solution may easily be found from the analysis of section 7 of the paper referred to. It is as follows:—

Let α, γ be the displacements at the point x, z vertically downwards and horizontally (α has here the opposite sign to the α of (44)). Let w be the density of the rocks of which the mountains are composed, g gravity, ν modulus of rigidity: then

$$\left.\begin{aligned} \alpha &= \frac{1}{2\nu} b \left[x \frac{d\mathrm{W}}{dx} - \mathrm{W} \right], \\ \gamma &= \frac{1}{2\nu} b x \frac{d\mathrm{W}}{dz}, \\ \text{where} \quad \mathrm{W} &= -gwh\, e^{-x/b} \cos \frac{z}{b}. \end{aligned}\right\} \quad \ldots\ldots \quad (1)$$

From these we have at once

$$\left.\begin{aligned} \alpha &= \frac{gwh}{2\nu} b\left(1+\frac{x}{b}\right)e^{-x/b}\cos\frac{z}{b}, \\ \gamma &= \frac{gwh}{2\nu} x e^{-x/b}\sin\frac{z}{b}, \\ \frac{d\alpha}{dz} &= -\frac{gwh}{2\nu}\left(1+\frac{x}{b}\right)e^{-x/b}\sin\frac{z}{b}. \end{aligned}\right\} \quad . \quad . \quad . \quad (2)^*$$

The first of these gives the vertical displacement, the second the horizontal, and the third the inclination to the horizon of strata primitively plane.

At the surface,

$$\left.\begin{aligned} \alpha &= \frac{gwh}{2\nu} b\cos\frac{z}{b}, \quad \gamma = 0, \\ \frac{d\alpha}{dz} &= -\frac{gwh}{2\nu}\sin\frac{z}{b}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Hence the maximum vertical displacement of the surface is $\pm gwhb/2\nu$, and the maximum inclination of the surface to the horizon is

$$\pm \text{ cosec } 1'' \times gwh/2\nu \text{ seconds of arc.}$$

Before proceeding further, I shall prove a very remarkable relation between the slope of the surface of an elastic horizontal plane and the deflection of the plumb-line caused by the direct attraction of the weight producing that slope. This relation was pointed out to me by Sir William Thomson, when I told him of the investigation on which I was engaged; but I am alone responsible for the proof as here given. He writes that he finds that it is not confined simply to the case where the solid is incompressible; but in this paper it will only be proved for that case.

Let there be positive and negative matter distributed over the horizontal plane according to the law $wh\cos(z \div b)$: this

* It is easy to verify that these values of α and γ, together with the value $p = gwh\, e^{-x \div b}\cos z \div b$ for the hydrostatic pressure, satisfy all the conditions of the problem, by giving normal pressure $gwh\cos z \div b$ at the free surface of the infinite plane, and satisfying the equations of internal equilibrium throughout the solid. I take this opportunity of remarking that the paper from which this investigation is taken contains an error, inasmuch as the hydrostatic pressure is erroneously determined in section 1. The term $-W$ should be added to the pressure as determined in (3). This adds W to the normal stresses P, Q, R throughout the paper, but leaves the difference of stresses (which was the thing to be determined) unaffected. If the reader should compare the stresses as determined from the values of α, γ in the text above, and from the value of p given in this note, with (38) of the paper referred to, he is warned to remember the missing term W.

forms, in fact, harmonic mountains and valleys on the infinite plane. We require to find the potential and attraction of such a distribution of matter.

Now the potential of an infinite straight line, of line-density ρ, at a point distant d from it, is well known to be $-2\mu\rho \log d$, where μ is the attraction between unit masses at unit distance apart. Hence the potential V of the supposed distribution of matter at the point x, z is given by

$$V = -2\mu wh \int_{-\infty}^{+\infty} \cos\frac{\zeta}{b} \log \sqrt{\{x^2+(\zeta-z)^2\}}\, d\zeta$$

$$= -\mu whb \left\{ \left[\sin\frac{\zeta}{b} \log\{x^2+(\zeta-z)^2\} \right]_{-\infty}^{+\infty} - 2\int_{-\infty}^{+\infty} \frac{(\zeta-z)\sin(\zeta/b)}{x^2+(\zeta-z)^2}\, d\zeta \right.$$

It is not hard to show that the first term vanishes when taken between the limits.

Now put $t = \dfrac{\zeta-x}{x}$ so that $\sin\dfrac{\zeta}{b} = \sin\dfrac{tx}{b}\cos\dfrac{z}{b} + \cos\dfrac{tx}{b}\sin\dfrac{z}{b}$,

and we have

$$V = 2\mu whb \int_{-\infty}^{+\infty} \left(\sin\frac{tx}{b}\cos\frac{z}{b} + \cos\frac{tx}{b}\sin\frac{z}{b} \right) \frac{t\,dt}{1+t^2}.$$

But it is known* that

$$\int_{-\infty}^{+\infty} \frac{t\sin ct\, dt}{1+t^2} = \pi e^{-c}, \quad \int_{-\infty}^{+\infty} \frac{t\cos ct}{1+t^2}\, dt = 0.$$

Therefore

$$V = 2\pi\mu whbe^{-x/b}\cos\frac{z}{b}.$$

If g be gravity, a earth's radius, and δ earth's mean density,

$$2\pi\mu = \frac{3g}{2a\delta}.$$

And

$$V = \frac{3gwh}{2a\delta} be^{-x/b}\cos\frac{z}{b}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The deflection of the plumb-line at any point on the surface denoted by $x=0$, and z, is clearly dV/gdz, when $x=0$. Therefore

$$\text{the deflection} = -\frac{1}{g} \times \frac{3gwh}{2a\delta} \sin\frac{z}{b}. \quad . \quad . \quad . \quad (5)$$

But from (2) the slope (or $\dfrac{d\alpha}{dz}$, when z is zero) is

$$-\frac{gwh}{2v}\sin\frac{z}{b}.$$

* See Todhunter's 'Integ. Calc.': chapter on "Definite Integrals."

Therefore deflection bears to slope the same ratio as v/g to $\frac{1}{3}a\delta$. This ratio is independent of the wave-length $2\pi b$ of the undulating surface, of the position of the origin, and of the azimuth in the plane of the line normal to the ridges and valleys. Therefore the proposition is true of any combination whatever of harmonic undulations ; and as any inequality may be built up of harmonic undulations, it is generally true of inequalities of any shape whatever.

Now $a = 6{\cdot}37 \times 10^8$ centim., $\delta = 5\frac{2}{3}$; and $\frac{1}{3}a\delta = 12{\cdot}03 \times 10^8$ grammes per square centimetre. The rigidity of glass in gravitation-units ranges from $1{\cdot}5 \times 10^8$ to $2{\cdot}4 \times 10^8$. Therefore the slope of a very thick slab of the rigidity of glass, due to a weight placed on its surface, ranges from 8 to 5 times as much as the deflection of the plumb-line due to the attraction of that weight. Even with rigidity as great as steel (viz. about 8×10^8), the slope is $1\frac{1}{2}$ times as great as the deflection.

A practical conclusion from this is that, in observations with an artificial horizon, the disturbance due to the weight of the observer's body is very far greater than that due to the attraction of his mass. This is in perfect accordance with the observations made by my brother and me with our pendulum in 1881, when we concluded that the warping of the soil by our weight when standing in the observing-room was a very serious disturbance, whilst we were unable to assert positively that the attraction of weights placed near the pendulum was perceptible. It also gives emphasis to the criticism we have made on M. Plantamour's observations—namely, that he does not appear to take special precautions against the disturbance due to the weight of the observer's body.

We must now consider the probable numerical values of the quantities involved in the barometric problem, and the mode of transition from the problem of the mountains to that of barometric inequalities.

The modulus of rigidity in gravitation-units (say grammes weight per square centimetre) is v/g. In the problem of the mountains, wh is the mass of a column of rock of one square centimetre in section and of length equal to the height of the crests of the mountains above the mean horizontal plane. In the barometric problem, wh must be taken as the mass of a column of mercury of a square centimetre in section and equal in height to a half of the maximum range of the barometer.

This maximum range is, I believe, nearly two inches, or, let us say, 5 centim.

The specific gravity of mercury is 13·6; and therefore $wh = 34$ grammes.

The rigidity of glass is from 150 to 240 million grammes

per square centimetre, that of copper 540, and of steel 843 millions.

I will take $v/g = 3 \times 10^8$; so that the superficial layers of the earth are assumed to be more rigid than the most rigid glass. It will be easy to adjust the results afterwards to any other assumed rigidity.

With these data we have

$$\frac{gwh}{2v} = \frac{5{\cdot}67}{10^8};$$

also

$$\frac{648{,}000}{\pi} \times \frac{5{\cdot}67}{10^8} = 0''{\cdot}0117.$$

It seems not unreasonable to suppose that 1500 miles ($2{\cdot}4 \times 10^8$ centim.) is the distance from the place where the barometer is high (the centre of the anti-cyclone) to that where it is low (the centre of the cyclone). Accordingly the wave-length of the barometric undulation is $4{\cdot}8 \times 10^8$ centim., and $b = 4{\cdot}8 \times 10^8 \div 6{\cdot}28$ centim., or, say, $b = {\cdot}8 \times 10^8$ centim.

Thus, with these data,

$$\frac{gwh}{2v} b = 4{\cdot}5 \text{ centim.}$$

We thus see that the ground is 9 centim. higher under the barometric depression than under the elevation.

If the sea had time to attain its equilibrium slope, it would stand $5 \times 13{\cdot}6$, or 68 centim. lower under the high pressure than under the low. But as the land is itself depressed 9 centim., the sea would apparently only be depressed 59 centim. under the high barometer.

It is probable that, in reality, the larger barometric inequalities do not linger quite long enough over particular areas to permit the sea to attain everywhere its due slope, and therefore the full difference of water-level can only be attained occasionally.

On the other hand, the elastic compression of the ground must take place without any sensible delay. Thus it seems probable that the elastic compression of the ground must exercise a very sensible effect in modifying the apparent depression or elevation of the sea under high and low barometer.

It does not appear absolutely chimerical that at some future time, when both tidal and barometric observations have attained to great accuracy, an estimate might thus be made of the average modulus of rigidity of the upper 500 miles of the earth's mass.

Even in the present condition of barometric and tidal infor-

mation, it might be interesting to make a comparison between the computed height of tide and the observed height, in connexion with the distribution of barometric pressure. It is probable that India would be the best field for such an attempt, because the knowledge of Indian tides is more complete than that for any other part of the world. On the other hand, we shall see in the following section that tidal observations on coast-lines of continents are liable to disturbance, so that an oceanic island would be a more favourable site.

It has already been shown that the maximum apparent deflection of the plumb-line, consequent on the elastic compression of the earth, amounts to $0''{\cdot}0117$; and this is augmented to $0''{\cdot}0146$, when we include the true deflection due to the attraction of the air. It is worthy of remark that this result is independent of the wave-length of the barometric inequality, and thus we get rid of one of the conjectural data.

Thus, if we consider the two cases of high pressure to right and low to left, and of low pressure to right and high to left, we see that there will be a difference in the position of the plumb-line relatively to the earth's surface of $0''{\cdot}0292$. Even if the rigidity of the upper strata of the earth were as great as that of steel, there would still be a change of $0''{\cdot}011$.

A deflection of magnitude such as $0''{\cdot}03$ or $0''{\cdot}01$ would have been easily observable with our instrument of last year; for we concluded that a change of $\frac{1}{200}$ of a second could be detected when the change occurred rapidly.

It was stated in our previous Report that at Cambridge the calculated amplitude of oscillation of the plumb-line due directly to lunar disturbance of gravity amounts to $0''{\cdot}0216$. Now, as this is less than the amplitude due jointly to elastic compression and attraction, with the assumed rigidity (300 millions) of the earth's strata, and only twice the result if the rigidity be as great as that of steel, it follows almost certainly that from this cause alone the measurement of the lunar disturbance of gravity must be impossible with any instrument on the earth's surface.

Moreover the removal of the instrument to the bottom of the deepest known mine would scarcely sensibly affect the result, because the flexure of the strata at a depth so small, compared with the wave-length of barometric inequalities, is scarcely different from the flexure of the surface.

The diurnal and periodic oscillations of the vertical observed by us were many times as great as those which have just been computed; and therefore it must not be supposed that more than a fraction, say perhaps a tenth, of those oscillations was due to elastic compression of the earth.

The Italian observers could scarcely with their instruments detect deflections amounting to $\frac{1}{100}$ of a second; so that the observed connexion between barometric oscillation and seismic disturbance must be of a different kind.

It is not surprising that in a volcanic region the equalization of pressure, between imprisoned fluids and the external atmosphere, should lead to earthquakes.

If there is any place on the earth's surface free from seismic forces, it might be possible (if the effect of tides as computed in the following section could be eliminated) with some such instrument as ours, placed in a deep mine, to detect the existence of barometric disturbance many hundreds of miles away. It would of course for this purpose be necessary to note the positions of the sun and moon at the times of observation, and to allow for their attraction.

2. *On the Disturbance of the Vertical near the Coasts of Continents due to the Rise and Fall of the Tide.*

Consider the following problem:—

On an infinite horizontal plane, which bounds in one direction an infinite incompressible elastic solid, let there be drawn a series of parallel straight lines, distance l apart. Let one of these be the axis of y, let the axis of z be drawn in the plane, perpendicular to the parallel lines, and let the axis of x be drawn vertically downwards through the solid.

At every point of the surface of the solid, from $z=0$ to l, let a normal pressure $gwh(1-2z/l)$ be applied; and from $z=0$ to $-l$ let the surface be free from forces. Let the same distribution of force be repeated over all the pairs of strips into which the surface is divided by the system of parallel straight lines. It is required to determine the strains caused by these forces.

Taking the average over the whole surface, there is neither pressure nor traction, since the total traction on the half-strips subject to traction is equal to the total pressure on the half-strips subject to pressure.

The following is the analogy of this system with that which we wish to discuss: the strips subject to no pressure are the continents, the alternate ones are the oceans, g is gravity, w the density of water, and h the height of tide above mean water on the coast-line.

We require to find the slope of the surface at every point, and the vertical displacement.

It is now necessary to bring this problem within the range of the results used in the last section. In the first place, it is convenient to consider the pressures and tractions as caused by

mountains and valleys whose outline is given by $x=-h(1-2z/l)$ from $z=0$ to l, and $x=0$ from $z=0$ to $-l$. To utilize the analysis of the last section, it is necessary that the mountains and valleys should present a simple-harmonic outline. Hence the discontinuous function must be expanded by Fourier's method. Known results of that method render it unnecessary to have recourse to the theorem itself. It is known that

$$\pm\tfrac{1}{2}\pi-\tfrac{1}{2}\theta=\sin\theta+\tfrac{1}{2}\sin 2\theta+\tfrac{1}{3}\sin 3\theta+\ldots$$

$$-\tfrac{1}{2}\theta=-\sin\theta+\tfrac{1}{2}\sin 2\theta-\tfrac{1}{3}\sin 3\theta+\ldots$$

$$\tfrac{1}{2}\pi\mp\theta=\frac{4}{\pi}\left\{\cos\theta+\frac{1}{3^2}\cos 3\theta+\frac{1}{5^2}\cos 5\theta+\ldots\right\},$$

the upper sign being taken for values of θ between the infinitely small positive and $+\pi$, and the lower for values between the infinitely small negative and $-\pi$.

Adding these three series together, we have

$$2\left\{\tfrac{1}{2}\sin 2\theta+\tfrac{1}{4}\sin 4\theta+\ldots\right\}+\frac{4}{\pi}\left\{\cos\theta+\frac{1}{3^2}\cos 3\theta+\frac{1}{5^2}\cos 5\theta+\ldots\right\}$$

equal to $\pi-2\theta$ from $\theta=0$ to $+\pi$, and equal to zero from $\theta=0$ to $-\pi$. Hence the required expansion of the discontinuous function is

$$\left.\begin{aligned}&-\frac{2h}{\pi}\left\{\tfrac{1}{2}\sin 2\theta+\tfrac{1}{4}\sin 4\theta+\ldots\right\},\\&-\frac{4h}{\pi^2}\left\{\cos\theta+\frac{1}{3^2}\cos 3\theta+\frac{1}{5^2}\cos 5\theta+\ldots\right\},\end{aligned}\right\}\quad(6)$$

where

$$\theta=\frac{\pi z}{l};\qquad(7)$$

for it vanishes from $z=-l$ to 0, and is equal to $-h(1-2z/l)$ from $z=0$ to $+l$.

Now, looking back to the analysis of the preceding section, we see that, if the equation to the mountains and valleys had been $x=-h\sin(z/b)$, α would have had the same form as in (2), but of course with sine for cosine, and γ would have changed its sign and a cosine would have stood for the sine. Applying then the solution (2) to each term of our expansion separately, and only writing down the solution for the surface at which $x=0$, we have at once that $\gamma=0$, and

$$\left.\begin{aligned}\alpha=&\frac{gwh}{\pi\upsilon}\cdot\frac{l}{\pi}\left\{\frac{1}{2^2}\sin 2\theta+\frac{1}{4^2}\sin 4\theta+\frac{1}{6^2}\sin 6\theta+\ldots\right\},\\&+\frac{gwh}{\pi\upsilon}\cdot\frac{2l}{\pi^2}\left\{\cos\theta+\frac{1}{3^3}\cos 3\theta+\frac{1}{5^3}\cos 5\theta+\ldots\right\}.\end{aligned}\right\}\quad(8)$$

The slope of the surface is $\frac{d\alpha}{dz}$ or $\frac{\pi}{l}\frac{d\alpha}{d\theta}$; thus

$$\frac{d\alpha}{dz} = \frac{gwh}{\pi v}\left\{\tfrac{1}{2}\cos 2\theta + \tfrac{1}{4}\cos 4\theta + \tfrac{1}{6}\cos 6\theta + \ldots\right\} - \frac{gwh}{\pi v}\cdot\frac{2}{\pi}\left\{\sin\theta + \frac{1}{3^2}\sin 3\theta + \frac{1}{5^2}\sin 5\theta + \ldots\right\}. \quad (9)$$

The formulæ (8) and (9) are the required expressions for the vertical depression of the surface and for the slope.

It is interesting to determine the form of surface denoted by these equations. Let us suppose, then, that the units are so chosen that $gwhl/\pi^2 v$ may be equal to one. Then (8) and (9) become

$$\alpha = \frac{1}{2^2}\sin 2\theta + \frac{1}{4^2}\sin 4\theta + \ldots + \frac{2}{\pi}\left\{\frac{1}{1^3}\cos\theta + \frac{1}{3^3}\cos 3\theta + \ldots\right\} \quad (10)$$

$$\frac{d\alpha}{d\theta} = \tfrac{1}{2}\cos 2\theta + \tfrac{1}{4}\cos 4\theta + \ldots - \frac{2}{\pi}\left\{\frac{1}{1^2}\sin\theta + \frac{1}{3^2}\sin 3\theta + \ldots\right\}. \quad (11)$$

When θ is zero or $\pm\pi$, $d\alpha/d\theta$ becomes infinite, which denotes that the tangent to the warped horizontal surface is vertical at these points. The verticality of these tangents will have no place in reality, because actual shores shelve, and there is not a vertical wall of water when the tide rises, as is supposed to be the case in the ideal problem. We shall, however, see that in practical numerical application, the strip of sea-shore along which the solution shows a slope of more than $1''$ is only a small fraction of a millimetre. Thus this departure from reality is of no importance whatever.

When $\theta = 0$ or $\pm\pi$,

$$\alpha = \frac{2}{\pi}\left\{\frac{1}{1^3} + \frac{1}{3^3} + \frac{1}{5^3} + \ldots\right\} = \frac{2}{\pi}\times 1{\cdot}052 = {\cdot}670, \quad (12)$$

being + when $\theta = 0$, and − when $\theta = \pm\pi$.

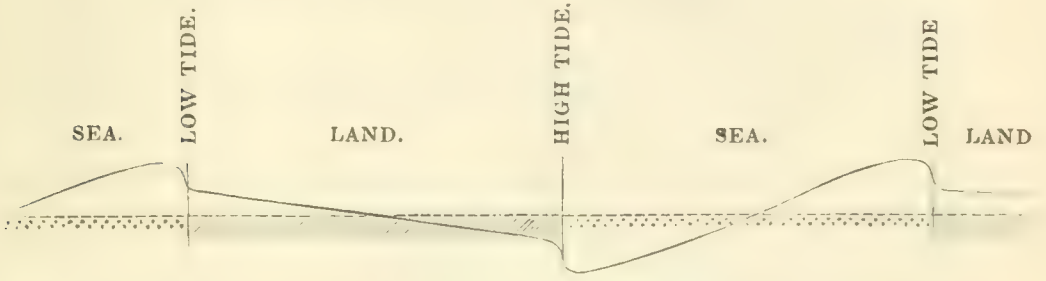
When $\theta = \pm\frac{1}{2}\pi$, α vanishes; and therefore midway in the ocean and on the land there are nodal lines, which always remain in the undisturbed surface, when the tide rises and falls. At these nodal lines, defined by $\theta = \pm\frac{1}{2}\pi$,

$$\frac{d\alpha}{d\theta} = -\tfrac{1}{2}\log_e 2 \mp \frac{2}{\pi}\left\{\frac{1}{1^3} - \frac{1}{3^3} + \frac{1}{5^3} - \ldots\right\}$$

$$= -{\cdot}3466 \mp {\cdot}6168 = -{\cdot}9634 \text{ and } +{\cdot}2702.$$

Thus the slope is greater at mid-ocean than at mid-land. By assuming θ successively as $\frac{1}{6}\pi$, $\frac{1}{4}\pi$, $\frac{1}{3}\pi$, and summing arithmetically the strange series which arise, we can, on pay-

ing attention to the manner in which the signs of the series occur, obtain the values of α corresponding to 0, $\pm\frac{1}{6}\pi$, $\pm\frac{1}{4}\pi$, $\pm\frac{2}{6}\pi$, $\pm\frac{3}{6}\pi$, $\pm\frac{4}{6}\pi$, $\pm\frac{5}{6}\pi$, $\pm\frac{6}{6}\pi$. The resulting values, together with the slopes as obtained above, are amply sufficient for drawing a figure, as shown annexed.



The straight line is a section of the undisturbed level, the shaded part being land, and the dotted sea. The curve shows the distortion, when warped by high and low tide as indicated.

The scale of the figure is a quarter of an inch to $\frac{1}{6}\pi$ for the abscissas, and a quarter of an inch to unity for the ordinates; it is of course an enormous exaggeration of the flexure actually possibly due to tides.

It is interesting to note that the land-regions remain very nearly flat, rotating about the nodal line, but with slight curvature near the coasts. It is this curvature, scarcely perceptible in the figure, which is of most interest for practical application.

The series (8) and (9) are not convenient for practical calculation in the neighbourhood of the coast, and they must be reduced to other forms. It is easy, by writing the cosines in their exponential form, to show that

$$\cos\theta+\tfrac{1}{2}\cos 2\theta+\tfrac{1}{3}\cos 3\theta+\ldots=-\log_e(\pm 2\sin\tfrac{1}{2}\theta), \quad . \quad (13)$$

$$\cos\theta-\tfrac{1}{2}\cos 2\theta+\tfrac{1}{3}\cos 3\theta-\ldots=\quad \log_e(2\cos\tfrac{1}{2}\theta), \quad . \quad . \quad (14)$$

where the upper sign in (13) is to be taken for positive values of θ and the lower for negative.

For the small values of θ with which alone we are at present concerned, the series (13) becomes $-\log_e(\pm\theta)$ and the lower $\log_e 2$.

Taking half the difference and half the sum of the two series, we have

$$\tfrac{1}{2}\cos 2\theta+\tfrac{1}{4}\cos 4\theta+\ldots\ldots=-\tfrac{1}{2}\log(\pm\theta)-\tfrac{1}{2}\log 2, \quad . \quad (15)$$

$$\cos\theta+\tfrac{1}{3}\cos 3\theta+\tfrac{1}{5}\cos 5\theta+=-\tfrac{1}{2}\log(\pm\theta)+\tfrac{1}{2}\log 2. \quad . \quad (16)$$

Integrating (16) with regard to θ, and observing that the

constant introduced on integration is zero, we have

$$\sin\theta+\frac{1}{3^2}\sin 3\theta+\frac{1}{5^2}\sin 5\theta+\ldots=-\tfrac{1}{2}\theta[\log(\pm\theta)-1]+\tfrac{1}{2}\theta\log 2. \quad . \quad . \quad (17)$$

Then, from (15) and (17),

$$\left.\begin{aligned}\tfrac{1}{4}\cos 2\theta+\tfrac{1}{4}\cos 4\theta+\ldots-\frac{2}{\pi}\left\{\sin\theta+\frac{1}{3^2}\sin 3\theta+\ldots\right\}\\ =-\tfrac{1}{2}\left(1-\frac{2\theta}{\pi}\right)\log(\pm\theta)-\tfrac{1}{2}\left(1+\frac{2\theta}{\pi}\right)\log 2-\frac{\theta}{\pi}.\end{aligned}\right\} \quad (18)$$

Integrating (15), and observing that the constant is zero, we have

$$\frac{1}{2^2}\sin 2\theta+\frac{1}{4^2}\sin 4\theta+\ldots=-\tfrac{1}{2}\theta[\log(\pm\theta)-1]-\tfrac{1}{2}\theta\log 2. \quad (19)$$

Integrating (17), and putting in the proper constant to make the left side vanish when $\theta=0$, we have

$$\begin{aligned}\frac{1}{1^3}+\frac{1}{3^3}+\frac{1}{5^3}+\ldots-\left(\frac{1}{1^3}\cos\theta+\frac{1}{3^3}\cos 3\theta+\ldots\right)\\ =-\tfrac{1}{4}\theta^2\log(\pm\theta)+\tfrac{1}{4}\theta^2\left(\tfrac{3}{2}+\log 2\right). \quad . \quad . \quad (20)\end{aligned}$$

For purposes of practical calculation, θ may be taken as so small that the right-hand side of (18) reduces to $-\frac{1}{2}\log(\pm 2\theta)$, and the right-hand sides of (19) and (20) to zero.

Hence, by (8) and (9), we have in the neighbourhood of the coast,

$$\left.\begin{aligned}\alpha=\frac{gwh}{\pi\upsilon}\times\frac{2l}{\pi^2}\left[\frac{1}{1^3}+\frac{1}{3^3}+\frac{1}{5^3}+\ldots\right],\\ =\frac{gwh}{\pi\upsilon}\times\frac{l}{\pi^2}\times 2{\cdot}1037,\\ \frac{d\alpha}{dz}=-\frac{gwh}{2\pi\upsilon}\log_e 10\log_{10}\frac{2\pi z}{l}.\end{aligned}\right\} \quad . \quad . \quad (21)$$

I shall now proceed to compute from the formulæ (21) the depression of the surface and the slope, corresponding to such numerical data as seem most appropriate to the terrestrial oceans and continents.

Considering that the tides are undoubtedly augmented by kinetic action, we shall be within the mark in taking h as the semi-range of equilibrium tide. At the equator the lunar tide has a range of about 53 centim., and the solar tide is very nearly half as much. Therefore at spring-tides we may take $h=40$ centim. It must be noticed that the highness of the tides (say 15 or 20 feet) near the coast is due to the shallowing of the water, and it would not be just to take such values

as representing the tides over large areas; w, the density of the water, is of course unity.

If we suppose it is the Atlantic Ocean and the shores of Europe with Africa, and of North and South America, which are under consideration, it is not unreasonable to take l as 3900 miles, or $6{\cdot}28 \times 10^8$ centim. Then $2\pi z/l = z \times 10^{-8}$.

Taking v/g as 3×10^8 (that is to say, assuming a rigidity greater than that of glass), we have for the slope in seconds of arc, at a distance z from the sea-shore,

$$\operatorname{cosec} 1'' \times \frac{40}{2\pi \times 3 \times 10^8} \times \log_e 10 \times (8 - \log_{10} z)$$
$$= 0''{\cdot}01008\,(8 - \log_{10} z). \quad . \quad . \quad . \quad (22)$$

From this the following table may be computed by simple multiplication:—

Distance from mean water-mark.		Slope.
1 centim.	= 1 centim. . . .	0″·0806
10 „	= 10 „	·0706
10^2 „	= 1 metre	·0605
10^3 „	= 10 metres. . . .	·0504
10^4 „	= 100 „	·0403
10^5 „	= 1 kilom. . . .	·0302
10^6 „	= 10 „	·0202
2×10^6 centim.	= 20 „	·0170
5×10^6 „	= 50 „	·0131
10^7 centim.	= 100 „	·0101

On considering the formula (22), it appears that z must be a very small fraction of a millimetre before the slope becomes even as great as 1′. This proves that the rounded nick in the surface, which arises from the discontinuity of pressure at our ideal mean water-mark, is excessively small; and the vertical displacement of the surface is sensibly the same, when measured in centimetres, on each side of the nick, in accordance with the first of (21).

The result (5) of section 1 shows that, with rigidity 3×10^8, the true deflection of plumb-line due to attraction of the water is a quarter of the slope. Hence an observer in a gravitational observatory at distance z from mean water-mark, would note deflections from the mean position of the vertical $1\frac{1}{4}$ times as great as those computed above; and as high water changes to low, there would be oscillations of the vertical $2\frac{1}{2}$ times as great. We thus get the practical results in the following table:—

Distance of observatory from mean water-mark.	Amplitude of apparent oscillation of the vertical.
10 metres	0″·126
100 „	·101
1 kilom.	·076
10 „	·050
20 „	·042
50 „	·035
100 „	·025

It follows, from the calculations made for tracing the curve, that halfway across the continent (that is to say, 3142 kilometres from either coast) the slope is

$$\frac{648{,}000}{\pi} \times \frac{gwh}{\pi v} \times \cdot 2703 \text{ second of arc} = 0'' \cdot 00237,$$

and the range of apparent oscillation is 0″·006.

In these calculations the width of the sea is taken as 6283 kilometres. If the sea be narrower, then, to obtain the same deflections of the plumb-line, the observatory must be moved nearer the sea in the same proportion as the sea is narrowed. If, for example, the sea were 3142 kilometres wide, then at 10 kilometres from the coast the apparent amplitude of deflection would be 0″·042. If the range of tide is greater than that here assumed (viz. 89 centim.), the results must be augmented in the same proportion. And, lastly, if the rigidity of the rock be greater or less than the assumed value (viz. 3×10^8), the part of the apparent deflection depending on slope must be diminished or increased in the inverse proportion to the change in rigidity.

I think there can be little doubt that in narrow seas the tides are generally much greater than those here assumed; and it is probable that at a gravitational observatory actually on the sea-shore on the south coast of England, apart from seismic changes, perceptible oscillations of the vertical would be noted.

Sir William Thomson has made an entirely independent estimate of the probable deflection of the plumb-line at a sea-side gravitational observatory*. He estimates the attraction of a slab of water 10 feet thick (the range of tide), 50 miles broad perpendicular to the coast, and 100 miles long parallel with coast, on a plummet 100 yards from the low-water mark, and opposite the middle of the 100 miles of length. He thinks this estimate would very roughly represent the state of things

* Thomson and Tait's 'Natural Philosophy,' § 818.

say at St. Alban's Head. He finds, then, that the deflection of the plumb-line as high tide changes to low would be $\frac{1}{4000000}$ of the unit angle, or $0''{\cdot}050$. The general theorem proved above, as to the proportionality of slope to attraction, shows that, with rigidity 3×10^8 for the rocks of which the earth is formed, the apparent deflection of the plumb-line would amount to $0''{\cdot}25$.

It is just possible that a way may in this manner be opened for determining the modulus of rigidity of the upper 100 or 200 miles of the earth's surface, although the process would be excessively laborious. The tides of the British Channel are pretty well known; and therefore it would be possible by very laborious quadratures to determine the deflection of the plumb-line due to the attraction of the tide at any time at a chosen station. If, then, the deflection of the plumb-line could be observed at that station (with corrections applied for the positions of the sun and moon), the ratio of the calculated to the observed and corrected deflection, together with the known value of the earth's radius and mean density, form the materials for computing the rigidity. But such a scheme would be probably rendered abortive by just such comparatively large and capricious oscillations of the vertical as we, M. d'Abbadie, and others have observed.

It is interesting to draw attention to some observations of M. d'Abbadie on the deflections of the vertical due to tides. His observatory (of which an account was given in the Report for 1881) is near Hendaye, in the Pyrenees, and stands 72 metres above and 400 metres distant from the sea. He writes*:—

"J'ai réuni 359 comparaisons d'observations spéciales faites lors du maximum du flot et du jusant ; 243 seulement sont favorables à la théorie de l'attraction exercée par la masse des eaux, et l'ensemble des résultats pour une différence moyenne de marées égale à 2·9 mètres donne un résultat moyen de $0''{\cdot}56$ ou $0''{\cdot}18$ pour le double de l'attraction angulaire vers le Nord-Ouest. Ceci est conforme à la théorie, car les différences observées doivent être partagées par moitié, selon la loi de la réflexion ; mais comme il y a toujours de l'inattendu dans les expériences nouvelles, on doit ajouter que sur les 116 comparaisons restantes il y en a eu 57 où le flot semble repousser le mercure au lieu de l'attirer. Mes résultats ont été confirmés pendant l'hiver dernier par M. l'abbé Artus, qui a eu la patience de comparer ainsi 71 flots et 73 jusants consécutifs, de janvier à mars 1880. Lui aussi a trouvé un tiers environ

* "Recherches sur la Verticale," *Ann. de la Soc. Scient. de Bruxelles*, 1881.

de cas défavorables à nos théories admises. On est donc en droit d'affirmer que si la mer haute attire le plus souvent le pied du fil à plomb, il y a une, et peut-être plusieurs, autres forces en jeu pour faire varier sa position."

We must now consider the vertical displacement of the land near the coast. In (21) it is shown to be

$$\alpha_0 = \frac{gwh}{\pi v} \times \frac{l}{\pi^2} \times 2{\cdot}1037,$$

where α_0 indicates the displacement corresponding to $z=0$.

With the assumed values, $h=40$, $v=3\times10^8$, $l=6{\cdot}28\times10^8$, I find $\alpha_0=5{\cdot}684$ centim. Hence the amplitude of vertical displacement is 11·37 centim. As long as hl remains constant this vertical displacement remains the same; hence the high tides of 10 or 15 feet which are actually observed on the coasts of narrow seas must probably produce vertical oscillations of quite the same order as that computed.

If the land falls, the tide of course rises higher on the coast-line than it would do otherwise; hence the apparent height of tide would be $h+\alpha_0$. But this shows there is more water resting on the earth than according to the estimated value h; hence the depression of the soil is greater in the proportion $1+\alpha_0/h$ to unity; this again causes more tide, which reacts and causes more depression, and so on. Thus on the whole the augmentation of tide due to elastic yielding is in the ratio of

$$1+\frac{\alpha_0}{h}+\left(\frac{\alpha_0}{h}\right)^2+\left(\frac{\alpha_0}{h}\right)^3+\ldots \text{ or } \frac{1}{1-\alpha_0/h} \text{ to unity.}$$

This investigation is conducted on the equilibrium theory; and it neglects the curvature of the sea-bed, assuming that there is a uniform slope from mid-ocean to the sea-coast. The figure shows that this is not rigorously the case; but it is quite near enough for a rough approximation. The phenomena of the short-period tides are so essentially kinetic that the value of this augmentation must remain quite uncertain; but for the long-period tides (the fortnightly and monthly elliptic) the augmentation must correspond approximately with the ratio

$$1:\left(1-\frac{gwl}{\pi^3 v}\times 2{\cdot}1037\right).$$

The augmentation in narrow seas will be small; but in the Atlantic Ocean the augmenting factor must agree pretty well with that which I now compute*.

* Sir William Thomson has pointed out to me, since the meeting of the Association, that this augmentation will only hold true in the cases of certain distributions of land.

With the previous numerical values we have α_0/h (which is independent of h) equal to ·1421, and $1-\alpha_0/h=·8579=\frac{6}{7}$ very nearly.

Thus the long-period tides may probably undergo an augmentation at the coasts of the Atlantic in some such ratio as 6 to 7.

The influence of this kind of elastic yielding is antagonistic to that reduction of apparent tide which must result from an elastic yielding of the earth's mass as a whole.

The reader will probably find it difficult to estimate what degree of probability of correctness there is in the conjectural value of the rigidity, which has been used in making the numerical calculations in this paper. The rigidity has not been experimentally determined for many substances; but a great number of experiments have been made to find Young's modulus. Now, in the stretching of a bar or wire the compressibility plays a much less important part than the rigidity; and the formula for Young's modulus shows that for an incompressible elastic solid the modulus is equal to three times the rigidity*. Hence a third of Young's modulus will form a good standard of comparison with the assumed rigidity, namely 3×10^8 grammes weight per square centimetre. The following are a few values of a third of Young's modulus and of rigidity, taken from the tables in Sir William Thomson's article on Elasticity† in the *Encyclopedia Britannica*:—

Material.	A third of Young's modulus and rigidity in terms of 10^8 grammes weight per square centimetre.
Stone	About 1·2
Slate	About 3 to 4.
Glass	Rigidity 1·5 to 2·4.
Ice	4·7.
Copper	4, and rigidity 4·6 to 5·4.
Steel	7 to 10, and rigidity 8·4.

It will be observed that the assumed rigidity 3 is probably a pretty high estimate in comparison with that of the materials of which we know the superficial strata to be formed.

It is shown, in another paper read before the Association at this meeting, that the rigidity of the earth as a whole is probably as great as that of steel. That result is not at all inconsistent with the probability of the assumption that the upper strata have only a rigidity a little greater than that of glass.

* Thomson and Tait's 'Natural Philosophy,' § 683.

† Also published separately by Black (Edinburgh).

3. *On Gravitational Observatories.*

In the preceding sections estimates have been made of the amount of distortion which the upper strata of the earth probably undergo from the shifting weights corresponding to barometric and tidal oscillations. These results appear to me to have an important bearing on the utility of gravitational observatories.

It is not probable, at least for many years to come, that the state of tidal and barometric pressure, for a radius of 500 miles round any spot on the earth's surface, will be known with sufficient accuracy to make even a rough approximation to the slope of the surface a possibility. And were these data known, the heterogeneity of geological strata would form a serious obstacle to the possibility of carrying out such a computation. It would do little in relieving us from these difficulties to place the observatory at the bottom of a mine.

Accordingly the prospect of determining experimentally the lunar disturbance of gravity appears exceedingly remote; and I am compelled reluctantly to conclude that continuous observations with gravitational instruments of very great delicacy are not likely to lead to results of any great interest. It appears likely that such an instrument, even in the most favourable site, would record incessant variations of which no satisfactory account could be given. Although I do not regard it as probable that such a delicate instrument should be adopted for regular continuous observations, yet, by choosing a site where the flexure of the earth's surface is likely to be great, it is conceivable that a rough estimate might be made of the average modulus of elasticity of the upper strata of the earth for one or two hundred miles from the surface.

These conclusions, which I express with much diffidence, are by no means adverse to the utility of a coarser gravitational instrument, capable, let us say, of recording variations of level amounting to $1''$ or $2''$. If barometric pressure, tidal pressure, and the direct action of the sun and moon combine together to make apparent slope in one direction, then, at an observatory remote from the sea-shore, that slope might perhaps amount to a quarter of a second of arc. Such a disturbance of level would not be important compared with the minimum deviations which could be recorded by the supposed instrument.

It would then be of much value to obtain continuous systematic observations, after the manner of the Italians, of the seismic and slower quasi-seismic variations of level.

I venture to predict that at some future time practical astronomers will no longer be content to eliminate variations of level merely by taking means of results, but will regard corrections derived from a special instrument as necessary to each astronomical observation.

XLVII. *New Views of* Mr. George H. Darwin's *Theory of the Evolution of the Earth-Moon System, considered as to its bearing on the question of the Duration of Geological Time. By the* Rev. Samuel Haughton, *M.D., Fellow of Trinity College, Dublin**.

IT has been tacitly assumed, even so far back as the times of Newton and Clairaut, that the earth and planets have passed through a liquid condition (owing to former great heat) before assuming the solid condition which some, at least, of them now possess.

Laplace, in his nebular hypothesis, also assumes the former existence of this liquid condition; and it is openly asserted by all geologists who believe that the earth consists of a solid crust (more or less thick), reposing upon a fluid or viscous nucleus.

It has been proved by Sir William Thomson, following out the views of the late Mr. Hopkins, that the present condition of the earth, taken as a whole, is such that it must be regarded as being more rigid than glass or steel, possibly more rigid than any terrestrial substance under the surface-conditions of pressure.

The following considerations show that it may be fairly doubted whether the earth or any other planet ever existed in a fluid condition.

1. The possibility of the equilibrium of the rings of Saturn, on the supposition that they are either solid or liquid, has been more than doubted, and the most probable hypothesis respecting them is, that they consist of swarms of discrete meteoric stones.

2. It is difficult to understand the low specific gravity of Jupiter and the other outer planets, on the supposition that they are either solid or liquid; for we know of no substance light enough to form them †. If the outer planets consist of

* From the 'American Journal of Science' for November 1882. Read before the Mathematical Section of the American Association for the Advancement of Science, at Montreal, August 1882.

† The force of this argument could not be felt before the revelations of the spectroscope, because at that time there was no proof that the whole universe was composed of the same simple substances, and those very limited in number.

discrete meteoric stones moving around a solid or liquid nucleus, the difficulty respecting their specific gravity would disappear.

3. The recent researches connecting the November, the August, and other periodic swarms of shooting-stars with comets, tend in the direction of showing that comets in cooling break up into discrete solid particles (each no doubt having passed through the liquid condition), and that probably the solar nebula cooled in like manner into separate fiery tears, which soon solidified by radiation into the cold of space.

4. Mr. Huggins's recent comparisons of the spectroscopic appearances of comets and incandescent portions of meteoric stones, showing the presence in both of hydrocarbon and nitrogen compounds, confirm the conclusions drawn from the identity of the paths of comets and meteoric periodic shooting-stars.

5. Mr. H. A. Newton, in a remarkable paper read before the Sheffield Meeting of the British Association (1879), showed the possibility (if not probability) of the asteroids being extinct comets, captured and brought into the solar system by the attraction of some one or other of the outer large planets, and permanently confined in the space between Mars and Jupiter, which is the only prison-cell in the solar system large enough to hold permanently such disorderly wanderers.

In the same paper Professor Newton threw out the idea that some of the satellites of the large planets might also be of cometary origin.

From all these and other considerations it is therefore allowable to suppose that the earth and moon, when they separated from the solar nebula, did so as a swarm of solid meteoric stones, each of them having the temperature of interstellar space, *i. e.* something not much warmer than 460° F. below the freezing-point of water.

Mr. George H. Darwin has shown admirably how the earth-moon system may have been developed from the time when the earth-moon formed one planet, revolving on its axis in a few hours, to the present time, when the earth and moon (in consequence of tidal friction) have pushed each other asunder to a distance of sixty times the radius of the earth *.

In his paper on the tidal friction of a planet† (supposed viscous and under the influence of bodily tides caused in it by an external body such as the moon), Mr. Darwin has found a remarkable equation of condition, which may be thus

* Proceedings of the Royal Society, 19th June, 1879.
† Phil. Trans. 1881, part ii. p. 494.

expressed :—

$$d(\sqrt{r}) \propto \frac{\Psi dt}{r^6}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

r = distance between centres of earth and moon,
t = time elapsed from a fixed point,

$$\Psi = \frac{p(n-\Omega)}{1+p^2(n-\Omega)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

n = angular velocity of earth's rotation,
Ω = angular velocity of moon's orbital revolution,
p = quantity varying inversely as the viscosity of the planet.

The extreme interest of equation (1) consists in the appearance of the inverse sixth power of the distance.

As the function Ψ varies very slowly, we find by integration, for any portion of time during which Ψ may be regarded as constant,

$$t = Ar^{\frac{13}{2}} + B, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

a most unexpected and remarkable result.

Upon reading Mr. Darwin's papers, my mind turned to a problem with which I was familiar, viz. the retardation of the earth's rotation produced by the lunisolar tide exerted upon the ocean supposed collected in an equatorial canal, the moon and sun having no declination; and I readily found an equation to express the evolution of the earth-moon system, on the foregoing hypothesis as to friction.

This equation is the following :—

$$d(\sqrt{r}) \propto \frac{f\Phi dt}{r^6}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where

$$\Phi = \frac{V_0(n-\Omega)}{\{4V_0^2(n-\Omega)^2 - k^2\}\sqrt{4(n-\Omega)^2+f^2}}, \quad . \quad . \quad . \quad . \quad (5)$$

f = coefficient of friction supposed proportional to relative velocity,
k varies inversely as r^3,
V_0 = velocity at earth's equator.

This leads, as in Mr. Darwin's hypothesis of viscous earth, to the integral

$$t = A'r^{\frac{13}{2}} + B'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The form of the functions Ψ and Φ is similar, as both ascend by odd powers of $(n-\Omega)$ and vanish when $n=\Omega$—that is to say, at the beginning and end of the evolution by friction of the earth-moon system.

It is quite clear, therefore, that the remarkable expression (1) found by Mr. Darwin is not peculiar to his special hypothesis of a viscous earth, but can be deduced equally well from the totally distinct hypothesis of an absolutely rigid earth retarded by the tidal action of a liquid ocean.

I was led by this result to consider the case of the earth-moon, separating (as I believe they did) from the central solar mass in the form of a swarm of discrete masses of meteoric iron and stone, each one having the temperature of the cold of interstellar space, or not much above it. Translating this conception into mathematical language, I find that the equation of continuity belonging to the hydrodynamical theory applies equally well to the meteoric theory, viz.

$$vy = v'y', \quad \ldots \quad (7)$$

where v, v' are the velocities at any two points, and y, y' are the depths of the ocean or meteoric swarm at the same points.

The depth of the swarm or ocean without jostling or friction will be least under the moon and greatest at right angles to the moon, and the velocities will be inversely. Hence the chances of jostling among the meteorites when disturbed by the moon's tidal action will be proportional to the velocity, being greatest where the velocity is greatest and the area of passage least, and *vice versâ*.

This consideration reduces the meteoric problem to that of the hydrodynamical problem, with a friction proportional to the velocity, and gives equations in all respects similar to those derived by Mr. Darwin from the hypothesis of a viscous earth.

On the meteoric hypothesis, if the jostling of the stones be slow they may cool almost as fast as they are heated, and the result will be a cool earth and almost indefinite time at the disposal of geologists.

XLVIII. *On Systems of Absolute Measures for Electric and Magnetic Quantities.* *By* Prof. H. Helmholtz*.

PHYSICISTS have hitherto been obliged to employ two different systems of electrical absolute measures, the *electrostatic* and *electromagnetic* ; while for *magnetic quantities* only one has always been made use of—namely that introduced by Gauss, in which only the parts of the metre and the gramme employed as the units of length and mass have changed. Indeed the employment of those two systems of electrical measures could not be dispensed with, for practical reasons, because the determination of the factor which had to be used

* Translated from Wiedemann's *Annalen*, 1882. no. 9. vol. xvii. pp. 42–54.

for the reduction of electrostatic to electromagnetic measures, namely Weber's critical velocity, could not yet be effected with the same degree of precision that could be attained within the sphere of electromagnetic measurements on the one hand and electrostatic measurements on the other. It was on this account more advantageous to employ in each experimental investigation that system of measures to which the quantities measured could be referred with the greater exactness.

To this is to be added the consideration of avoiding excessively large numbers, which will probably induce us to continue to employ for electrostatic and galvanic phenomena two kinds of measures, although reducible to one another. At present the electromagnetic methods of measurement are the most perfect; they are unmistakably the most important practically for an art that advances with giant strides; and I have therefore considered that the International Congress that met in Paris last year acted quite suitably in endeavouring to establish an electromagnetic system of absolute measures. Had the aim been purely scientific, I should have preferred the electrostatic system hitherto employed, since this, I think, best represents the essential analogies of the phenomena by analogous formulæ, and gives to them the clearest and most intelligible expression. It was on this system, grounded on Gauss's principles, that most of the physical-mathematical treatises in this department of science have hitherto been based.

Just on this account it would appear to me very undesirable if this system should now entirely fall, and even its name give place to a new one, as proposed by Clausius in his recently published memoir*. I would not at all recommend the multiplication of systems of measures without very urgent reasons; and certainly the transference of a name already in use and frequently employed to a new system would inevitably produce needless and vexatious confusion in physical literature, even apart from any estimate of the relinquished in comparison with the new system.

Any determination of a new absolute measure must be based on the measuring observation of a natural process or behaviour, just as already, among the three fundamental units, the gramme has been reduced to the two others by means of the density of pure water at 4° C. The measure of magnetic quanta which has hitherto been exclusively employed is founded on the definition laid down by Gauss, according to which the repellent force between two magnetic quanta, m_1 and m_2, which are situated at the distance r from one another,

* Clausius, *Verhandl. des naturh. Vereins d. preuss. Rheinl. u. Westfal.* March 6, 1882; Wied. *Ann.* xvi. p. 529: Phil. Mag. June 1882, xiii. p. 381.

is put not merely proportional, but equal to the value of $(m_1 . m_2/r^2)$. Since the force and the length r are to be measured by known methods, the value of the product $(m_1 . m_2)$ is thereby determined in absolute measure; and therefore, if from other facts the ratio (m_1/m_2) can be determined, m_1 and m_2 can each be separately determined.

Exactly the same principle is also applied by Gauss, at the commencement of his memoir "Allgemeine Lehrsätze in Beziehung auf die im verkehrten Verhältnisse des Quadrats der Entfernung wirkenden Anziehungs- und Abstossungskräfte"*, to electrical quanta and gravitating masses. Although he has not in the latter two cases carried the principle into practical effect, it would be justifiable to designate all three methods by his name as that of their mental author. That which refers to electricity gives the *electrostatic system* as it has hitherto been employed. The third, referring to gravitating masses, will probably in future play an important part, when we have succeeded in accomplishing more exact determinations of the force of gravitation. If, like Maxwell, we denote by angular brackets the dimensions of the expression enclosed in them, by M a mass, by L a length, and by T a time, according to Gauss the attraction between two heavy masses m at the distance r is

$$\left[\frac{m^2}{r^2}\right] = \left[\frac{\mathrm{M} . \mathrm{L}}{\mathrm{T} . \mathrm{T}}\right] \quad \text{or} \quad \left[\frac{\mathrm{M}}{\mathrm{L}^3}\right] = \left[\frac{1}{\mathrm{T}^2}\right].$$

On the left stands a density, on the right a function of the time. If, therefore, as hitherto, we put the absolute density of water equal to *unity*, while the unit of mass is determined in gravitation-measure, a time-measure is thereby given which is independent of the probably variable rotation of the earth, and only a single measure, the metre, is left to be handed down by tradition. But even this could be absolutely defined if we availed ourselves of an invariable velocity, for instance the velocity of light in free æther.

Thus, for example, the period of revolution T of a small satellite revolving close to the surface of a sphere of pure water of normal density D, would, independently of the radius of the sphere, in gravitation-measure be

$$\mathrm{T}^2 = \frac{3\pi}{\mathrm{D}},$$

and the velocity of light

$$c = \frac{\mathrm{L}}{\mathrm{T}} = \mathrm{L}\sqrt{\frac{\mathrm{D}}{3\pi}}.$$

* *Resultate aus den Beobachtungen des magnetischen Vereins* 1839.

by which latter equation the length L would be given. This system would therefore free us from the handing-down of any traditional measure.

In Gauss's magnetic and electrostatic measure the dimensions of the magnetic quantum m and the electrostatic quantum e are determined by the equations

$$[m]=[e]=[\mathrm{M}^{\frac{1}{2}}\mathrm{L}^{\frac{3}{2}}\mathrm{T}^{-1}],$$

both based on the phenomenon of repulsion between resting magnetic or resting electric quanta.

On the other hand, for electromagnetic determinations the ponderomotive action of a closed electric current upon a pole of a magnet was used, the laws of which have been mainly and completely formulated by Ampère.

The components of the magnetic forces produced in its vicinity by an electric current can, like those of a magnet, be represented as differential quotients of a potential-function which satisfies the same differential equations as those of magnets, and only differs from those of the latter in that it periodically increases in value by the same quantity as often as only one pole is caused to make a whole revolution about the conductor of the current. As the electromagnetic forces are proportional to the current-intensity of the conductor, the period of the potential is also proportional to that intensity, and independent of the shape of the conductor. Maxwell on this account employs the value of the period of the potential Ω as a measure for the intensity of the current C, and therefore, in § 623 of his Treatise on Electricity and Magnetism, puts the dimensions of the two equal:

$$[\Omega]=[\mathrm{C}].$$

The fixed numerical relation between the two follows from an earlier passage of the above-mentioned work, § 479, where T denotes the magnetic force of a very long straight current-conductor at the small distance r from its axis, and J is put for C:—

$$\mathrm{T}r=2\mathrm{J}.$$

Since

$$\Omega=\mathrm{T}\,.\,2\pi r,$$

$$\Omega=4\pi\mathrm{J},$$

by which Maxwell's determination becomes, when Gauss's magnetic measure is employed, equal to the electromagnetic measure proposed by W. Weber.

In Ampère's time a complete theory of potential-functions did not yet exist. He has, however, represented quite accu-

rately what we can now, in the manner stated, express in conformity with nature, by a suitably chosen fiction; namely, he imagined a surface bounded by the conductor, dividing the in this case doubly connected space covered with a double magnetic layer. If the magnetic moment of each unit of surface of the double layer is denoted by μ, according to well-known principles the leap of potential between the two sides must be

$$\Omega = 4\pi\mu,$$

and therefore $\mu = J$. With this form of expression of Ampère's law Prof. Clausius stops.

Both forms are perfectly equivalent and equally justified, so long as we measure the magnetic quanta according to Gauss's rule. This Prof. Clausius also admits; but he thinks an extension of Maxwell's expression to other systems of measures must be rejected; he explains this as an oversight on Maxwell's part, and the equations and determinations of measurements derived from it as erroneous.

The only reason which, in this respect, he alleges against Maxwell's definition is the following, in § 3 of the memoir above cited:—"The force, however, which a current exerts upon a magnetic pole is *electrodynamic*; and from this it follows that an equation of which the deduction is based upon this force can be regarded as valid only in the dynamic system founded upon the electrodynamic forces, and not in the static system based on the electrostatic forces."

But even if one, as an adherent of Ampère's hypothesis, entertained no doubt respecting the first part of this proposition, I do not see why the conclusion should be urged against Maxwell only, and not against the interpretation of Ampère's law adopted by Clausius himself, since the latter is, after all, nothing but another way of expressing the same facts. Both quantities, Maxwell's potential-period Ω as well as Ampère's magnetic momentum of unit surface, are, in Gauss's system of measurement, of the dimension $[m/\mathrm{L}]$; both have a physical meaning only in "the representation of the force which a current exerts upon a magnetic pole."

The true reason of the difference moreover appears to me to lie in quite another circumstance—namely, in Maxwell's definition of the magnetic potential Ω. This is with him not the form of calculation $\Sigma[m/r]$, but he defines it in this case, as also in the corresponding applications to electrostatics and electrodynamics, by stating that $\Omega \,.\, m$ is a work—which definition also underlies Gauss's definition of m.

The differential quotient $-d\Omega \;dx$ is, corresponding to this, the force which acts upon the unit of magnetism, and therefore

$(-\Omega)$ Jacoby's force-function. If we introduce for m another measuring unit, and measuring with it obtain m_1, Ω_1, J_1 instead of m, Ω, J, then must, according to Maxwell's definition,

$$m\Omega = m_1\Omega_1,$$

and therefore

$$mJ = m_1J_1.$$

The unit of current therefore increases in the inverse proportion of the newly chosen unit of magnetism to the old one; but the force which the unit of current exerts upon the unit of magnetism remains constant. With Prof. Clausius, on the contrary, both m and J increase in the same, and not in the inverse ratio, and the force increases as m^2 or J^2. According to his determinations, the feigned surface of Ampère always produces one more leap, equal to the momentum of unit of surface, in the function $\Sigma(m, r)$; but this function has then no longer the signification of the force-function for the newly chosen magnetic units.

In all this I cannot perceive any mistake of Maxwell's; and his equations, derived from the formulation chosen by him of the fundamental phenomenon, are altogether as consistent with each other and as correct, if understood in the sense of their author, as those of Prof. Clausius. Rather, this case shows that, if we abandon Gauss's determination of the magnetic unit, we again fall into at least two different and equally justifiable systems of measuring-units; and, for my part, I could, with respect to both Maxwell and Clausius, draw from this the practical conclusion that we ought by no means to forsake the above-mentioned method of Gauss until we have particularly important reasons and a definite purpose for such a proceeding, when the choice between the systems of Maxwell and Clausius would probably be decided on positive grounds.

Now, although I must vindicate Maxwell from the charge of having, in consequence of an oversight, set up incorrect equations, yet it should be mentioned that in the wording of the text of § 623 of his work, where he reduces the dimensions of all the rest of the electric and magnetic quantities to the dimensions of any one chosen from among them, an omission occurs which might easily lead the reader into error, if he does not closely examine the connexion of the somewhat complicated systems of equations of §§ 622 and 623, and which seems to give to the propositions of § 623 a greater extension than Maxwell himself would probably have given them. For at the beginning of Chapter X., in § 620, he speaks of electrostatic and electromagnetic but not of magnetic units, and

moreover gives definitions according to which the electrokinetic units are determined without any consideration of magnetism. But in § 621 he introduces magnetic quantities without in any way saying expressly that, in all his determinations of the ratio between electric and magnetic quantities, the electromagnetic determination discussed in the chapters in question,

$$4\pi J = \Omega,$$

will be retained.

In this respect Maxwell's intention in sketching different systems was exactly the same, and as limited, as that more recently carried out by Clausius, although, from the cause above discussed, the manner of carrying it out by the two has turned out different.

The matter being so important, I will take leave to give here a summary of the connexion of the equations of the corresponding paragraphs (621–623) of Maxwell's Treatise on Electricity and Magnetism. In § 621 he arranges the quantities for which the dimensions of the unit are to be determined. I place them here in a somewhat different order, and with the notation of their meaning which is more usual in Germany. There are four electrostatic, namely:—

e, electric quantum;

E, electrostatic potential-function;

$\mathfrak{D}$, dielectric polarization, measured by the electric density at the surface of the dielectric;

$\mathfrak{E}$, the electric force in a point, acting upon the unit of s.

The four corresponding magnetic quantities he denotes, in the same order, by m, Ω, $\mathfrak{B}$, $\mathfrak{H}$.

To these are added four more corresponding quantities, namely:—

C, the intensity of a current;

$\mathfrak{C}$, current-density;

p, the electrokinetic momentum of a current;

$\mathfrak{A}$, the vector-potential of electric currents.

As regards the meaning of the last two quantities, p is Neumann's electrodynamic potential of the other currents present, referred to the entire conductor (passed through by the unit of current) for which it is calculated; and $\mathfrak{A} \cdot ds$ is the same potential referred to a conductor-element ds situated at a determined place.

I remark further that the quantities denoted by German capital letters are vectors, *i. e.* quantities having direction and resolvable into components according to the law of the parallelogram of forces,—and that the selection of them resulted from Maxwell's endeavour to introduce, as far as possible, only

directly observable quantities into the calculation, and to avoid hypotheses.

In § 622 Maxwell sets forth dimension-determinations for the products and quotients of these twelve quantities, as given immediately by their meaning. There are fifteen of these determinations, in which those quantities referable to electricity are kept quite separate from those which refer to magnetism, so that (which is certainly remarkable) from these fifteen determinations no relation between electricity and magnetism appears. I arrange them tabularly, retaining Maxwell's above-employed notation of the dimensions.

No.	Dimension.	Electric quantities.	Magnetic quantities.
1.	L^2M/T^2	$[e\,.\,\mathrm{E}]$, $[p\,.\,\mathrm{C}]$	$[m\,.\,\Omega]$
2.	$M/(LT^2)$	$[\mathfrak{D}\,.\,\mathfrak{E}]$, $[\mathrm{C}\,.\,\mathfrak{A}]$	$[\mathfrak{B}\,.\,\mathfrak{H}]$
3.	T	$[e/\mathrm{C}]$, $[p/\mathrm{E}]$, $[\mathfrak{A}/\mathfrak{E}]$	
4.	L	$[\mathrm{E}\,\mathfrak{E}]$, $[p/\mathfrak{A}]$	$[\Omega\,\mathfrak{H}]$
5.	L^2	$[e/\mathfrak{D}]$, $[\mathrm{C}/\mathfrak{C}]$	$[m/\mathfrak{B}]$

The last series of determinations result from the first, if they be divided by the product of the quantities in question of the second and fourth series. The fifth series may therefore be omitted as superfluous; then there remain the following identical equations between the quantities in the first four rows:—

$$\left[\frac{p}{\mathrm{E}}\right] = \frac{[p\,.\,\mathrm{C}]}{[e\,.\,\mathrm{E}]}\cdot\left[\frac{e}{\mathrm{C}}\right];$$

$$\left[\frac{\mathfrak{A}}{\mathfrak{E}}\right] = \frac{[p\,.\,\mathrm{C}]\cdot\left[\frac{e}{\mathrm{C}}\right]}{[e\,.\,\mathrm{E}]\cdot\left[\frac{p}{\mathfrak{A}}\right]}\cdot\left[\frac{\mathrm{E}}{\mathfrak{E}}\right].$$

Lastly, there are left three independent determinations for the four magnetic quantities m, Ω, $\mathfrak{B}$, $\mathfrak{H}$, and seven for the eight electric quantities e, E, p, C, $\mathfrak{D}$, $\mathfrak{E}$, $\mathfrak{C}$, $\mathfrak{A}$. If, therefore, of these two groups one quantity each be defined by other determinations—for example m and e, the quantum of magnetism and the quantum of electricity—all the others are completely defined, namely:—

	Electric.	Electrokinetic.	Magnetic.
Potentials ...	$[\mathrm{E}]=\left[\frac{\mathrm{L}^2\mathrm{M}}{e\mathrm{T}^2}\right]$	$[p]=\left[\frac{\mathrm{L}^2\mathrm{M}}{\mathrm{CT}^2}\right]$	$[\Omega]=\left[\frac{\mathrm{L}^2\mathrm{M}}{m\mathrm{T}^2}\right]$
Forces	$[\mathfrak{E}]=\left[\frac{\mathrm{LM}}{e\mathrm{T}^2}\right]$	$[\mathfrak{A}]=\left[\frac{\mathrm{LM}}{\mathrm{CT}^2}\right]$	$[\mathfrak{H}]=\left[\frac{\mathrm{LM}}{m\mathrm{T}^2}\right]$
Densities ...	$[\mathfrak{D}]=\left[\frac{e}{\mathrm{L}^2}\right]$	$[\mathfrak{C}]=\left[\frac{\mathrm{C}}{\mathrm{L}^2}\right]$	$[\mathfrak{B}]=\left[\frac{m}{\mathrm{L}^2}\right]$
$[e]=[\mathrm{CT}]$			

These are the determinations which, without any further limiting equations, result from the above fifteen. These can be applied to any definition of the units of m and e, and therefore also to the *electrostatic-magnetic* system of Gauss.

Now follows, in Maxwell, § 623, "These fifteen equations are not independent of each other; and in order to deduce from them the dimensions of all the twelve units they contain, we require one more equation." In fact we require two, since e and m must be determined singly by recurring to two facts of observation regarded as fundamental phenomena. The one here wanting, not expressly mentioned by Maxwell, but from the connexion self-evidently presupposed, we can write as above:—

$$[\Omega]=[\mathrm{C}].$$

Clausius has chosen for it the less perspicuous

$$[p]=[m].$$

But, since one of the fifteen determinations in § 622 reads

$$[m\,.\,\Omega]=[p\,.\,\mathrm{C}],$$

each of the two follows from the other.

Just on this account, however, the closing words of § 623, "All the above-given determinations are correct for any system of units we may choose," must be altered, and limited to electromagnetic systems, and, indeed, to such only as are derived from the meaning, as defined by Maxwell, of the fundamental law of electromagnetism. For that concluding sentence applies neither to the electrostatic system nor to the system set up by Clausius. Of the possibility that another conception of the electromagnetic fundamental law might here lead to other consequences Maxwell probably did not think; and in this respect Clausius has indeed, in his most recent memoir, given a thankworthy enrichment of our ideas.

Finally, we must speak of the reason why Prof. Clausius is

willing to drop the electrostatic system, constructed according to Gauss's principles, hitherto employed. The only thing he says on this subject is contained in § 1 of his memoir. After mentioning that the forces exerted upon each other by closed electric currents may be regarded as indubitably known, he continues:—"As, further, the small electric currents which according to Ampère are to be assumed as existing in the interior of a magnet are likewise closed, we have in magnetism to do with forces of the same kind." Thereupon follows an analysis, according to which the forces of two magnetic quanta are to be regarded as dynamic.

This reason, however, would be decisive only if it were certainly proved that Ampère's hypothesis corresponds to the reality, while up to the present it can hardly be said that it has been clearly and consistently worked out for all sorts of diamagnetic and ferromagnetic bodies. In particular, this hypothesis would also require that the hypothetic molecular currents of magnetized bodies should show the changes which are to be generated by electrodynamic induction, as they are in fact logically assumed to do by W. Weber in his well-known explanation of diamagnetic polarization. How this can be reconciled with the properties of ferromagnetic bodies I leave the adherents of this theory to explain. Meanwhile, however interesting this theory may be, we may look upon it as neither verified nor even completely worked out.

Among the electrodynamic theories which assume direct action at a distance, its quantity and direction depending on the absolute or relative motions of two electric quanta, stands that of Faraday and Maxwell. It has at least this superiority to those, that it does not violate either the principle of the finiteness and constancy of energy or that of the equality of action and reaction; and moreover it bases a theory of light, free from many difficulties of the hitherto received undulation theory, upon the identical hypotheses which form the groundwork of electrodynamics. In order to discover the essential character of the forces of electricity and magnetism, Maxwell excludes those processes in which, according to the sort of friction, heat is generated and electric or magnetic energy lost, and founds his theory upon conservative processes only. In particular, he excludes the conduction of electricity in conductors, and the coercive force in magnets. Then, however, his fundamental equations present the most complete analogy, not between magnets and moved electricity, but between resting dielectric and resting magnetic polarization. It is precisely to this analogy that Gauss's electrostatic system of measures perfectly accommodates itself.

I will make one more remark, that when one seeks to form, after the analogy of Hamilton's principle, that integral, taken with respect to the time, whose variation gives the equations of motion according to Maxwell in place of the *electrodynamic potential* of Clausius (Maxwell's *electrokinetic energy*), a bilinear function of the components of the electric flow, on the one hand, and of the components of the magnetic momenta, on the other, arises in which the latter have to be dealt with, but *not* as velocities. This last point I reserve to myself to treat soon in another place.

For the present I need only remind physicists that the ground on which Prof. Clausius is inclined to reject the hitherto accepted electrostatic system is a hypothetical assumption, contested between different theories; and I would beg them not to transfer the name of the *electrostatic* system proceeding from Gauss, and hitherto employed by preference in mathematical works, to another. In this system the potentials (m^2/r) and (e^2/r) are quantities of work; their entire physical importance rests upon the fact that they are such. The theory of the potential-functions forms one of the most complex and interesting chapters of mathematical physics, corresponding to well and perfectly known physical processes. If Gauss's units be changed, then must we accustom ourselves to add factors to all potential-functions, in order that they may remain quantities of work and their differential quotients give the forces. On the contrary, whether J^2 is a force and mJ a work, or whether we must write for them A^2J^2 and AmJ, appears to me much less important, especially as we know well and accurately just a portion of the department of electromagnetic actions, viz. that consisting of closed currents, but in the province of unclosed currents the most luxuriant flora of hypotheses still flourishes.

XLIX. *The Thomson Effect.* *By* John Trowbridge *and* Charles Bingham Penrose*.

SIR WILLIAM THOMSON† first discovered that when an electrical current passes through a piece of metal, the ends of which are of different temperatures, it carries heat with it; the direction depending upon the character of the metal and the direction of the current. This phenomenon is known as the Thomson Effect. Le Roux‡ subsequently verified Thomson's results, and gave an incomplete table of the effect in different metals. No especial pains have

* From Silliman's American Journal of Science for November 1882.

† Phil. Trans. 1856, vol. iii. p. 661.

‡ *Ann. de Chim. et de Phys.* 1867, [4] vol. x. p. 258.

been taken hitherto in experimenting with pure metals. We have thought it would be valuable to test the effect in as pure a metal as we could obtain by electrolysis. We have also extended Le Roux's table by the addition of the effect in nickel, which Thomson was unable to obtain, and also in carbon. An endeavour has been made to ascertain if the effect is reversible, and also to discover if it is modified in a magnetic field.

The strip of nickel, 45 centim. long, 2·6 wide, and 2 millim. in thickness, was placed with its flat surface horizontal. One face of a thermopile was placed at a fixed point on the surface of the nickel, separated from it by a thin piece of mica. A weight pressed upon the other surface. The thermopile was connected with a Thomson's reflecting galvanometer of six ohms resistance. The two extremities of the strip of nickel were connected with a battery of six Grove cells, the wires first passing through a key so that the direction of the current could be reversed. One end of the nickel was kept at the temperature of the air, 15° C.; the other at a constant red heat by means of a Bunsen burner. The metal was heated in this way from 9 A.M. to 3 P.M., until it reached a condition of thermal equilibrium, as shown by the galvanometer. The scale of the galvanometer was then moved until the spot of light came to 0. The current from the Grove cells was then passed for one minute alternately in opposite directions, and the deflections of the galvanometer were read every quarter of a minute. Before the direction of the current was changed, the circuit was each time broken, and the spot of light was allowed to fall to 0. The following table gives the results. The column marked "C–H" gives the deflections when the current was passing from cold to hot. The small numbers show which deflections in each pair were taken first.

C–H. Deflections taken every ¼ minute.					H–C. Deflections taken every ¼ minute.				
1	2	1	2	1	2	1	2	1	2
4·1	4·2	4·0	4·3	4·4	3·3	4·0	3·6	3·8	4·1
6·3	6·4	6·5	6·4	6·4	5·0	6·2	5·4	5·9	6·0
7·3	7·2	7·5	7·0	7·2	5·8	6·7	6·2	6·5	7·0
7·4	7·6	7·6	7·3	7·7	6·1	7·3	6·5	6·8	7·2

From this table it is obvious that more heat is evolved by a constant current per unit time in passing from the cold to the hot end of the nickel than in passing in the opposite direction. The Thomson Effect in pure nickel is consequently negative;

i. e. heat is absorbed by a current in passing from hot to cold, and evolved in passing from cold to hot. The above results were confirmed by many similar experiments, as will be seen later.

It was next determined to find whether the Thomson Effect was reversible—that is, whether the heat absorbed by a current in passing across a section of temperature t was equal to the heat evolved by the same current when passing in the opposite direction across the same section. This subject has important bearings on the thermodynamical theory of thermoelectricity.

The following method was pursued:—Both ends of the nickel were at the temperature of the air, 15° C. The current from six Grove cells was passed as before, and the deflections of the galvanometer were observed every half-minute. The apparatus was arranged exactly as before. Column I. of the accompanying table gives the deflections. One end of the nickel was now placed in melting ice. After one hour it reached a condition of thermal equilibrium, and the current from the Grove cells was passed alternately in opposite directions. The deflections are given in II. and III.

If the deflections in II. and III. are subtracted from the corresponding deflections in I., we get the amount of deflection due to the Thomson Effect. It will be observed that all the deflections in II. are less than those in I., and those in III. are greater, as they obviously should be. The only inaccuracy in this determination is due to the fact that we neglected the alteration in electrical resistance of the nickel due to the slight change in temperature.

I. Deflections taken every ½ minute.	II. H–C. Deflections every ½ minute.	III C–H. Deflections every ½ minute.	I.–II.	I.–III.
1·8	1·8	2·0	0·0	−0·2
2·6	2·4	2·8	0·2	−0·2
2·9	2·65	3·15	0·25	−0·25
3·1	2·75	3·25	0·35	−0·15

The numbers in these tables are obviously too small to draw any conclusions. They, however, confirm the preceding results as to the direction of the Thomson Effect; and tend rather to prove than disprove the reversibility of the effect. The experiment was repeated several times, but with no better result.

Experiments were also made to test the influence of mag-

netism on the Thomson Effect. Nothing but negative results, however, were obtained.

The strip of nickel was placed horizontally, with its flat surface perpendicular to the axis of a large electromagnet, the strip being between the two poles of the magnet. One surface of the nickel was pressed against one pole; on the other surface was placed one face of the thermopile, while the opposite face was in contact with the second pole of the magnet. Mica was used, as in the previous experiments, to protect the faces of the pile. The whole was wedged and pressed tightly together, and clamped by means of wire, the object being to prevent any motion of the nickel when the magnet was made. One end of the nickel was heated by a Bunsen burner; the other was at the temperature of the air. Six hours were required for the apparatus to reach a condition of thermal equilibrium. The electromagnet was connected with thirty-eight freshly set-up bichromate-of-potash cells, with plates of large size. A current from eight Grove cells was now passed along the nickel, with and without the circuit of the magnet being made. The deflections of the galvanometer were exactly the same in each case, showing that in a magnetic field (at least of the strength in the experiment) the Thomson Effect was unaltered.

It is unfortunate that the strength of the field could not be accurately obtained, as the batteries had been running about thirty minutes by the time the experiment was completed. The field, however, was very much stronger (as shown by rough tests) than in another experiment, where the minimum value was found to be 184 times the vertical intensity of the earth's magnetism.

The determination of the relative value of the Thomson Effect in nickel by the following method gives of course but approximate results. The value, however, is probably as accurate as those given by Le Roux for other metals.

A strip of copper, of about the same dimensions as the nickel used before, was arranged exactly as the nickel had been. The thermopile was insulated from the strip by the same piece of mica, and the same weights were placed on the upper surface. One end of the copper was heated in boiling water; and when the apparatus had reached a condition of equilibrium, the deflection of the galvanometer was 35 centim. A current from four amalgamated Grove cells was now passed alternately in opposite directions along the bar, the deflections of the spot being taken, in each case, after one minute. The results are given in the left-hand table :—

C–H. Deflections taken every minute.	H–C. Deflections taken every minute.	Differences between the corresponding deflections.	C–H. Deflections taken every minute.	H–C. Deflections taken every minute.	Differences between the corresponding deflections.
12·0 [2]	13·0 [1]	1·0	14·5 [1]	13·3 [2]	1·2
12·3 [1]	13·1 [2]	0·8	14·3 [2]	13·3 [1]	1·0
12·1 [1]	13·4 [2]	1·3	14·3 [1]	13·4 [2]	·9
12·0 [2]	12·8 [1]	0·8	14·8 [2]	13·3 [1]	1·5
Mean difference = 0·97.			Mean difference = 1·15.		

The strip of nickel was now substituted for the copper, every thing else remaining exactly the same. One end of the nickel was heated, and the thermopile was placed on such a spot that the galvanometer gave a deflection of 35 centim.

The same current was passed as above. The results are given in the right-hand table. Let d = the mean difference in the first table, and d' = that in the second, d and d' are then proportional to the elevation of temperature of the part of the bars under the pile on account of the Thomson Effect. Let σ = coefficient of Thomson Effect; that is, σ is such a quantity that $\sigma d\theta$ represents the heat absorbed per unit current per unit time in passing from section at temperature θ to section at temperature $\theta+d\theta$. The heat evolved in unit section when the temperature is increased by $\frac{1}{2}d$. K is $\frac{1}{2}$KdSD, where K is a constant depending on the galvanometer, S is the specific heat, and D the density of the metal. If we consider the Thomson Effect to be constant under the pile, and θ and θ' to represent the temperature of the ends of the space covered by the pile, we have :—

$$\sigma(\theta-\theta')=\mathrm{K}\frac{d}{2}\mathrm{SD};$$

and the similar expression for nickel,

$$\sigma'(\theta-\theta')=\mathrm{K}\frac{d'}{2}\mathrm{S'D'}.$$

$$\therefore\ \frac{\sigma'}{\sigma}=\frac{d'}{d}\cdot\frac{\mathrm{S'}}{\mathrm{S}}\cdot\frac{\mathrm{D'}}{\mathrm{D}}\cdot\quad\ldots\quad\ldots\quad(\mathrm{I.})$$

Equation (I.) then gives the relative value of the coefficient of the Thomson Effect at any temperature θ.

$$\mathrm{S}=\cdot095,\ \mathrm{S'}=\cdot108,\ \mathrm{D}=8\cdot9,\ \mathrm{D'}=8\cdot3,\ d=0\cdot97,\ d'=1\cdot15$$

$$\therefore\ \frac{\sigma'}{\sigma}=1\cdot25;\quad\therefore\ \sigma'=1\cdot25\sigma.$$

In Le Roux's table $\sigma=2$; $\therefore\ \sigma'=2{\cdot}50$: σ and σ', however, are of opposite sign. Introducing nickel, Le Roux's table becomes:—

+		−	
Sb	64	Fe	31
Cd	31	Bi	31
Zn	11	Arg	25
Ag	6	Pt	18
Cu	2	Ni	2·25
		Al	0·1
		Sn	0·1

The Thomson Effect in carbon was next investigated. The carbon used was the graphite of the common carpenter's lead-pencil. The pencils which gave the best results were Faber's.

Attempts were first made to measure the direction of the Thomson Effect in the same way as in the case of nickel—that is, by placing a face of the thermopile on one surface of the carbon, the two ends of the carbon being maintained at constant temperatures, and passing the electric current alternately in opposite directions. This method was unsuccessful from the fact that one Grove cell heated the carbon to such a degree that in one minute the spot of light was thrown off the galvanometer-scale, thus rendering it impossible to measure, with any accuracy, the rate at which the deflection increased.

The method of Le Roux was then tried, of using two strips of carbon, each face of the pile being in contact with one strip. This method not only doubles the deflection due to the Thomson Effect, but also greatly diminishes the deflection due to the heat evolved on account of the electrical resistance of the carbon. If the two strips of carbon were exactly the same in all their physical properties, and the contacts with the faces of the thermopile were the same on each side, the latter deflections would evidently be entirely eliminated.

Two carpenter's pencils were split longitudinally, the lead being left in one half of the wood. They were then tightly bound, parallel, against each face of the thermopile, and insulated from it by thin pieces of mica. Especial care was taken to fasten the carbons firmly, so as to prevent any motion from the passage of the current. The pencils were placed perpendicularly, the lower ends in two vessels of mercury, surrounded by melting ice; the upper ends were at the temperature of the air. The upper ends were electrically connected; and the wires from a battery of three Grove cells were placed in the vessels of mercury. The thermopile was connected with a reflecting galvanometer of six ohms resistance.

When the system had reached a condition of thermal equilibrium, the current from the battery was passed, and the observations were made. The vessels of mercury and the corresponding pencils are denoted by "*a*" and "*b*." The current entered alternately in "*a*" and "*b*," the deflections of the galvanometer being taken, in each case, every half minute. The deflections showed that the pencil "*a*" was warmer than "*b*;" but the difference of temperatures was greater in one case than in the other.

The following table represents the results of two sets of experiments. The small numbers at top show which column of each pair was taken first.

First experiment.			Second experiment.		
Current enters at "*a*."	Current enters at "*b*."	Difference, proportional to 47 E.	Current enters at "*a*."	Current enters at "*b*."	Difference, proportional to 47 E.
2	1		1	2	
21·0	20·8	0·2	20·8	20·2	0·6
34·5	32·4	2·1	34·7	32·8	1·9
2	1		1	2	
21·2	21·0	0·2	19·5	18·2	1·3
34·5	33·0	1·5	31·0	29·3	1·7
			37·0	34·2	2·8
2	1		1	2	
21·4	20·6	0·8	19·8	18·0	1·8
34·3	32·8	1·5	31·8	29·2	2·6
			37·3	34·0	3·3
2	1		1	2	
21·7	21·6	0·1	20·0	18·8	1·2
36·0	34·3	1·7	32·6	30·4	2·2
42·4	40·0	2·4	38·7	35·7	3·0
2	1		1	2	
23·0	21·7	1·3	21·0	19·8	1·2
38·2	35·0	3·2	33·8	31·3	2·5
45·5	41·2	4·3	39·8	36·3	3·5
2	1		1	2	
23·5	23·0	0·5	20·0	19·0	1·0
39·0	37·0	2·0	32·2	29·8	2·4
45·8	43·8	2·0	37·4	34·3	3·1

From this table it appears that the Thomson Effect in ordinary graphite is negative; that is, heat is apparently evolved when the current passes from cold to hot, or the negative current carries heat with it. The difference in the last columns are obviously proportional to four times the Thomson Effect,

assuming that the effect is reversible. It also appears from the table that the effect increases as the temperature increases, which is in accordance with Tait's assumption.

These experiments were repeated with the graphite from other kinds of pencils; but in no case was the effect nearly as marked as in Faber's. Even in the case of Faber's pencils many trials were made before satisfactory results were obtained.

Equations representing the thermal condition of a bar when acting as a conductor of heat and electricity may be deduced as follows:—One end of the bar is supposed to be maintained at a constant temperature, the other at that of the air; and the electric current is supposed to be constant. For simplicity, we will assume that the specific electrical resistance of the bar is constant throughout, *i. e.* is independent of slight differences of temperature.

The quantity of heat, H, evolved by the current in time δt, in the section of the bar $S\delta x$ (S being the area of a section), is represented by

$$H = I^2RS\delta x \,.\, \delta t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

x = distance of the section from heated end. If we assume that the thermal conductivity is unaltered by the slight rise in temperature due to the current, it can easily be seen that the flow of heat due to conduction is unaltered by the current. Hence we can consider that the heat evolved by the current is partly used in raising the temperature of the section $S\delta x$, and that all the rest escapes from the surface by radiation.

The Thomson Effect is at present purposely neglected.

The bar is supposed to have reached a permanent condition as regards conduction before the current was passed. Let θ be the temperature of the section of the bar we are considering before the current passes; let h = the exterior conductivity or velocity of cooling; let p = the rise of temperature above θ when the current passes. Assuming Newton's law of cooling, the heat radiated on account of the rise of temperature p is proportional to ph; and the quantity radiated from the section in time δt from the same cause is

$$H_1 = phl\delta x \,.\, \delta t, \quad . \quad . \quad . \quad . \quad . \quad . \quad (II.)$$

l = the periphery of the bar.

In time δt the increase of temperature p becomes $p + \delta p$; and the heat developed in the section by this increment is

$$H_2 = CSD\delta x \,.\, \delta p. \quad . \quad . \quad . \quad . \quad (III.)$$

As we saw that the heat of the current was expended only

in the ways represented by (II.) and (III.), we have

$$H = H_1 + H_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (IV.)$$

If we now consider the influence of the Thomson Effect, we simply add that a certain quantity of heat is absorbed or evolved by the current in the section $S\delta x$, distinct from that represented by I^2R.

If σ = the coefficient of the Thomson Effect, the heat absorbed or evolved due to this effect is, in time δt,

$$H_3 = I\sigma\delta\theta \,.\, \delta t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (V.)$$

The effect being proportional to the current, and σ being defined as such a quantity that $\sigma\delta\theta$ represents the heat absorbed or evolved in passing from a point at temperature θ to $\theta + \delta\theta$, per unit current per unit time, introducing this effect in (IV.),

$$H = H_1 + H_2 + H_3, \quad . \quad . \quad . \quad . \quad . \quad . \quad (VI.)$$

as the total value of the excess of heat (due to the current) in the section can be considered as made up of these quantities. Substituting the values in (VI.) from (I.), (II.), (III.), (V.), and transposing,

$$phl\delta x \,.\, \delta t = I^2RS\delta x \,.\, \delta t - CSD\delta x \,.\, \delta p - I\sigma\delta\theta \,.\, \delta t\,;$$

$$\therefore\ phl = I^2RS - CSD\frac{\delta p}{\delta t} - I\sigma\frac{\delta\theta}{\delta x},$$

or, at the limit,

$$\frac{dp}{dt} = \frac{1}{CSD}\left[I^2RS - phl - I\sigma\frac{d\theta}{dx}\right]. \quad . \quad (VII.)$$

This equation gives the rate at which the temperature rises when the current passes, and will approximately apply to the preceding experiments.

When the temperature of the bar becomes permanent,

$$\frac{dp}{dt} = 0,$$

and (VII.) becomes

$$I^2RS - phl = I\sigma\frac{d\theta}{dx} = 0;$$

$$\therefore\ p = \left[I^2RS - I\sigma\frac{d\theta}{dx}\right]\frac{1}{hl}, \quad . \quad . \quad . \quad (VIII.)$$

giving the excess of temperature due to the current in the permanent condition of the bar.

The values in (VIII.) are all easily determined except σ and h. The differential coefficient $\frac{d\theta}{dx}$ (the rate of change of

temperature due to conduction along the bar) can readily be found by experiment; or deduced by analysis, as in the case of an infinite square bar, where

$$\theta = a\mathfrak{S}^{-kx} \text{ and } \frac{d\theta}{dx} = -ak\mathfrak{S}^{-kx}.$$

As p may easily be determined by experiment, the equation can be used to determine σ, as

$$\sigma = \frac{\mathrm{I}^2\mathrm{RS} - phl}{\mathrm{I}\dfrac{d\theta}{dx}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(IX.)}$$

If Tait's assumption that $\sigma = \mathrm{MT}$ (where M is some constant and T the absolute temperature) is true, we might obtain two values of σ for two points of the bar, the temperature of which was known, eliminate h from the two equations, and thus obtain a value for M. If we performed the same operation for two other points, we should get another value for M, and could verify Tait's assumption if this value was equal to the preceding.

The sources of error in the preceding investigation are due to assuming Newton's law of cooling, to neglecting the change of electrical resistance due to a change of temperature, and to partly neglecting the change of thermal conductivity due to the same cause.

L. *On the Reflection of Electrical Rays.*

By Dr. E. Goldstein *.

[Plate VII. figs. 1-8.]

IT has been usually assumed† that the (rectilinear) electrical rays radiating from the kathode of the discharge of an induction-coil terminate where they impinge upon a solid wall, and that beyond the point in which they cut the wall they cannot propagate themselves in any direction ‡. The experi-

* *Monatsber. der Königl. Akademie der Wissenschaften zu Berlin.* Translated from a separate impression communicated by the Author.

† Hittorf, Pogg. *Ann.* cxxxvi.

‡ Herr J. Puluj (*Wien. Ber.* 1880, [2] p. 886) is the only physicist who has assumed a limited power of reflection of the kathode-rays, under the assumption that the kathode-light consists of scattered particles of the electrode, since "it is not intelligible why these should in general suffer no reflection at the wall." The conditions of an experiment made by Herr Puluj to examine whether reflection takes place were not, in my opinion, such that any possible reflection would have been recognizable. That which Herr Puluj considers phosphorescence produced by reflected rays is partly phosphorescence produced by the positive light of the so-

ments which have led me to reject this assumption were suggested by an observation made by Prof. E. Wiedemann*.

Prof. Wiedemann, in using a tube of the form of fig. 1 (Plate VII.), where the disk k at right angles to the axis of the vessel C forms the kathode, not only observed green phosphorescence such as produced by the kathode-rays on the sides of the tube up to the point x which could be reached by straight lines from k, but saw also a feeble illumination of the tube r beyond the bend, and a brighter phosphorescent surface F on the wall C opposite the mouth of the tube r. The motions of the small surface F under the influence of the magnet showed that it was produced directly by electrical rays, and not simply by optical rays possibly reflected at the glass.

Prof. Wiedemann is disposed to explain the appearance as one of the phenomena of deflection discovered by me†, assuming that the glass wall at x becomes charged and acts as a weak kathode, causing the deviation of the pencil of rays passing by it out of the direction at right angles to the plane of k, into the direction xF.

This explanation seemed improbable to me for two reasons:—

(1) Because the surface F is always much more feebly illuminated than would have been the case if the phosphorescence had been exerted by the direct kathode-rays penetrating to C. In order to make the comparison, the kathode-rays may be so curved by the influence of a weak magnet as to pass the bend x. The comparison may be more certainly made without the use of a magnet in a vessel of the form shown in fig. 2, where two paths are offered to the kathode-rays—on the one side the path as in fig. 1 through the bent tube $r r_1$, and on the other side the path through the equally long straight tube r_2 at right angles to k.

(2) Because the surface F totally disappears if the tube $r r_1$ has a second bend in it, in whatever direction this second bend is made. This would not happen with the phenomena of deflection, which I have examined, where a ray may be bent any number of times.

If now the rays which produce the surface F are not direct rays from the kathode k, then we may suppose either (a) that the portions of the tube about x, upon which the rays from the

called reflex currents in the system of tubes, partly phosphorescence produced by direct kathode-rays, which Herr Puluj unintentionally produced by touching the glass with the finger in order to concentrate the light.

* E. Wiedemann, Wied. *Ann.* xi. p. 236; Phil. Mag. [5] x.

† Goldstein, *Monatsber. d. Königl. Akad. der Wiss.* 1876, p. 285; Phil. Mag. 5 iv. Also 'A new Form of Electrical Repulsion' (Berlin, 1880).

kathode impinge directly, become charged with negative electricity, which reaches such a tension that they themselves form a second kathode and radiate electric rays, which then produce F; or (*b*) that the rays which produce F are rays from the kathode *k*, which suffer reflection when they fall upon the solid wall. In this reflection, either the power of producing phosphorescence of the rays becomes weakened or their density, thus explaining the small intensity of light emitted by F.

The hypothesis (*a*) may be excluded, as shown further on; for the phenomenon in question is not altered if the surface upon which the kathode-rays impinge directly be metallic, and if this metal surface be made the anode of the discharge.

If in accordance with this assumption we suppose that reflection takes place, then again reflection according to the optical law is at once to be excluded, since the position and form of the surface F remain unchanged even when the angle of the bend at *x* varies from 25° to 80°. Consequently the law of the equality of the angles of incidence and reflection, or the rule that when the reflecting surface is rotated while the incident ray preserves the same direction, the reflected ray rotates through twice the angle, is not obeyed.

We may, however, easily make numerous experiments which agree in showing the presence of diffused reflection, in consequence of which each point of the wall on which the rays impinge directly diffuses rays in all directions.

If diffuse reflection is proved, we have at once the explanation of the small luminosity of F in comparison with portions of the tube reached by the direct rays in the diminution in density of the incident pencil of rays.

In the next place, if we employ a vessel such as fig. 3, we obtain phenomena corresponding to the surface F in the cylinders C_1 and C_2 at the same time. The rays which travel as far as *x* nearly parallel to each other, therefore, after reflection, follow at least two directions at right angles to each other.

The following experiment forms an *experimentum crucis*:—A chamber B was introduced between the portion of the tube containing the kathode and the bend at *x*, which contained a paper diaphragm which could be turned round the axis *a*, and which had a slit cut in it about 1 millim. broad and parallel to *a*. If the plane of the diaphragm falls along the axis *r r*, its edge intercepts no perceptible portion of the pencil of rays which reaches the tube *r* from the kathode, and which is about 7 millim. across. But if, on the other hand, D is at right angles to the axis of *r*, then only the portion of this pencil which passes through the narrow slit can reach the bend *x*

If the reflection were similar to that of a mirror, the form and magnitude of the surface F would change perceptibly. This surface, in all the experiments so far described, and in this one also, resembles an ellipse of small excentricity, where D has the position first mentioned. Its smaller axis, which falls in the plane of $r\,r_1$, is about twice as large as the diameter of r_1 in the vessel C, which is about 3 centim. in diameter. If now we mark the position, form, and magnitude of F on the outside of the tube C when the diaphragm presents its edge only to the kathode-rays, and then place the diaphragm at right angles to the rays, we find that *the position, form, and magnitude of* F *remain unchanged*; only its *luminosity* is now considerably diminished.

If, instead of the diaphragm with a slit, a plate without openings is introduced into the chamber B capable of free motion, so as to cut off at pleasure either the upper or lower half (and also the right-hand or the left-hand half of the kathode-pencil) by covering the corresponding portion of the mouth of r, then also the position, form, and magnitude of the surface F remain unchanged; the luminosity only of the whole surface decreases, but most in the half which is opposite to the half that has been intercepted. Thus, for example, the surface is darkest in the upper half when it is the lower half of the kathode-pencil which is intercepted.

We easily see how these observations, inconsistent with optical reflection, entirely agree with the assumption of a diffuse reflection of the kathode-rays.

Tubes of the form of fig. 5 are better adapted for the further study of this diffuse reflection than the vessels employed by Wiedemann. The rays emitted by the kathode k which pass through the connecting tube r into the wider cylinder Z fall then upon the plate P, which is fastened to a wire d insulated with glass inside Z. The cylinder is closed air-tight by the caoutchouc stopper k, by removing which the plate P can be exchanged for another; or other changes in the apparatus can be made.

If the plate P consists of phosphorescent glass, then the rays which fall upon it directly produce at the plate s simply an oval very bright green phosphorescent surface. We see, however, distinctly how the diffuse reflection from this surface causes the whole wall of the tube Z lying above the plane of P up to the stopper k to phosphoresce with subdued green light, which is weaker the further the portion of the wall is removed from s.

If the plate P be covered with chalk, its surface at s shines with orange-red light, but the wall of Z presents a green

luminosity as before—a proof that this luminosity does not depend upon optical reflection. So also the phosphorescence produced by diffuse reflection remains unaffected if P be constructed of some material which does not phosphoresce at all. It is further a matter of indifference whether P is metallic or consists of an insulator. In the former case P may even be made the anode of the discharge, without the reflection of the rays appearing in any way weakened.

The kathode-rays are therefore not absorbed by the anode, even when they play directly upon the surface of the anode. Further we see, as already mentioned above, that the phenomenon in question cannot be explained by supposing that the surface struck by the direct kathode-rays is itself converted into a kathode.

If we bring small objects between the plate P and the phosphorescent surface of Z, such, for example, as the wire D (fig. 6), whose distance from P can be varied by rotation round the axis D, we can very well recognize the character of the diffuse reflection which the place s causes in the rays which fall directly upon it. For the shadow of the wire D only appears narrow and sharp when the wire is brought close to the wall of the vessel; if D is moved from the wall towards P, its shadow soon becomes bread and indistinct. If we cut off a further portion of the kathode-rays, by means of a small movable plate of mica introduced into Z at the mouth of r, the space s, directly impinged upon by the rays, of course becomes smaller. The further this decrease proceeds the narrower and sharper does the shadow of D become, exactly as we should expect on the theory of diffuse reflection.

I will here cite only one other consequence of this theory which has been experimentally verified. I may take it as known that a pencil of rays emitted by a plane kathode after it has passed, as in fig. 7, through the aperture (supposed circular) of a diaphragm occupying the whole area of the tube, gives on the flat wall W a well-defined circular luminous figure on a dark ground.

Upon our assumption of the diffusion of the kathode-rays, this ought not to happen any more if the rays are made to pass through a cylindrical tube open at both ends (fig. 8), in place of the thin diaphragm. For since the different rays of a plane kathode are not altogether parallel to each other, but also diverge somewhat around the central portion of the kathode, a part of them must play upon the wall of the tube r, and be then diffusely reflected. The portion of the diffuse rays which reach C must then form an extended luminous space round the bright surface resulting from the direct rays. We find

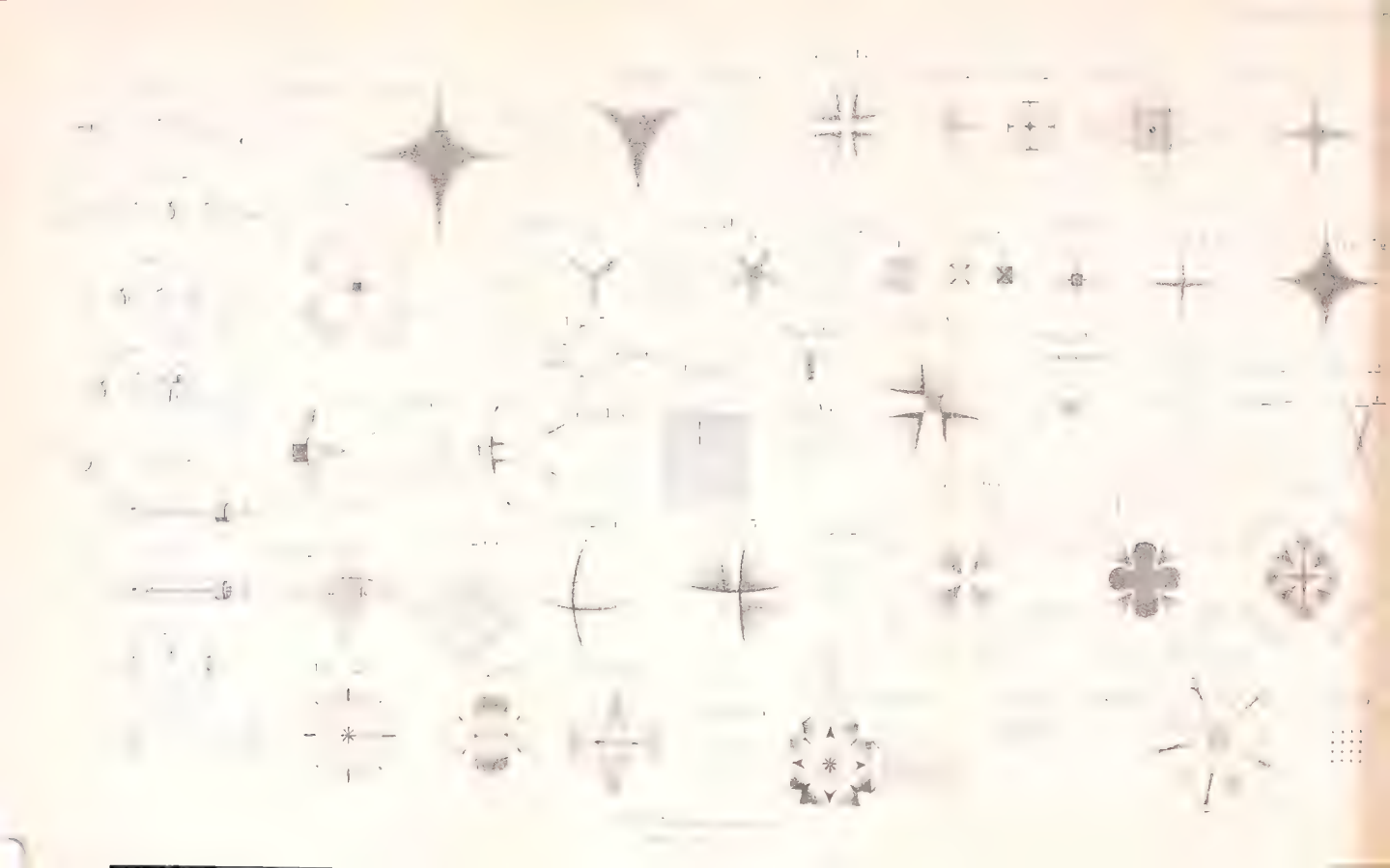
this confirmed by experiment; and if we bring into C a wire δ throwing a shadow, we find that its shadow is sharp and narrow in the region illuminated by the direct rays, but broader and ill defined in the surrounding region. This takes place also when δ is made the anode. We see from this that the difference in breadth of shadow does not depend upon a stronger deflection which δ, apparently neutral but really acting as a weak kathode, causes in the reflected rays*. These last are indeed themselves capable of deflection, as we see if, instead of making δ an anode, we connect it with the earth, or give to it a small portion of the current from the kathode.

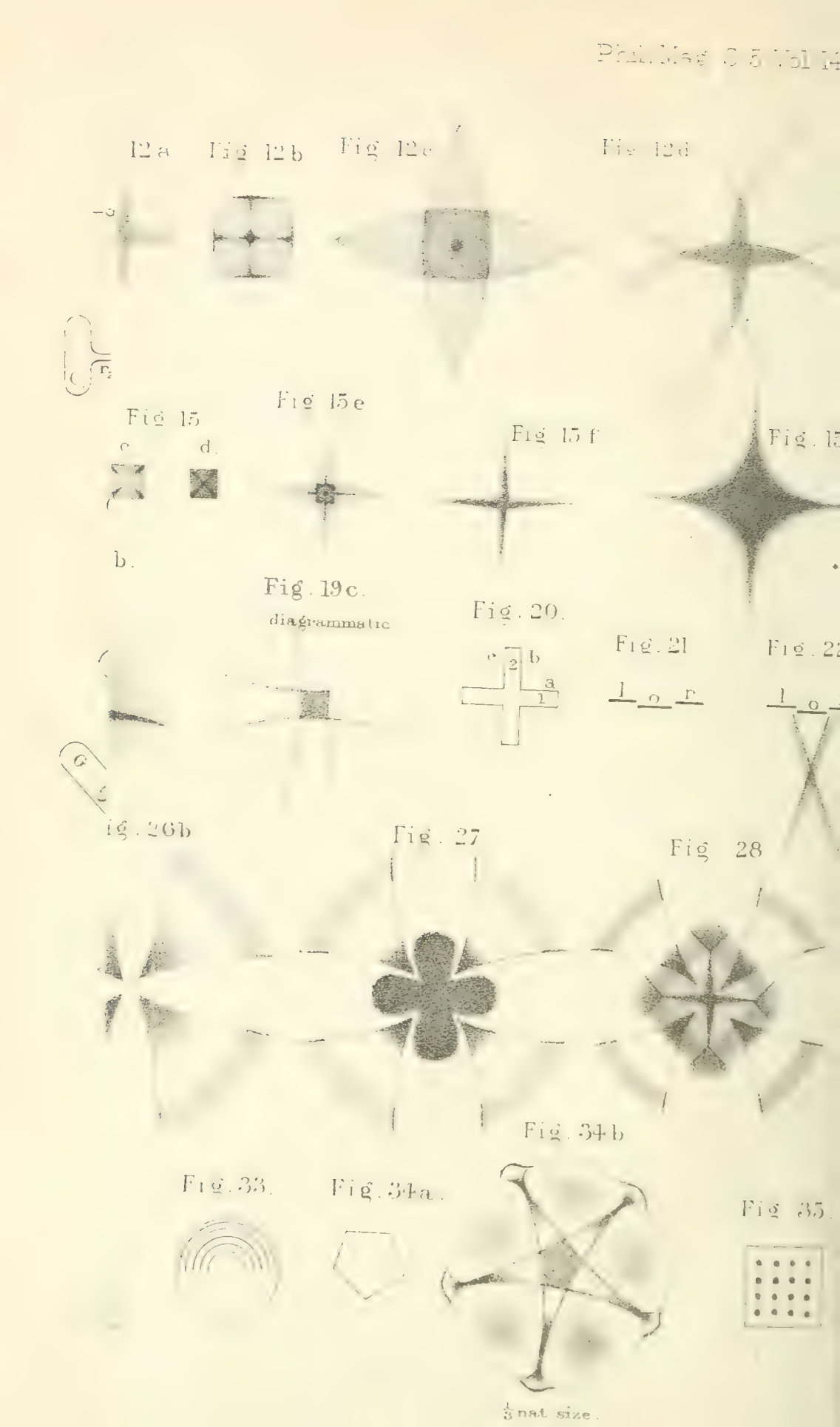
The motions of the shadow of δ under the influence of a magnet, and with other arrangements the motions of the surface F under similar influence, show that the reflected rays are deflected, as far as one can observe, in the same way as the direct kathode-rays would be if their course were the same as that of the reflected rays.

If r be placed equatorially above a horseshoe magnet of suitable strength, the direct kathode-rays before reaching C are compressed against the upper or under wall of r, and the phosphorescent surface (ϕ) produced by the direct rays disappears; but the feebly illuminated region remains, occupying now the position of the surface ϕ: this corresponds exactly to Wiedemann's surface F; its production here is due to the diffuse rays which issue from the portions of the straight tube r struck by the magnetized rays. The further the terminal point of the direct rays is forced towards C by the action of the magnet, the less luminous does the surface F become, since then a continually smaller portion of r is able to reflect rays.

On the whole, the foregoing series of experiments leads to the following result, which I propose to describe more fully in a further communication:—A pencil of kathode-rays does not end (at any rate under the conditions suitable for producing phosphorescence) where it strikes upon a solid wall, but electric rays radiate in all directions through the space occupied by the gas from each point of the wall struck by the direct rays. These rays may be called reflected rays. Any solid wall, no matter what its properties are, may serve as reflecting surface. It is a matter of indifference whether it is capable of becoming phosphorescent or not, whether it consists of a conductor or of an insulator. The reflection is diffuse, equally whether the reflecting surface is dead or as smoothly polished as possible. An anode apparently reflects the kathode-rays, the same as a neutral conducting surface or an insulator. The reflected rays, like the direct kathode-rays, have the property

* 'A new Form of Electrical Repulsion,' p. 124.




12a
Fig 12b
Fig 12c
Fig 12d
Fig 15
Fig 15e
Fig 15f
c
d
b.
Fig. 19c.
diagrammatic
Fig. 20.
Fig. 21
Fig. 22
Fig. 26b
Fig. 27
Fig 28
Fig. 33.
Fig. 34a.
Fig. 34b
Fig 35.
⅓ nat size.

of exciting phosphorescence at their ends. They are capable of being deflected; and their ends are bent aside by a magnet in the same direction as the ends of the kathode-rays would be which radiated from the reflecting surface to the points reached by the reflected rays.

LI. *On the Influence of the Shape of the Kathode on the Distribution of the Phosphorescent Light in Geissler's Tubes.* By Dr. E. Goldstein*.

[Plate VII. figs. 9–35.]

CYLINDRICAL wires cut off at right angles have been almost exclusively employed as kathodes in systematic investigations on the discharge of the induction-coil in rarefied gases, or, in particular cases, spherical electrodes or plane circular disks. Kathode-surfaces, which can be divided into two halves of similar shape by an infinite number of cuts, do not give rise to a class of phenomena which I have observed with kathodes of regular surface, in which nevertheless there is no axis of symmetry corresponding to an infinite number of equivalent sections.

We are concerned with extremely regular figures, in which the phosphorescent light of the walls illuminated by the raysfrom those kathodes arranges itself, which, however, are for the most part altogether unlike the shape of the kathode itself. Reserving a detailed description, I may here give the general characters of the most important types of these figures †.

Kathodes of concave spherical form were first examined constructed of thin soft iron, which was first of all stamped and then ground into the desired form.

The kathodes were soldered at the middle points of their convex sides to wires which conveyed the current, and which were insulated by being covered with glass thermometer-tubing between their junction to the kathode and the point at which they entered the vessel.

The discharge-tubes were glass bulbs of 4 to 5 centim. radius; the axis of the spherical concave mirror which formed the kathode was placed in a diameter of the bulb. The distance of the kathode from the wall, measured along this diameter from the centre of the mirror, could be varied; in the

* *Monatsber. der Königl. Akademie der Wissenschaften zu Berlin*, July 1881 Translated from a separate impression communicated by the Author.

† A preliminary notice appeared in the *Wien. Akad. Anzeiger* of the 13th Jan. 1882. The phenomenon of figures in phosphorescent light dissimilar from the kathode was described by me for the case of a kathode of cylindrical curvature so long ago as 1876 (*Wien. Sitzungsber.* lxxiv. [2] p. 465).

experiments next to be mentioned it was made equal to twice the radius of curvature of the spherical kathode.

If we now assume, as for example Crookes does in his well-known memoirs, that from each point of a concave kathode only one rectilinear ray radiates, and that along the normal to the surface, it would follow that the phosphorescent image of a concave kathode on a concave spherical wall, at a distance of twice the radius of curvature of the kathode, would be identical in form and dimensions with the kathode itself, if the radius of the vessel were equal to that of the kathode; it would be coincident in form and nearly in dimensions with the kathode itself if, as in my experiments, the radius of the vessel were greater than that of the kathode, without the kathode having any considerable aperture. There will be no essential change in the character of the phenomena to be expected, if we also take into account the feebler phosphorescence caused by the rays* emitted by the elements of the kathode on its edge in variously oblique directions up to the tangential direction. But experiments show very different phenomena.

1. Fig. 9*a* represents a square of the actual size, ground into a spherical surface of 40 millim. diameter; and fig. 9*b* represents the phosphorescent image, also of the actual size, formed by this kathode in a highly exhausted glass vessel of 8 centim. diameter; we remark a star of light with four rays, the axes of the rays being at right angles to the sides of the square kathode. In the figure representing the luminous star, the edge of the kathode is marked by black dots in order to indicate the relative positions of kathode and image. At extreme exhaustions there appear, less distinctly marked, four much shorter rays coming from the centre of the image and corresponding to the directions of the diagonals of the kathode†.

An equilateral triangle having the same curvature (fig. 10) produced a star with three rays, whose axes were at right angles to the sides of the triangle. So also polygons of 5, 6, 7, and 8 sides gave stars, with a corresponding number of rays, whose axes appeared to bisect the sides of the polygon at right angles.

The position of these figures with reference to the kathode

* *Eine neue Form electrischer Abstossung*, i. p. 11.

† In the accompanying figures of portions of the surface of a sphere, the arcs of great circles between the centre of the light-figure and the separate points forming the bounding surface of the figures are approximately represented by the chords of these arcs, or in the smaller figures by the corresponding aliquot parts of these chords. This corresponds with the method of measurement employed, in which distances on the spherical surface were determined by the direct distance between the points of a pair of compasses applied to the surface.

is worthy of remark, as throwing light upon their mode of production. If we cover up the upper portion of a polygonal kathode by a screen placed between the kathode and the centre of curvature, then the *upper* arms of the star are wanting in the image. The arms are therefore not produced, as we might have expected, by the radiation from the portions of the kathode diametrically opposite.

A four-armed cross, fig. 11 *a* (actual size), forming a portion of a sphere of 40 millim. diameter, gives fig. 11 *b* on the wall of a vessel 8 centim. in diameter, in which again the position of the kathode is marked by dots. The metallic arms of the kathode thus correspond to the dark arms of the cross in the phosphorescent figure, and the light is concentrated in fields corresponding to the intermediate spaces in the kathode.

If, again, the kathode has three arms, with angles of 120° between the arms, we obtain a figure with three dark arms. These again fall upon the metallic arms, and the bright fields upon the intermediate spaces between the metal arms *.

The dark arms of the image, however, are much narrower with the four-rayed figure than with the three-rayed one, if the arms of the kathode are of equal width and equal length in the two cases.

We obtain analogous figures when the kathode has five or six (uniformly distributed) arms, except that as the number of arms increases the dark fields which correspond to the metallic arms become narrower, not only absolutely but also relatively —that is, in proportion to the width of the bright intermediate fields.

These two typical forms—the polygon, and the star consisting of rectangles—may suffice as a preliminary indication of the forms of the images which appear when the exhaustion is sufficient.

2. The images formed by kathodes of this sort alter very much when the density of the gas is altered.

The image-forms described above are obtained with densities of gas about $\frac{1}{100}$ millim. mercury. We can, however, trace the phosphorescence at pressures only slightly less than 1 millim., or even over this pressure, by including in the discharge at the same time sparks in free air. We obtain then, for example, for the four-armed cross-shaped kathode, fig. 11 *a* (radius of curvature 20 millim.), the images 12 *a* to 12 *e* in

* I have made repeated attempts to obtain a result described in Carl's *Repert.* 1880, p. 244, where a sort of three-rayed star gave simply an erect image of the kathode, but never with success.

succession, to which succeeds the image represented in fig. 11 *b* when the exhaustion becomes sufficient.

We obtain then at first an image of the kathode itself—a cross having nearly the dimensions of the cylindrical projection of the kathode upon the wall. The arms of the cross in the image grow smaller and become the central lines of a well-defined square, that appears as a luminous background round the cross. As the density decreases, the square becomes smaller and its luminosity increases. Its sides lengthen beyond the angular points, and form points resting on the sides of the square. The central cross disappears ; only the intersection of its arms remains as the bright centre point of the whole figure.

The square now becomes smaller; and the points superposed on the sides become narrower in the same proportion. This takes place in consequence of the concave sides of the contour-lines approaching each other, and passing over each other, 12 *d* and 12 *e*. As this displacement continues in the same direction while the density of gas decreases, the pair of arcs which previously intersected finally pass apart, and leave a dark space between their convex sides : thus finally, at great exhaustion, the dark cross already figured in fig. 11 *b* results.

The series of changes thus described is typical also for the successive images given by crosses or stars with other numbers of arms.

We obtain in each case at first an image closely resembling the kathode, and of nearly the same dimensions. Observation with three- and four-rayed stars (fig. 13 and fig. 14), shows—what was not evident with the four-rayed star, nor generally with regular figures with an even number of arms—that these figures are reversed images of the kathode, formed by the rays from the kathode crossing each other. Next, the background round these figures becomes brighter, bounded by as many sides as the kathode-star has rays. The rays of the star-figure form the smaller radii of the polygon so formed. As the exhaustion of the gas continues, this polygonal figure decreases, its surface becoming brighter; and the inserted star disappears, its bright centre point remaining visible the longest. On each side of the polygon appears a point similar to those in fig. 12 *d*. The concave sides of the bounding arcs approach each other more the further the exhaustion proceeds ; and at last, as the result of displacements exactly similar to those of figs. 12 *f* and 12 *g*, there appear dark rays between bright fields, which again correspond to the metallic arms of the kathode.

The type of successive changes in decreasing density of gas with polygonal kathodes may be illustrated by the series of images given by a concave spherical square (fig. 15). Figs. *c* and *d*, for the sake of greater distinctness, are represented on a scale somewhat larger. If the kathode-polygon possesses an odd number of sides, then the luminous polygonal images corresponding to figures 15 *b–d* are reversed with reference to the polygonal kathodes, corresponding with the observation made with star-shaped kathodes with an odd number of rays.

It is these luminous figures so far described and figured which first strike the observer, in the phosphorescent images of the corresponding kathodes; upon closer observation we see that the other surfaces of the glass vessel are also not devoid of luminosity, but show phosphorescent surfaces of feeble luminosity at different points. The boundaries of these surfaces, in the case of star-shaped kathodes, are prolongations of the luminous curves which bound the chief figures; with polygonal kathodes they are extensions of the star-rays observed in the luminous figure. Partly because, from what has been said, a sufficient preliminary account of the way in which these less-luminous portions complete the figures already figured and described is now possible to the reader, and partly to economize space, I abstain from further description of these outlying portions until the separate observations have been completely described. The image obtained in a spherical vessel of about 9 centim. diameter, employing as kathode the cross figured in fig. 11 *a*, may serve as a good example: it is represented in figs. 16 *a* and 16 *c*, in the pointed phase and in the dark-cross phase.

3. Besides the forms obtained by simple variation of the number of sides and arms of polygons and stars composed of rectangles, I have further examined the images given by numerous other forms of kathode—some simpler, some more complex—in order to separate as far as possible that which is general from that which is special. Thus, for example, of simpler forms were examined:—rectangle, rhombus, rhomboid, isosceles right-angled triangle, &c. of compound forms; crosses composed of obliquely compounded rectangles, and crosses formed of isosceles triangles (the latter either with the vertices or with the bases outwards); further, figures such as are obtained by cutting out of squares segments of circles or smaller squares at the four sides.

So far as the effect of the form of the kathode, and of the variation in density of gas, manifests itself with these images, I must defer a description of the phenomena observed until I give a more complete explanation of the whole. The

following general rules (4–7), however, hold good for all the kathode-forms mentioned.

4. At constant density of gas, the forms of the phosphorescent images (not only their absolute dimensions) alter when the distance of the kathode from the wall of the vessel is made to change; as the distance of the wall decreases, the same figures appear, in the same order, as when the distance of the wall remains constant and the density of gas decreases.

Instead of altering the distance of the wall by displacing the kathode, we may, as in the vessel represented in fig. 17, displace the wall which receives the rays with reference to the kathode.

If we experiment with varying distance of wall, and also with varying density of gas at the same time, then, in order to pass from one given figure to another of the same series, the wall must be displaced through a greater distance the smaller the density of gas is.

This shows that all the figures which a kathode can call forth upon a fixed wall as the density of gas decreases, do at any fixed density of gas already exist in space one behind the other at the same time, and that the different figures are produced by the rays intersecting each other in various ways at different points of space. As the density of gas decreases, the images move further apart and further away from the kathode, no doubt because the rays which first converge become less convergent, and then, when after intersection they diverge, their divergence is decreased.

The influence of the distance of wall thus described would lead us to expect that the series of images given in figs. 12 and 15 as examples obtained with a kathode at a distance equal to twice the radius of curvature, would not include all the forms which the particular kathode is capable of producing, but that a diminution of the distance of the wall at the highest obtainable exhaustion would in general give other figures besides these. This conclusion is found to be confirmed by experience, although the forms thus obtained are not for the most part so striking as those previously described. It may, however, be mentioned at this stage as worthy of consideration further on, that upon diminishing the distance of the wall the dark arms of the cross in fig. 11 *e* increase considerably in width.

5. If we take similar plane figures, and then bend them into portions of spheres of different radii, ρ_1, ρ_2, ρ_3, and place them as kathodes in similar vessels with equal distance of wall, then at equal density their images represent different phases of the series of figures obtained from a single such

kathode with varying distance of wall; and the figure produced by a particular kathode k corresponds to a greater distance of wall from the kathode used to compare with, the greater the curvature of the kathode k is.

This result might indeed have been regarded as *à priori* probable. We might indeed expect to obtain simultaneously like figures from different kathodes (similar in their original plane condition) by making the distance of the wall equal to $n\rho$, $n\rho_1$, $n\rho_2$, &c. for different radii of curvature ρ, ρ_1, ρ_2, &c. —that is, the distance of the wall in each case the same multiple or submultiple of the radius of curvature—as, for example, by placing each kathode at a distance from the wall equal to twice its radius of curvature.

But experiment shows also that in this case the phases are different; and the increased curvature acts in the same way as, *cæteris paribus*, the increase of the distance of the wall or an increase of the density of gas. This influence goes so far that, with kathodes which are much curved, it has not been found possible by exhausting the gas to produce those forms of the series of figures which, with electrodes of less curvature, correspond to the lowest degrees of the scale of density. Thus, for example, with the four-armed cross fig. 11*a*, of a radius of curvature of $12\frac{1}{2}$ millim. instead of 20 millim., and with a distance of wall 2ρ, we find it impossible by exhausting to reach the phase of the dark cross fig. 11*b*. The figure obtained immediately before the cessation of the current at the greatest exhaustion is the figure with curved points, fig. 12*c*.

6. If we leave the general form of the kathode and its curvature unaltered, but increase the aperture of the kathode, this increase acts also as an increase of the distance of the wall would do.

If, for example, we replace a square of 12 millim. in the side which has been bent to form a spherical surface of 40 millim. diameter by a square of similar curvature, but with sides 30 millim. long, then at the extreme exhaustion we do not advance further than fig. 15*d*, whilst the small square gives us figures up to 15*g*.

We obtain similar results with the more complicated forms of kathodes—for example, the four-armed cross made up of rectangles, fig. 11*a*. If the length of the cross be increased from 20 to 25 millim. without altering the width of the arms, then at the greatest exhaustion at which the current will pass, the dark cross is just visible; but it cannot be obtained with arms of any considerable breadth.

If the cross is increased to 40 millim. the dark cross is no

longer to be obtained, and fig. 12*c* is the last obtained at extreme exhaustion.

The results are qualitatively identical if we increase the breadth of the arms in the same proportion*.

7. The experiments mentioned in No. 5 show that the phase alters when plane kathodes of similar shape are bent into spherical surfaces of different curvature. Since the same figure, formed to different spheres, will form a mirror of greater aperture the smaller the radius of the sphere is, the result described in No. 5 as to the influence of increased curvature might possibly seem to be only another statement of the influence mentioned in No. 6 of increased aperture. In that case we should expect that kathodes of like form and different curvature but like aperture, at distances forming the same multiples of their radii of curvature, would give like figures at like densities of gas.

To test this I constructed a series of kathodes (*e. g.* three four-armed crosses, I., II., III.), whose dimensions were as follows:—

	Radius of curvature (ρ). millim.	Length. millim.	Breadth of arms. millim.
I.	$12\frac{1}{2}$	$12\frac{1}{2}$	$2\frac{1}{2}$
II.	20	20	4
III.	$26\frac{3}{4}$	$26\frac{3}{4}$	$5\frac{1}{3}$

The kathodes thus all covered equal aliquot parts of the spheres from which they were formed. They were placed at a distance 2ρ from the spherical wall of the similar containing vessels—that is, 25 millim., 40 millim., and $53\frac{1}{2}$ millim. respectively.

Here also the result was obtained that there is no identity of phase for equal density, but the figures given by kathodes II. and III. corresponded to the figures which kathode I. would have given if the distance of the wall had been increased. This occurred indifferently, whether the two electrodes of each of the three discharge-vessels were separately connected with the poles of the induction-coil, or whether, in order to secure equal intensity of discharge, the current was sent at the same time through the three vessels, connected together in line.

* The changes in curvature and aperture of kathode described in the two preceding paragraphs cannot, in one respect, be always compensated by changes in density of gas or in distance of wall: the last term of the series of figures (corresponding to fig. 15*g*) for a polygonal kathode shows with kathodes of greater curvature or larger aperture richer differentiation and finer detail. The rougher figure cannot be reproduced with these kathodes.

8. The phosphorescent figures which appear when spherical electrodes are employed, as might have been expected, become replaced by others when the originally plane kathodes are formed, not into spherical surfaces, but into cylindrical or conical surfaces. I will content myself in this cursory report with mentioning that, with cylindrical kathodes, the phosphorescent images are different according to the position which the axis of the cylinder has with reference to the axis of symmetry of the kathodes. Thus, for example, with a square kathode formed to a cylindrical surface we obtain different figures according as the axis of the cylinder is parallel to a side of the square or to one of its diagonals. In the same way there are marked differences in the images of cross-figures according as the axis of the cylinder is parallel to one of the arms of the cross, or bisects the angle between the arms.

9. With reference to the explanation of the phenomena so far described, it is only wise to maintain a certain reserve towards a class of phenomena as yet very imperfectly known. A doubt as to the success of the attempt to bring a large portion of these phenomena into any simple relationship with known causes, could only be supported by the observations described in No. 10, on the images given by plane kathodes. All that I hope to do is to give some indications, derived from experiment, on the direction in which the explanation of many of the phenomena observed with star-shaped kathodes is to be sought. As to the explanation of most of the phenomena given by the above-mentioned polygonal kathodes, I do not here venture upon any hypothesis. The star-shaped kathodes are of course also polygons with reentrant angles: and their separation from the polygonal surfaces appears unnatural at first sight. Nevertheless the phenomena which we have to explain with star-shaped kathodes are for the most part just those which are closely connected with the presence of reentrant angles, and which may be approximately explained by taking into account only the edges of the kathode. The phenomena due to the surfaces themselves, in those forms of kathode where the surface of the kathode is relatively unimportant as compared with the extent of its bounding edge, are insignificant in comparison with the phenomena due to the curves of its circumference ; and the former remain unexplained here just as with the polygons with convex angles, where the phenomena of the surface constitute the chief part of the phenomena observed. Only the surface-phenomena produced at small exhaustions can be approximately accounted for with both kinds of polygons That the images of a star-shaped kathode which appear at relatively small exhaustions result

from the intersection of the rays emitted by one half of the cross with those from the other half, was shown to be probable by the experiments already described with stars of an uneven number of arms. This may, however, be better shown by experiments with screens arranged to throw shadows, which also prove the same thing for kathodes with an even number of arms.

If we arrange (fig. 18 *a*) a screen of paper or of mica in front of the kathode, and at a distance from its centre less than the radius of curvature, so that, for example, the lower half of the kathode-cross is covered, then in the phosphorescent image it is the upper half of the cross which is wanting (fig. 18 *b*).

Since at a certain density these images formed on the wall at the distance 2ρ from the kathode possess nearly the same dimensions as the kathode itself, it follows that the rays which produce the images, at least for the most part, are under these circumstances at right angles to the surface from which they are emitted, assuming, of course, that their course till they strike upon the walls of the tube is rectilinear.

(Exactly similar experiments show that the images of ordinary polygonal kathodes observed at small pressures are formed by the intersection of the rays which issue from points situated symmetrically with reference to the centre of the kathode.)

As with the figure of the kathode-cross, so also in the contour of the square (fig. 12 *b*) which appears surrounding the figure of the cross as the density decreases, each side is formed by rays which come from the opposite side of the kathode.

We may obtain a more satisfactory explanation of the figures of kathode-crosses which appear at small densities; we only need to place a screen P (fig. 19 *a*) between two of the arms of a cross, so that it projects beyond the kathode on the concave side. The screen may be rectangular, and the edge which faces towards the image may be called its front edge. I anticipated, and the anticipation was verified by experiment, that the figures which correspond to the higher degrees of exhaustion are formed by the rays emitted by one arm of the kathode suffering a repulsion by the neighbouring arm, of the same or a similar kind to that described in my work on kathodic deflection. This deflection cannot take place through a solid plate.

If now, at high exhaustion, we obtain repeatedly the figure with the dark cross so often mentioned, then, when the plate is introduced, we obtain the figure 19 *b*; or, if the plate does not project so far beyond the kathode, we obtain 19 *c*.

We have therefore, in the first case, only the left bounding

arc of the upper arm of the cross, and only the lower bounding arc of the right-hand arm: the two dark arms themselves cannot be perceived; but the space between the bounding arcs mentioned is uniformly illuminated.

The two bounding arcs, which are now absent, result therefore from an action between the upper and right-hand arms, prevented or hindered by the presence of the screen.

If the plate is withdrawn a little, then, as already mentioned, the figure 19 *c* is seen.

The dark arms which were wanting in the first image are here seen as very narrow strips, and the corresponding two boundary curves are of feeble luminosity. The more the screen is withdrawn towards the kathode the broader do the two dark arms become, and the more closely does the luminosity of the two curves approach to the normal luminosity. The mutual action between each two neighbouring arms, to which the production of the dark cross is due, may be supposed to take place in three different ways:—either (1) as an immediate action of each of the arms of the kathode upon the other, which thus indirectly produces an effect upon the course of the electric rays; or (2) as an action of each arm upon the system of electric rays emitted by the other; or (3) as a mutual action of the two systems of rays.

Any further discussion of the utility of the three hypotheses at present would lead us too far away. I will content myself with remarking that the assumption (1), so far as I see, is opposed to the details of the phenomena of kathodic deflection; but it is not possible to decide certainly between (2) and (3).

Taking account, however, of the fact that the third assumption involves certain accessory assumptions which have not yet been verified by experiment, I shall employ the language of the second hypothesis in seeking for further explanation; which hypothesis, moreover, I have employed throughout in my research on kathodic deflection, in describing the phenomena observed.

If now (fig. 20) rays issue from the edge *a* of the right-hand arm (1) towards *b*, then, according to the laws of kathodic deflection, these rays will be repelled by the edge *b* and the whole surface of (2). The same holds good for the rays emitted by the left arm at *c*. Let fig. 21 be a rough representation of the kathode, turned through 90° from its position in fig. 20, the upper half having moved forwards; and let *l* and *r* represent the upper edges of the left and right arms respectively, and *o* the (fore-shortened) upper arm. The repulsion which *o* exerts upon the systems of rays emitted by *r* and *l* will cause the mutual convergence of these two last

to decrease, but so that the two systems of rays still intersect each other (at x, in fig. 22).

If now a plate capable of phosphorescing move at right angles to the plane of the drawing in the space free from rays between the points of intersection x and o, a dark space will be seen on it, bounded by its luminous intersection with the repelled systems of rays; thus the upper arm of the dark cross is bounded by its two luminous curves. As the density of the gas decreases, the repulsion increases; the convergence of the two pencils of rays will therefore be still further diminished; the point of intersection, x, moves further away from the kathode; and the plate may now be further off from the kathode and still show the dark cross. As we saw already, in examining deflection, the rays further off from the repelling surface are carried with those which are nearer to it, but are not deflected through so great an angle; the deflected rays are therefore compressed together on the side turned towards the repelling surfaces; hence the greater brilliancy of the narrow contour-line immediately bounding the dark arms.

If the phosphorescent plate moves away from o beyond x, the dark cross on it must of course disappear, since the plate comes into a space occupied by rays. We must therefore have a bright field on the plate opposite o, the outer contour of which is again formed by the intersection of the plate with bounding surface of the deflected system of rays.

If we take account of the fact that, according to the form of the dark cross, the curved surfaces of these systems of rays have their *convex* sides turned towards each other before the intersection, we see that beyond x they will have their concave sides towards each other, and will thus form the "curved points," which observation shows to exist.

Consequently the upper "curved point" (fig. 23) is not formed, as we should have expected at first, by rays from the upper arm of the kathode, but it is formed by rays from the two horizontal arms of the kathode—the left-hand half, and in particular the contour-line l, being formed by rays from the right-hand arm R, and the right-hand half by rays from the arm L.

If this is the right way of regarding the curved points, we ought to find it confirmed by experiments in which shadows are thrown. This is, in fact, the case.

If we put a plate as a screen close in front of the kathode, so that, as in fig. 24 a, it covers one half of the kathode obliquely, then at the density at which the curved points appear we observe the phosphorescent figure 24 b. The bounding curves shown in dotted lines are now wanting. The phe-

nomenon thus agrees most exactly with the explanation given above.

If we exhaust up to the density which corresponds to the figure with a dark cross for the uncovered kathode, we obtain the figure 25. By comparison with fig. 24 *b*, we see here also very plainly that the bounding curves of the dark cross, with their convex sides turned towards each other, are nothing else than the bounding lines of a "curved point" with concave sides turned towards each other, which have become displaced across each other.

10. The most remarkable phenomena of the kind we are now considering, however, are undoubtedly those produced by *plane* kathodes.

That plane kathodes cut into figures would present phenomena similar to those of the dark cross was probable, since rays emitted by one edge of an arm of the figure would be repelled by the other edge. Thus, in fact, a four-armed plane (fig. 26 *a*) gives a figure resembling fig. 26 *b*, with a distance of wall from 3 to 4 centim.

The figures 12 *c*, 12 *d*, &c., observed with the curved cross-shaped kathode, do not appear with the plane kathode, but as the density decreases the first figure recognizable is the dark cross; if the density is still further diminished the arms increase in width, and show in the part nearest the centre a nebulous luminosity, with convex contour line (fig. 27).

If the distance of the wall be made less than 3–4 centim., this nebulous portion increases in brightness, and contracts, becoming better defined; and with a distance of wall of about $1\frac{1}{3}$ millim. we obtain the figure represented in fig. 28. In each of the four arms, which would otherwise be dark, there appears a beautifully forked line of light, the space between the forks being filled with uniform light with convex contour, while each fork is connected with the others by a slightly luminous arc of light.

In using a concave spherical cross of the same dimensions as kathode, the radius of curvature being, however, greater than 25 millim., we observe in the arms of the dark cross, so often mentioned, which appears at low pressures, this same luminous fork. We have therefore, as was to be expected, a gradual passage from the forms given by the curved kathodes to those of the plane kathodes, the figures given by the plane kathodes regulating those of the spherical kathodes. Inasmuch as no explanation of the images produced by the former is possible, so for the present no explanation is to be given for a number of phenomena obtained with the curved surfaces. The dark cross produced by the plane cross-shaped kathode

we might foresee ; but it is not possible to predict, *à priori*, the phenomena produced by plane kathodes, not cut out, nor having concave edges, and these constitute an entirely new class of phenomena. I make here the general remark that, while it is true that the images given by plane kathodes alter somewhat with decreasing density, they do so much less than the figures obtained with curved kathodes, and often the changes consist only in the acquisition of richer detail and more definite contour, or perhaps somewhat larger dimensions. If these images are produced by kathodes of relatively small surfaces (1 to $1\frac{1}{2}$ square centimetre), the kathodes must be placed tolerably near to the wall of the vessel. The larger the surface of the kathode is, the greater the distance of wall at which it first appears. If with a given kathode we go further off than a certain distance, we obtain a uniformly illuminated phosphorescent surface, whose luminosity slowly and gradually decreases from the centre outwards.

If a plane square be employed as kathode, we obtain fig. 29 on the wall of the spherical discharge-tube of 9 centim. diameter. The relative size and position of the kathode ($2\frac{2}{3}$ centim. distant from the wall) is marked by the dotted lines. We obtain therefore a star of eight arms, four of whose rays correspond in direction to the diagonals of the kathode, and the others to its central lines. The centre of the figure is formed by a feebly illuminated square space, upon which the star appears; a luminous zone surrounds the dark central space, formed of four arcs, convex outwards.

The four rays corresponding to the central lines of the kathodes have their maxima of light within this outer zone ; the four diagonal rays, which are narrower than the others, are uniformly luminous, except that all eight rays are more luminous at the centre of the whole figure. The whole figure is considerably larger than the kathode-square, the darker central square space being larger than the kathode.

The figure given by a rectangle 2 centim. by 1 centim., with its longest sides horizontal, on the wall at a distance of $1\frac{1}{3}$ centim., is represented in fig. 30. The main figure is thus a narrow line of light, corresponding to the central line of the rectangle, which forks at each end, and is surrounded by a broad band. The lower ground is an oblong, rounded at the small ends.

A plane circular disk, as mentioned in the introduction, gives no special figure on the illuminated ground obtained in all the figures, unless we regard the bright central point of the image as such a figure. The rays produce a circular disk, which is not sharply defined, with the feebly illuminated ground, and

brighter zone at the edge ; the centre point is, of course, bright. An ellipse whose axes measure 10 mm. and 20 mm., on the other hand, gives a comparatively complicated figure (fig. 31) at a distance of wall of 1 cm. in a vessel of $9\frac{1}{2}$ cm. diameter. In all these figures the ground expands the more the density of the gas is reduced.

If we use kathodes made up of several of these simpler forms joined together, we of course obtain much more complicated images. The images obtained with the comparatively simple form of kathode fig. 32 *a* may serve as example. A square out of whose edges smaller squares have been cut (fig. 32 *b*) shows the central portion of the phosphorescent image completely; but, to save space, the figure gives only two of the streams of light which project from the four sides.

To enter at present further into detail in describing a large number of these phenomena, which are often characterized by surprising beauty, would be without scientific interest, since the simpler cases already described sufficiently represent whatever is new and characteristic amongst phenomena of the kind, viz. :—

(1) The fact that such figures are produced.

(2) The circumstance that the magnitude of the images varies with the density of the gas, and exceeds the magnitude of the kathode itself at high exhaustions.

This latter phenomenon, to which I have devoted a separate series of experiments, may be supposed to occur in either of two ways : either the direction of the rays varies with the change of density, the pencil emitted by a plane becoming more divergent the smaller the density of the gas becomes ; or the direction of the rays remains constant, and as the density of the gas decreases the previously unobserved rays of those elements of the surface which are situated obliquely to the outward-directed rays become strengthened.

Experimental trial gives as result that the first-named reason (variation in direction of rays with variation of density) is to be preferred. Of the different methods of proof employed I will mention only one here.

If we cut slits in a plane disk, the spaces in the disk show themselves in the phosphorescent image, for which the disk acts as a kathode, as narrow dark lines. A number of concentric and equidistant semicircular cuts were made in a disk, so that the outside one lay near the edge of the disk (fig. 33). In the phosphorescent image there appear, even at the highest density at which it is visible, the same number of dark semicircular lines, showing that even at the highest density the phosphorescence produced by the elements on the edge of the

disk is manifest. If the exhaustion be carried further, the dark semicircular lines move further apart ; and at a constant density the distance between any two of the dark semicircles is greater the further the pair in question lies from the centre. This last behaviour manifests itself also if the phosphorescent image is received on a plane surface parallel to the kathode instead of upon a spherical wall.

The phenomena described for plane disks lead to the following conclusions:—

(1) The different points of a plane kathode-surface are not of equal value in the emission of kathode-rays, but the intensity of the rays depends on the position of the elements by which they are radiated with respect to the contour-line of the kathode.

(2) The rays of a plane kathode-plate do not in general form a parallel pencil* ; but the inclination of the rays varies from element to element, in accordance with the distance from the contour-line of the plate.

(3) The direction of the radiation from each separate element varies moreover with the density of the gas. The smaller the density becomes, the more does the direction of radiation differ from the normal to the element ; and the direction of deviation is always outwards.

Whether any density exists at which there would be deviation from the normal in the opposite direction (*i. e.* inwards), at which therefore the rays would be convergent, is a question to which an answer must for the present be deferred. The deviations which make their appearance as the density diminishes are the more considerable the nearer the element in question lies to the edge of the surface.

11. *Convex* kathodes of regular outline also produce regular phosphorescent images.

Convex spherical forms, so far as I have observed, give the same figures as plane kathodes of the same outline—only of larger dimensions at an equal distance of wall, in consequence of the stronger divergence of the rays.

With cylindrical convex surfaces the figures obtained with plane surfaces of similar outline are deformed, as might have been expected ; the image is, *ceteris paribus*, more expanded at right angles to the axis of the cylinder than parallel to the

* If we assume that each point of a kathode emits, not simply one ray, but a small conical pencil of rays, then in the above proposition, instead of "ray" we must read "axis of conical pencil of rays." I am still occupied with experiments to determine the limit of aperture which we can ascribe to the pencil of rays from a point of a surface.

axis, and that in greater degree the greater the curvature of the surface.

12. Still another class of simple forms of kathodes which produce figures are such as may be termed "interrupted;" to which belong, amongst others, prismatic tubes open at the ends and cut off at right angles, then (plane) figures formed by bending wire (*e. g.* polygons of wire), and so on.

The images given by such wire kathodes are amongst the most beautiful which can be obtained. In order to render intelligible at least the general mode of their formation, the following may be mentioned:—

If a cylindrical or prismatic tube cut off at right angles to its axis and open at both ends be employed as a kathode, then as the exhaustion proceeds a conical pencil of rays issues from each of the open ends of the tube, the axis of which is coincident with the axis of the tube, and which expands more and more into the gas-space the further the exhaustion is carried. Assuming rectilinear rays, the pencil is then so directed as if it issued from a metal plate closing the actual opening of the tube. When the exhaustion is sufficiently great, this pencil reaches to the wall of the vessel and excites phosphorescence there.

The phosphorescent image of this pencil forms the phosphorescent figure produced by a tube-shaped kathode.

In general, there are two images produced corresponding to the two pencils which issue from the two openings of the tube, and which are congruent if, for example, the discharge-vessel be spherical and the middle point of axis of the tube coincide with the centre of the sphere.

We have a similar result to that obtained with a cylindrical kathode, when the wall of the tube kathode is saddle-shaped, such for example as is formed by the revolution of an arc of a circle about an axis lying on its convex side.

If we imagine such a tube of very small height, we obtain a case which can also be realized by surrounding a space with a wire of the corresponding form. Just as pencils of rays issued at right angles to the opening of the tube, so they issue from the wire kathode at right angles to its plane, to all appearance as if the empty space surrounded by the wire acted as a kathode.

The luminous figures obtained from wire kathodes are larger than the space enclosed by the wire, even at small distances of the wall from the kathode. The images may show great changes with change of pressure. I content myself with

giving as example the image represented in fig. 34 *b*, given at high exhaustion on the wall of a spherical vessel by a regular pentagon of 12 millim. in the side bent out of wire about 1 millim. thick (fig. 34 *a*). The normal to the polygon at its centre point was placed radially.

The waving contour-line which passes through the ends of the five-rayed star forms a perfectly sharply defined contour-line.

If we employ a regular wire polygon of some other number of sides (3–8), we obtain a similar luminous star with the corresponding number of rays. With polygons with an odd number of sides, the rays of the star correspond in direction to the longer radii of the kathode-polygon.

With polygons of an even number of sides, on the other hand, the axes of the rays correspond to the shorter radii of the polygon, and thus appear to intersect the sides of the polygon at right angles.

With the phenomena which are given by interrupted kathodes may be connected an observation made with plates perforated with holes, which is at first sight surprising. If we make use of a square plate perforated with a number of holes (fig. 35), we might perhaps expect that the places from which the metal has been removed would appear dark in the phosphorescent images, or at least would correspond to mimima of light. We find, however, that these points are really maxima of light; thus with the kathode fig. 35 we obtain 16 very bright points of light. The reason is to be found in the fact that the walls of each perforation form a short open tube; and, according to what we have seen, a bright pencil of light issues from such a tube, which at suitable exhaustion extends to the wall and excites phosphorescence there.

Berlin, Physical Institute
of the University.

LII. *Notices respecting New Books.*

Graphical Determination of Forces in Engineering Structures. By James B. Chalmers, *C.E.* London: Macmillan. 1881. 405+ xxvi pages, 6 plates, 267 cuts.

THIS is a large and important work, aiming at being a complete treatise on use of graphic methods in engineering-designs. It is a high-class work, requiring a fair knowledge of modern geometry for its comprehension. To facilitate this (to the Engineer) a special Chapter on "Projective Geometry" (66 pages) is given; within this compass a wide range is compressed, *e. g.* projections, homology, Carnot's, Pascal's, Desargues's theorems, &c.

In application to Engineering great superiority is claimed for graphic methods over computation. The practical applications are skilfully and neatly worked out; and the study of them is an intellectual treat (not easy reading). The scope of the work is very wide: Resultants of Forces, Moments, Centre of Gravity, Moments of Inertia, and Stresses in Structures, *e. g.* in Frames, Beams (supported, fixed, and continuous), Arches (rigid and elastic), Suspension-Bridges, Retaining Walls, and Tunnels are all treated by graphic methods. These processes are only meant to supersede computation: analysis is often used for their actual elucidation; thus the explanation of the graphic methods for the Elastic Arch covers 20 pages of a somewhat difficult analysis.

In some cases the graphic methods have decided advantage, chiefly when the work is simply a repeated application of the theorem of the "polygon of forces:" the diagrams of these are simple and can be quickly drawn. But in cases where sums of products are required (*e. g.* in moments of forces, moments of inertia, &c.), the advantage is not so clear: the process increases greatly in complexity as the number of variable factors in each product increases, the diagrams become intricate, and are finally a network of lines (see fig. 65–i, fig. 143 pl. I, pl. V a), requiring great skill in their original preparation, and not to be unravelled in after-examination without careful study. The risk of mistake in construction, not so much from inaccurate drawing as from mistaking one point or line for another, in such a network must be considerable, and quite analogous to that of numerical slips in computing. Even in the simple case of the sum of the products of two factors, the result would probably be got more quickly with a slide-rule or Crelle's mutiplication-table than by the very neat graphic process given; but in the more complex cases of several factors computation would surely be quicker.

The processes given are by no means always the shortest. Thus the determination of the pressures on the supports due to a single load placed on a beam requires only the division of the line representative of the load into segments inversely proportional to the segments of the beam on either side of the load. This requires only three lines for its complete graphic solution: but the process given (the same as for the general case of many loads) requires seven lines.

When several processes are available for the same purpose, it would surely suffice to give the best, unless each has peculiar advantages in special cases, which should then be stated. Now, for finding stresses in frameworks three processes are given, covering 66 pages. No. i., by "Method of Sections," requiring at every section a preliminary reduction of the frame on one side thereof to an ideal triangle and evaluation of the Resultant force thereon: this is a difficult and troublesome process. This troublesome preliminary is avoided in No. ii., the Method of Sections proper: this is nearly the same in its application as No. iii., but gives rise to

stress-diagrams sometimes imperfectly "reciprocal" to the original —a slight disadvantage. No. iii. is Clerk-Maxwell's beautiful process: this is the simplest and easiest of the three; its simplicity seems to depend on the complete reciprocity of the stress-diagram with the original figure. Methods i. and ii. might have been omitted with advantage, and more space given to the last. This Chapter is illustrated by numerous* well-chosen examples. The three processes therein are really a graphic solution of the "conditions of equilibrium" among the forces at each section or joint; as there are thus only two equations for each section or joint, the magnitudes of two stresses can be found for each section or joint. Thus the problem is indeterminate for a frame at any of whose joints so many bars meet as to require the determination of more than two stresses thereat. This is actually the case with two of the frames (figs. 49 and 51) for which finished stress-diagrams are given without comment. Some explanation is surely wanted in the text as to how this indeterminateness (which is inherent in both) is to be met. One of these (No. 51) is solved in Rankine's 'Civil Engineering,' art. 576, by a method of dissecting the complex Truss into partial Trusses, which bridges the difficulty by (tacitly) assuming the interaction of the partial Trusses.

In Clerk-Maxwell's process for Frames under dead load the graphic methods probably appear at their best; but with moving load the greatest stress in each bar occurs with a different state of load, thus involving a tolerably complete special diagram for each bar, greatly increasing the work and the intricacy of the finished drawing.

In investigating the stability of Retaining Walls and Masonry Arches, again, the graphic methods have decided advantage over computation: this arises partly from the fact of the cross-sections being solid, so that the limit within which the centre of pressure at each joint should fall is easily known to be the middle third. The tracing of lines of pressure and resistance therein is well explained and illustrated.

In the case of the Arch, however, one difficulty (indeterminateness) has not been adequately met. In general many lines of pressure and resistance could perhaps be traced within the "core" or admissible limits (the middle third); and the question is, which is the true line? The author says, "the true line of pressures is that which is nearest the axial line" (art. 181); this seems doubtful. Moseley's Principle of Least Resistance gives a means of locating it so that the passive resistance required at the springing shall be the least: this seems sound for rigid material; but its applicability to non-rigid material is not so clear.

Of all the processes given, the applications to Continuous Beams and the Elastic Arch are naturally the most intricate. These are masterly specimens of the power of graphic work in the hands of

* There are several lines wrong in lower part of fig. 57 *b*.

one skilled in its use. The amount of drawing required for the complete investigation of any one arch seems very great. Pl. v.a, the finished (?) result for an arch, is so complex a whole (although several preliminary drawings are omitted from it) as to require great care for its comprehension; and even it seems (arts. 214, 219) to be only a part of what is required.

Among practical details, it is laid down (art. 121) that "an arch ought to be wholly in compression." Now this principle is obviously right for masonry; but there can be no occasion for applying it to iron or steel arches (as in art. 183): this would surely be a waste of power.

The theory of earth-pressure given, depending on the "angle of repose" and frictional stability, is complex and difficult (covering 23 pages before application to retaining walls). The "angle of repose" is an item which in many cases can hardly be said to be known at all, so that mathematical refinements are of little use. Rankine's theory (which is much simpler) seems good enough for such imperfect data.

There are numerous references to foreign works on geometry and graphic statics; the influence of these is obvious in the diction. The author is thoroughly at home in the practical application of graphic methods; but for a didactic work the mathematical rendering might be improved. Thus there is occasional obscurity in the explanations, *e. g.* props. xxxix., xlii., and arts. 80, 161, 223: results to be derived as the fourth term of a proportion are commonly presented as $\left(a : b :: c : \frac{bc}{a}\right)$ a mere identity; the insertion of the name or symbol for the required fourth term would be more useful (*e. g.* in aiding its discovery in the diagrams). There is also a certain looseness of expression, *e. g.* moments termed forces (pp. 113, 139), the use of the term "centre of gravity" of forces (pp. 268, 274): also of notation, *e. g.* in use of symbols Δ and d, Σ and $\int$ (*passim*), and of $-$ in geometry (pp. 363, 364); also of analysis, *e. g.* omission, removal, or change of variables under summatory symbol (pp. 162, 268; 150, 275, 277; 281); these latter mistakes generally correct themselves in the final results. There are also two mistakes in the geometric theorems. Thus, Poncelet's condition of projectivity (prop. xxxii.) is stated in too general terms without due limitations, and the example given is non-projective. Again, in the proof of Pascal's theorem (prop. xlvi.), the conic and its inscribed hexagon are projected into a circle and inscribed hexagon with opposite sides parallel and one pair equal (which is not generally possible); and it is stated that "the points of the hexagon joined two and two concur in a point P" (and they actually do in the figure in consequence of its being a regular hexagon), which is not generally true.

Several minor points might be improved (in a new edition). Thus there is hardly enough lettering on some of the diagrams for their easy comprehension; and in many cases the symbols given are

too complex for use on a diagram—*e. g.* one length in fig. 100 is marked thus:

$$\frac{\dot{A}_1}{a} \cdot z_1 \cdot \left(z + \frac{k^2_1}{z_1}\right) : c \cdot \tfrac{1}{2} \Sigma \frac{\dot{A}}{a}.$$

Now in all such cases a single symbol (with reference in the text) would be better. The numbering of the diagrams also should be made consecutive; there are at present three numberings intermixed, which renders reference difficult. The number of misprints also is very great, especially among the references. Even with these faults the work is a valuable one, and no one can read it without learning much. ALLAN CUNNINGHAM, *Major R.E.*

Questions in Pure Mathematics proposed at the B.A. and B.Sc. Pass and Honours Examinations of the University of London, with complete Solutions by J. E. A. STEGGALL, *M.A.* Van Voorst, 1882; pp. viii+245.

THE title sufficiently indicates the nature of the work. The solutions, we think, are in all cases neat, and in many instances they are elegant. Mr. Steggall does not confine himself to single solutions, but often gives two or more proofs of the same question. The work is very carefully printed, and there are, we believe, very few typographical errors. On p. 13 another mode of solution might have been indicated, depending on the fact that the sinister side vanishes when $x=y=z$. We venture to suggest that on p. 25 reference might have been made to Euc. vi. 3 and A as also readily furnishing a solution; and the equation on p. 37 might be worked from the fact that the terms on one side are reciprocals of those on the other. But these and other instances we could bring forward only illustrate the well-known fact that there are more ways than one of attacking problems; and the exigencies of space have no doubt restricted the author in general to the single solution he adopts. On pp. 21, 45, occur, as we think, two slight inaccuracies in expression. We note the following slips:—p. 120, "Solving for y" read "x," and for "$\cos x$" read "$\cos^2 x$"; p. 164, for "$-3y$" read "$+3y$"; p. 224, for ">1" read "<1." A few others are easily corrected; but on p. 205 there is a great derangement of "subscripts," and this may puzzle some readers. We hope that Mr. Steggall will receive sufficient encouragement to bring out a second edition in a few years time, with additional solutions up to date; for such works as this are of great service to students.

Geological Chart, arranged by Professor JOHN MORRIS, *M.A., F.G.S., &c.* New Edition. Large Sheet. Reynolds and Sons, Strand, London. 1882.

THIS enlarged and revised edition of a good Geological Table shows the order of the many stratified formations in their regular succession, their mineral characters, uses in the arts, principal fossils,

places of occurrence in the British Isles, and their relative thicknesses. Notes also, to similar effect, on the Metamorphic and Igneous rocks are supplied. The later determinations of geologists as to the better division and classification of some of the recognized groups of strata are, in several instances, incorporated where they are not likely to be otherwise than clear and useful to the student; and there are but few points in printing or arrangement which we would find fault with. We therefore recommend this Chart as having been carefully revised by its well-known accomplished author, and as having been brought up to the latest date in useful information, and forming a complete and ready Geological Synopsis for the several lines of study indicated above.

LIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 151.]

November 1, 1882.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "The Hornblendic and other Schists of the Lizard District, with some Additional Notes on the Serpentine." By Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

The author described the metamorphic series, chiefly characterized by hornblendic schist, which occupies the southern portion of the Lizard and an extensive tract to the north of the serpentine region, besides some more limited areas. He found that this series was separable into a lower or micaceous group—schists with various green minerals (often a variety of hornblende), or with brownish mica; a middle or hornblendic group, characterized by black hornblende; and an upper or granulitic group, characterized by bands of quartz-felspar rock, often resembling in appearance a vein-granite. These are all highly metamorphosed; yet the second and third occasionally retain to a remarkable extent indications of the minuter bedding structures, such as alternating lamination and current-bedding of various kinds. They form, in the author's opinion, one continuous series, of which the uppermost is the thinnest. The general strike of the series, though there are many variations, is either N.W. or W.N.W.

The junctions of the Palæozoic with the metamorphic series at Polurrian and at Porthalla were described. These are undoubtedly faulted; and the two rocks differ greatly, the former being a slate like any ordinary Palæozoic rock, the other a highly metamorphosed schist. Moreover fragments of the hornblende schist and a kind of gneiss occur in a conglomerate in the former, S. of Nare Point.

The author considers the metamorphic series (the microscopic

structure of which was fully described) undoubtedly Archæan, and probably rather early in that division. The rocks of the micaceous group have considerable resemblance to the greenish and lead-coloured schists of Holyhead Island and the adjoining mainland of Anglesey, and of the Menai Strait.

Two outlying areas of serpentine, omitted in his former paper, were described—one at Polkerris, the other at Porthalla. The latter shows excellent junctions, and is clearly intrusive in the schist. The author stated that he had reexamined a large part of the district described in his former paper, and had obtained additional evidence of the intrusion of the serpentine into the sedimentary rock with which it is associated. This evidence is of so strong a nature that he could not conceive the possibility of any one who would carefully examine the district for himself entertaining a doubt upon the matter.

2. "Notes on some Upper Jurassic Astrorhizidæ and Lituolidæ." By Dr. Rudolf Häusler, F.G.S.

LIV. *Intelligence and Miscellaneous Articles.*

ON MR. C. W. SIEMENS'S NEW THEORY OF THE SUN.

BY M. G. A. HIRN.

TO the grave objection brought forward by M. Faye against Mr. Siemens's new theory of the conservation of the solar energy, another, also a very serious one, may be added. This objection may be summed up in few words.

Up to the present time there is no general agreement as to the real value of the Sun's temperature. Père Secchi carried it to millions of degrees. Other physicists, especially in France, lowered it to about twenty thousand degrees. According to the magnificent experiments of Mr. Langley (of Alleghany) this latter amount is, at any rate, a minimum. What is certain then, starting from the fine memoirs upon dissociation of our lamented colleague Henri Sainte-Claire Deville, is that none of the chemical compounds that we know upon our Earth could exist at the surface of the Sun. All, even those which are most refractory in our laboratories, would be dissociated and reduced to their constituent elements. And this is what is admitted in M. Faye's theory of the Sun.

The natural and direct consequence of the preceding fact is, that the chemical compounds which Mr. Siemens supposes to be dissociated by degrees in space by the solar radiation, might certainly, in returning under the action of gravity and in the elementary state towards the central body, become reformed, and regenerate the heat expended in their dissociation in space; but this recombination could only be effected at an appreciable distance from the solar photosphere, and the compounds reproduced, on falling into the bosom of the latter, would be again completely dissociated. This action, therefore, would cause the expenditure of all the heat

previously developed by the combination. From this it follows evidently that this return of the elements towards the centre would contribute nothing at all towards the conservation, or rather the continuous reproduction, of the solar temperature.

It seems to me that Mr. Siemens's theory may be subjected to another decisive critical test. If the solar radiation, or say the heat, whether visible or not, emitted or sent off by any celestial body, during its course effects the chemical dissociation of the hypothetical compounds disseminated in stellar space, the intensity of this radiation must necessarily be reduced by the positive work effected, and all that serves for this work is lost for the visibility of the star.

From this, then, it follows that the lustre of the sun, of the stars, and of the planets must diminish according to a *much more rapid* law than that of the inverse proportion of the square of the distances. I say *much more rapid*; but we must say *extremely rapid*. In fact, from the moment when the recombination of the elements at the surface of the Sun would be capable of regenerating the heat emitted, it is evident that all this emitted heat would be employed in its turn in dissociating the chemical compounds in space. In order that the Sun could be thus continually maintained in its energy, it would be necessary that the distance at which it is visible, far from being unlimited as it probably is, should, on the contrary, be restricted; for wherever it would be still visible there would be light *not employed in chemical dissociation*, and consequently there would still be a definite loss *possible*. Nothing in the aspect of our planets and their satellites, it seems to me, authorizes us to assume that there is any other reduction in the brilliancy of the light than that resulting from the inverse proportion of the square of their distance from the central body. We see stars the light of which has taken at least three years, and others of which the light has perhaps taken thousands of years to reach us. None of this light, therefore, has been employed in chemical dissociation; nothing could have been restored to them by the mode indicated by the ingenious theory of Mr. Siemens.

May I be permitted, in concluding this note, to revert to the objection formulated by M. Faye, and to render it in some degree palpable by a numerical example? In an extensive work upon which I am engaged, upon the constitution of the stellar space, I naturally examine into the consequences that the resistance of a gas diffused in space would have upon the movements of the planets. From this work I extract an example relating to the application of analysis to the motion of our Earth. According to Laplace, the diminution or augmentation which one may attribute to the duration of our sidereal year 3000 years ago, taking into account the uncertainty of the observations, would be 90 seconds at the maximum (a modification of which, however, there is nothing to demonstrate the reality). Accepting a *reduction* of this amount as real, I inquire what density a gas would need to have to produce

it; and I show that it would suffice if there were 1 kilogr. of matter in vapour in 700 thousand millions of cubic metres—in other words, that the density would be 0·000 000 000 001 43 kilogr. It will be seen that we are far from the reduction to the $\frac{1}{2000}$, and even to the millionth assumed by Mr. Siemens. If, instead of taking account only of the resistance opposed by such a gas to the motion of our planet, we direct our attention to the consequences which its existence would have upon that of our atmosphere, we find that, unless we multiply our 700 thousand millions of cubic metres by 10,000, and reduce the density sought for to 0·000 000 000 000 0001 kilogr., our atmosphere would be in a few moments *swept away* by the pressure exerted above by the interstellar gas.

M. Faye is perfectly justified in saying that it is not such or such a degree of rarefaction, but that it is the vacuum (*of matter*, of course) that the astronomer requires to ensure the stability of the movements that his analysis shows. This vacuum no doubt upsets the doctrine, supposed to be so undeniable, which ascribes all the phenomena of the physical world to movements and collisions of material atoms independent of each other. One day or another, no doubt, this doctrine will have to give up its existence, and its defenders will have to resign themselves to admit in the physical world something more than matter in motion. In a remarkable letter to Bentley, Newton said that one must be destitute of all aptitude for a serious philosophical discussion to suppose that between two bodies which seem to attract each other at an unlimited distance, there is not something which establishes this relation; but, he adds immediately, is this intermediary material or immaterial? This I leave to the reader to decide. With that great genius undoubtedly there was no uncertainty upon this latter point; but, perhaps justly, he refrained from putting before his contemporaries a solution which might have seemed incomprehensible to them, as it still is, apparently, to so many minds of the present day.—*Comptes Rendus*, November 6, 1882, p. 812.

REPLY TO M. FAYE'S OBJECTIONS TO MR. C. W. SIEMENS'S THEORY OF THE SUN. BY C. W. SIEMENS.

M. Faye, while approving, generally, of the physical part of my investigations, questions their application to astronomy, and for the following reasons:—

1. That the presence of a universal gaseous medium at a pressure of $\frac{1}{2000}$ atmosphere would oppose an excessive resistance to the movements of the planets; 2. That this vapour, thus distributed, would be gradually attracted towards the sun and would tend to augment its mass considerably.

Allow me to point out, as regards, in the first place, the second of M. Faye's objections, that the degree of diffusion supposed by me is such as may ensure the permanence of the statical equilibrium between the forces of expansion and diffusion on the one hand, and

the attraction towards the sun and the celestial bodies on the other. If no such equilibrium were established, M. Faye's objection would at once upset my theory. I am, moreover, inclined to admit that if Mariotte's law with regard to the tension of gases could be applied indefinitely, the pressure of the interplanetary gaseous medium would be reduced almost beyond any thing of which we can form an idea; but it seems to me, from considerations drawn from the dynamical theory of gases, and from the manner in which, as demonstrated by Mr. Crookes, gases behave when rarefied to an extraordinary degree in tubes—it seems to me, I say, that at least there exists no reason *à priori* why this law should be extended rigorously to vapours beyond the confines of our atmosphere and of that of the Sun.

As regards M. Faye's first objection, I admit that a density of $\frac{1}{2000}$ atmosphere would have the consequences which he so correctly establishes; and I remember having said (see 'Proceedings of the Royal Society,' p. 395) that assuming as demonstrated the results of my experiments on the dissociation of vapours by the solar energy, and that stellar space is filled with vapour at a pressure *not exceeding* the limit of $\frac{1}{1000}$ atmosphere, which corresponds to the highest rarefaction that I was able to obtain in my experiments, a dissociation of this cosmical vapour must ensue by the radiation of the Sun. It must nevertheless be remarked that this observation only relates to the physical phenomena submitted to my experiments, and that it is evident that if, the dissociation of aqueous vapour and of carbon-compounds is effected by the direct radiation of the sun at so high a pressure as $\frac{1}{1000}$ atmosphere, it would with still more reason be effected in the much more rarefied medium.

In another passage of my memoir (p. 397), when I apply my hypothesis to comets, I assume that, even at their perihelion, they represent a vapour-medium with a density of only $\frac{1}{3000}$ atmosphere, and that this density suffices to give rise to incandescence by compression. This supposition proves, at any rate indirectly, that I regarded stellar space as filled with vapour at a pressure much below $\frac{1}{3000}$ atmosphere, while still speaking of this medium (in the absence of all data of experiment and observation) as in an extremely rarefied state, without fixing any limit of this rarefaction.

Since then new facts of observation have tended to confirm my hypothesis of a stellar space filled with rarefied matter analogous to that which we can actually produce in our vacuum-tubes. The equatorial prolongations of the solar atmosphere observed in America during the eclipse of 1859, seem to demonstrate the existence of matter extending from the Sun for several millions of leagues, and rendered visible, no doubt, by solid particles, illuminated partly by the reflection of the solar light, and partly by discharges of electricity towards the Sun.

My hypothesis has found a still more direct confirmation in the remarkable spectroscopic investigations communicated by Capt. Abney to Section A of the British Association in the month of August last, which demonstrate that carbon-compounds, probably

analogous to ethyl, easy to observe distinctly, exist, and at a low temperature, between the Sun's atmosphere and our own. The observations made in America by Prof. Langley with his bolometer, although made for a totally different purpose, tend to confirm the results obtained by Capt. Abney upon the Riffel. We may also add to these proofs the interesting observation of Prof. Schwedoff (still unpublished, and communicated to me on the same occasion by Prof. Silvanus Thompson), according to which large hailstones of cosmical origin have sometimes fallen upon the earth. This observation, however, needs to be confirmed.

Accepting these observations as founded upon facts, physical considerations are not wanting for the approximate determination of the actual density of the stellar vapour, which, in this case, is only a function of the temperature of space. As Gorschow, on 30th November 1871, observed a temperature of $-63°$ C. in the Arctic regions, it follows that the stellar medium (which, if it consists of a vapour, must be able to intercept calorific rays) must be at a temperature comprised between $-63°$ and the absolute zero ($-273°$): the solar radiation must maintain in it some temperature, or, at least, such a temperature that the dissociation of this medium is very active.

It is to Regnault that we owe our most exact knowledge of the density of vapours at different temperatures; but his researches did not extend below $-32°$ C., and his formulæ cannot be rigorously applied below that point: nevertheless they enable us to estimate approximately what may be the densities of a vapour at lower temperatures; and it is thus that we are led to believe that at $-130°$ the density of aqueous vapour does not exceed $\frac{1}{5,000,000}$ atmosphere. If we assume, further, that the gaseous mass which fills space contains only $\frac{1}{5}$ of aqueous vapour, the other four fifths being composed of hydrocarbons, carbonic acid, and nitrogen, the total pressure of the vapour would not exceed $\frac{1}{1,000,000}$ atmosphere.

These vapours would traverse space with a velocity equal, probably, to half the tangential velocity at the surface of the Sun, or at about 1 kilometre per second. It would be easily demonstrated that a column of these dissociated gases travelling with this velocity towards the polar surfaces of the sun and taken at a distance of 5,500,000 kilom. from the Sun (equal to the mean distance of Mercury, the nearest of his planets) would present a section of flow towards the Sun equal to 140,000 milliards of square kilometres, much more than sufficient to furnish the material necessary to yield by combustion the heat required to maintain the solar radiation.

Perhaps the eminent Director of the Bureau des Longitudes may be inclined to think that a gaseous medium of a density equal at most to $\frac{1}{1,000,000}$ of that of our atmosphere might still interfere with planetary movements to a degree incompatible with the facts ascertained by astronomical observations. If this be the case, it would suffice to assume a still lower temperature for this medium,

and in consequence a more attenuated rarefaction of the interstellar gaseous matter.—*Comptes Rendus de l'Académie des Sciences*, Oct. 30, 1882, p. 769.

ON A PROPERTY OF THE COEFFICIENT OF ABSORPTION.

BY EILHARD WIEDEMANN.

Bunsen represents the absorption-coefficient α at a temperature t by the formula

$$\alpha = a - bt + ct^2,$$

in which a and b = a constant.

Instead of this, we can write

$$\alpha = a\left\{1 - \frac{b}{a}t + \frac{c}{a}t^2\right\}.$$

In the following Table I have set down, together with the values of a, $b \cdot 10^3$, and $c \cdot 10^7$, the values of b/a and c/a for a series of gases and water:—

	a.	b.	c.	$\frac{b}{a}$.	$\frac{c}{a}$.
Hydrogen	0·0193	0	0	0	0
Nitrogen	0·0203	539	112	0·02648	0·000548
Air	0·0247	654	135	0·026487	0·000548
Diethyl	0·0315	1045	251	0·033198	0·000796
Carbonic oxide	0·0329	816	164	0·0243	0·000499
Oxygen	0·0412	1089	226	0·02648	0·000548
Marsh-gas	0·0545	1180	103	0·02166	0·000188
Dimethyl	0·0871	3324	603	0·03816	0·000692
Hydride of ethyl	0·0946	3532	628	0·03735	0·000663
Ethylene	0·2563	9136	1881	0·03564	0·000654
Propylene	0·4465	22075	5388	0·04943	0·000206
Nitrous oxide	1·3052	45362	6483	0·03475	0·000496
Carbonic acid	1·7967	77610	16424	0·04320	0·000914
Sulphuretted hydrogen	1·3706	83687	5213	0·01914	0·000119
Sulphurous acid	79·789	2607700	263490	0·03268	0·000367
Chlorine	3·0361	46196	1107		

It will be remarked that the values of b/a in this Table vary only from 0·02 to 0·05, while a itself ascends from 0·02 to 79·789, *i. e.* up to 4000 times the former value. But the values of b/a indicate how large a fraction of the gas absorbed at 0° escapes on the temperature being raised 1°: and from the above it follows that that fraction varies within very narrow limits for all gases.

In order to further test this proposition, it would be necessary to institute experiments for larger intervals of temperature.

A comparison of the absorptions in alcohol gave similar results. —Wiedemann's *Annalen*, vol. xvii. p. 349 (1882).

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